THE DOVER MEETING OF THE BRITISH ASSOCIATION.

Dover, September 19.

THE meeting of the British Association at Dover, which concludes this week, has been on the whole a great success, especially when the size of the town and the fact that it is the most ambitious effort the town has ever made are considered. The number of members and associates present falls little short of 1400, and amongst these are included an unusually large number of the chief representatives of science. The proceedings at the various Sections have been interesting, though there has been no very startling announcement made at any of them, but very good work has been done. Though there is, perhaps, no longer as great a necessity as formerly for the missionary side of the Association's work, yet its usefulness as a common meeting ground for representatives of science in every branch can never be overrated. The necessity for some such central gathering point as the Association affords becomes the greater as science becomes more and more specialised.

The Presidential Addresses in the various Sections have reached a very high standard of excellence. Prof. Poynting's address in Section A was a masterpiece of exposition; that of Mr. Horace Brown in Section B was remarkable for the light it throws on many of the obscure problems so interesting alike to botanists and chemists, and contained much original work. The Mechanical Science Section had a most interesting address from its President, Sir W. H. White. The Geological Section arranged to have its address on Saturday, September 16, when the French Association paid its visit to Dover. A very large gathering of a cosmopolitan character assembled to hear Prof. Geikie discourse on geological time. The vote of thanks was moved by Lord Lister and seconded by the President of the French Geological Society.

The Presidential Addresses to which reference has just been made were delivered for the most part on Thursday and Friday, arrangements being made so that no two addresses were appointed for the same time. Thus all the Sectional meetings at the time of the Presidential Addresses were well filled.

On Thursday afternoon the first social function took place in the College grounds, where the Chairman of the College Council, headmaster and master entertained over one thousand guests. The band of the Royal Artillery and the Westminster Glee Singers enlivened the proceedings. The balloon ascent did not take place owing to a high N.E. wind prevailing, which would have taken the balloon into the North Sea. On Friday, however, the wind had fallen a little, and a non-scientific balloon ascent took place. The balloon descended later on at Gravelines, and a message of greeting from the Headmaster of Dover College was delivered to the Mayor on behalf of the President of the British Association. On Friday evening a most interesting though short address was delivered by Prof. Richet, of Paris, who proved himself to be an adept both in oratory and in the art of scientific exposition. The vote of thanks was moved by Lord Lister and seconded by Sir W. T. Thiselton-Dyer in most appropriate terms. A smoking concert followed which reflected great credit on its organisers.

The reception of the members of the French Association took place on Saturday. About 280 members arrived at the Admiralty Pier about 9.30, and were received by the President of the British Association and those members who were Correspondents and Associates of the Academy of Sciences. The military were also represented at the landing on the pier. Some disappointment seems to have been felt by the spectators that a larger number of members of the British Association were not present, but this feeling was not experienced by the French visitors, who were delighted with the warmth and cordial nature of their greeting, especially when Sir Michael Foster kissed Dr. Brouardel on both cheeks. Sir Michael Foster in his speech at the luncheon wittily referred to this act as the embracing of the daughter by the mother. Seven tram cars then conveyed the members off to the Town Hall, where the Mayor of Dover, accompanied by the Corporation, officially received the visitors, and various speeches were delivered. The gathering then broke up, and various Sections were visited. At two o'clock some 800 guests sat down to an elaborate luncheon in a marquee near the reception-room. After the lunch speeches were delivered, and toasts of a most cordial nature were proposed. The Presidents of the two Associations, the Mayors of Dover and Boulogne, the Under-Secretary for War (Mr. G. Wyndham), the member for Dover, being amongst the chief speakers. Everything passed off with great cordiality and enthusiasm. After the lunch the whole assembly was photographed in the College grounds. The French visitors then paid a visit to the Castle, where they were shown the chief objects of interest by the Rev. S. P. H. Statham, Senior Chaplain to the Forces, and author of a recently published history of Dover. The visitors were taken back to Boulogne by a special steamer (the *Empress*) at six o'clock. In the evening an interesting military tattoo took place on the sea-front, which was lavishly illuminated for the occasion. On Sunday there were services at most of the Dover churches, and a large number of members of the Association visited Canterbury, where an organ recital was given in the afternoon, in addition to the special services and sermons announced for the occasion. In Dover College Chapel, the Rev. A. H. Stevens gave a very interesting and well-arranged organ recital in the afternoon also.

On Monday there was a garden-party at the Park, which attracted a large gathering of people and was a perfect success in every way. The feature of the evening was the lecture by Prof. Fleming on the centenary of the electric current, which was illustrated by numerous exceedingly interesting experiments. Prof. Fleming for a couple of hours kept his large audience listening in rapt attention to the masterly exposition of his subject. To the general public, perhaps the most interesting part was the demonstration of the Marconi wireless telegraphy, by which messages were exchanged with Dr. Brouardel and with the Goodwin Lightship.

On Wednesday there is to be a visit to Canterbury to meet one hundred members of the French Association. There will be a lunch, which will be attended by about one hundred of the leading members of the British Association, in addition to the French visitors. Previously to the visit to Canterbury, the concluding general meeting will be held, when a vote of thanks to the Mayor and Corporation for their reception of the Association will be moved by Lord Lister, and seconded by Sir Frederick Bramwell. The vote will be acknowledged by the Town Clerk on behalf of the Mayor, and by Mr. W. H. Pendlebury, who is local secretary jointly with Colonel Knocker. A second vote of thanks will be proposed to the Council and Headmaster of Dover College for their kindness in allowing the use of their rooms and the College grounds, which have added so much to the interest of the meeting. Votes of thanks will also be given to those who have offered hospitality to members of the Association, and especially to the naval and military authorities who have in various ways helped to make the meeting a success.

On Thursday, if the weather is propitious, a large number of members, including the chief representatives of the various branches of science, are expected to visit Boulogne. This will conclude the Dover meeting of the British Association, which will be looked back upon with great interest by most of those who have attended it.

The chief members of the French Association who visited Dover were Dr. Brouardel, the president; Dr. Aigre, the Mayor of Boulogne; Dr. Boushard, the ex-president; MM. Dislere, also ex-president; Gariel, secretary; Lôir (nephew of Pasteur), also secretary; M. Bergoine, Professor of Medicine at Bordeaux; M. Namy, Membre de l'Institut; M. Giard, professor at the Sorbonne; Dr. Ferraud; M. Collignon; M. Farjon; M. de Guerne, ex-president; Dr. de Walcourt; Dr. Dufour, of Lausanne, and others. It will be seen that the French visitors were representative men.

W. H. PENDLEBURY.

Work of the General Committee.

The report of the couneil of the Association was read and adopted at the first meeting of the general committee. It announced that after due consideration the council had resolved to recommend the general committee to contribute the sum of 1000*l*. to the National Antarctic Expedition, and that the grant be given out of the accumulated funds of the Association, and not out of the sum allocated to annual grants. The report also stated that the following resolutions, referred to the council by the general committee for consideration and action if desirable, have been considered and acted upon:—

(1) That having regard to the letter of December 15, 1897, from Sir E. Maunde Thompson, the council be requested to take further action with regard to a bureau of ethnology, by renewing the correspondence with the Trustees of the British Museum.

A committee was accordingly appointed for the purpose of conferring with the officers of the British Museum. The President has also been in correspondence with the Marquis of Salisbury regarding this matter, and the council have the pleasure to announce that satisfactory arrangements have been made for the establishment of such a bureau, and that Lord Salisbury has directed that reports prepared by officers in the various Protectorates under the administration of the Foreign Office be forwarded to the British Museum.

(2) That the council be requested to consider the desirability of representing to the Colonial Government that the early establishment of a magnetic observatory at the Cape of Good Hope would be of the highest utility to the science of terrestrial magnetism, especially in view of the Antarctic expeditions which are about to leave Europe, and that the observatory should be established at such a distance from electric railways and tramways as to avoid all possibility of disturbance from them.

The question having been considered, the council requested the President to make the necessary representation to the Colonial Government. The council have received a minute of the Government of Cape Colony, through the High Commissioner, stating that while Ministers have much sympathy with the suggestion to establish a magnetic observatory, and do not overlook the scientific and practical aspects of the project, they do not regard as practicable the immediate provision by the colony of funds for the carrying out of the scheme.

(3) That the council be requested to consider the advisability of urging Her Majesty's Government to place at the disposal of the Seismological Committee of the British Association a suitable building for the housing of apparatus for continuous seismological observations.

A committee which was appointed to report on this resolution stated that in their opinion it is desirable that a central station should be established, and recommended the council to request the Government to place a suitable building at the disposal of the Seismological Committee which could be used as a station for carrying on observations and would serve as a centre for the stations (now twenty-three in number) in various parts of the world which, at the request of the committee, have been supplied with seismographic apparatus of the pattern they have recommended.

The council decided to reappoint the committee for the purpose of reporting further on the best situation for the proposed central seismological station and on the cost of its maintenance.

(4) That the council be requested to urge strongly on the Indian Government the desirability, in the interests both of administration and of science, to promote an inquiry, under the direction of skilled anthropologists, into the physical and mental characteristics of the various races throughout the Empire, including their institutions, customs and traditions, and a carefully organised photographic survey.

A committee which was appointed to consider this question reported that in their opinion the resolution in its present form is of too comprehensive and costly a character to justify the council in submitting it to the Indian Government.

(5) That the council be recommended to issue the collected reports on the North-Western tribes of Canada in a single volume at a moderate price, reprinting so many of the reports as may be necessary.

The council resolved that the reports be not reprinted.

(6) That the council be requested to bring under the notice of the Admiralty the importance of securing systematic observations upon the erosion of the sea coast of the United Kingdom, and that the co-operation of the coastguard might be profitably secured for this purpose.

A committee having been appointed to report on the above resolution, recommended that the council inquire whether the Admiralty would be willing to arrange that observations of a simple character on changes in the sea coast be recorded and reported by the coastguards. The committee pointed out that if the Admiralty consented to carry out this proposal it would be necessary to appoint a committee for the purpose of drawing up a scheme of instruction for the observers, making arrangements for starting the work, and subsequently examining from time to time such localities as may seem to require special attention. This recommendation having been adopted by the council, the president was requested to approach the Admiralty upon the subject, and in response a reply was received from the Admiralty stating that my Lords saw no objection to this proposal, as the required observations could be made by the men in the ordinary course of their duty.

At the second meeting of the general committee invitations for the meeting of the Association in 1902 were received, and the officers were appointed for next year's meeting at Bradford, to commence on Wednesday, September 5. The meeting will be held at Glasgow in 1901. Representatives of the cities of Belfast and Cork invited the Association to meet at one of these places in 1902; but the president explained that no definite answer could yet be given to the invitations.

Upon the proposal of Lord Lister, seconded by Sir Archibald Geikie, Sir William Turner, F.R.S., was appointed President-elect for the meeting at Bradford in 1900.

Sir J. Evans proposed that the following persons be asked to serve as vice-presidents at the Bradford meeting:—The Earl of Scarborough (Lord Lieutenant of the West Riding), the Duke of Devonshire, the Marquis of Ripon, the Bishop of Ripon, Lord Masham, the Mayor of Bradford, the Hon. H. E. Butler, Sir A. Binnie, Prof. Rücker, and Prof. Thorpe.

Sir Norman Lockyer seconded the resolution, which was carried unanimously.

The general secretaries (Sir W. Roberts-Austen and Prof. Schäfer), the assistant general secretary (Mr. Griffith), and the general treasurer (Prof. Carey Foster) were re-elected.

The following is a synopsis of the grants of money made for scientific purposes by the general committee, at the meeting just concluded :—

Mathematics.

	£
*Rayleigh, Lord—Electrical Standards (£300 in hand) ...	25
*Judd, Prof. J. W.—Seismological Observations (£9 5*s.* 4*d.* in hand)	60
*FitzGerald, Prof. G. F.—Radiation in a Magnetic Field	25
*Rücker, Prof. A. W.—Magnetic Force on board Ship ..	10
*Callendar, Prof. H. L.—Meteorological Observatory, Montreal	20
*Kelvin, Lord—Tables of Mathematical Functions ...	75

Chemistry.

*Hartley, Prof. W. N.—Relation between Absorption Spectra and Constitution of Organic Bodies	30
*Roscoe, Sir H. E.—Wave-length Tables	5
*Reynolds, Prof. J. E. — Electrolytic Quantitative Analysis	5
Miers, Prof. H. A.—Isomorphous Sulphonic Derivatives of Benzene	20
Neville, Mr. F. H.—The Nature of Alloys	30

Geology.

*Hull, Prof. E.—Erratic Blocks (£6 in hand)	
*Geikie, Prof. J.—Photographs of Geological Interest ...	10
*Dawkins, Prof. W. B.—Remains of Elk in the Isle of Man	5
*Dawson, Sir J. W.—Pleistocene Fauna and Flora in Canada	10
*Lloyd-Morgan, Prof. C.—Ossiferous Caves at Uphill (£8 in hand)	10
Watts, Prof. W. W.—Movements of Underground Waters of Craven...	40
Scharff, Dr.—Exploration of Irish Caves...	20

Zoology.

*Herdman, Prof. W. A.—Table at the Zoological Station, Naples	100
*Bourne, Mr. G. C.—Table at the Biological Laboratory, Plymouth	20
*Woodward, Dr. H.—Index Generum et Specierum Animalium...	50
*Newton, Prof.—Migration of Birds	15
Lankester, Prof. E. Ray.—Plankton and Physical Conditions of the English Channel	40
*Newton, Prof.—Zoology of the Sandwich Islands ...	100
Sedgwick, Mr. A.—Coral Reefs of the Indian Region...	30

Geography.

Murray, Sir John—Physical and Chemical Constants of Sea Water	100

Economic Science and Statistics.

Price, Mr. L. L.—Future Dealings in Raw Produce ...	5
Sedgwick, Prof. H.—State Monopolies in other Countries (£13 13s. 6d. in hand)	

Mechanical Science.

*Preece, Sir W. H.—Small Screw Gauge (£17 1*s.* 2*d.* in hand)	

Anthropology.

*Evans, Mr. A. J.—Silchester Excavation	10
*Penhallow, Prof. D. P.—Ethnological Survey of Canada	50
*Tylor, Prof. E. B.—New Edition of "Anthropological Notes and Queries"	40
*Garson, Dr. J. G.—Age of Stone Circles (balance in hand)	
*Read, Mr. C. H.—Photographs of Anthropological Interest	10
*Brabrook, Mr. E. W.—Mental and Physical Condition of Children	5
Read, Mr. C. H.—Ethnography of the Malay Peninsula	25

* Re-appointed.

Physiology.

*Schäfer, Prof. E. A.—Physiological Effects of Peptone...	20
Schäfer, Prof. E. A.—Comparative Histology of Suprarenal Capsules	20
*Gotch, Prof. F.—Comparative Histology of Cerebral Cortex	5
Gotch, Prof. F.—Electrical Changes in Mammalian Nerves	20
Starling, Dr.—Vascular Supply of Secreting Glands ...	10

Botany.

*Darwin, Mr. F.—Assimilation in Plants (£6 6*s.* 8*d.* in hand)	
*Farmer, Prof. J. B.—Fertilisation in Phæophyceæ ...	20

Corresponding Societies.

*Meldola, Prof. R.—Preparation of Report...	20
	£1115

SECTION C.

GEOLOGY.

OPENING ADDRESS BY SIR ARCHIBALD GEIKIE, D.C.L., D.SC., F.R.S., PRESIDENT OF THE SECTION.

AMONG the many questions of great theoretical importance which have engaged the attention of geologists, none has in late years awakened more interest or aroused livelier controversy than that which deals with time as an element in geological history. The various schools which have successively arisen—Cataclysmal, Uniformitarian, and Evolutionist—have had each its own views as to the duration of their chronology, as well as to the operations of terrestrial energy. But though holding different opinions, they did not make these differences matter of special controversy among themselves. About thirty years ago, however, they were startled by a bold irruption into their camp from the side of physics. They were then called on to reform their ways, which were declared to be flatly opposed to the teachings of natural philosophy. Since that period the discussion then started regarding the age of the earth and the value of geological time has continued with varying animation. Evidence of the most multifarious kind has been brought forward, and arguments of widely different degrees of validity have been pressed into service both by geologists and palæontologists on one side, and by physicists on the other. For the last year or two there has been a pause in the controversy, though no general agreement has been arrived at in regard to the matters in dispute. The present interval of comparative quietude seems favourable for a dispassionate review of the debate. I propose, therefore, to take, as perhaps a not inappropriate subject on which to address geologists upon a somewhat international occasion like this present meeting of the British Association at Dover, the question of Geological Time. In offering a brief history of the discussion, I gladly avail myself of the opportunity of enforcing one of the lessons which the discussion has impressed upon my own mind, and to point a moral which, as it seems to me, we geologists may take home to ourselves from a consideration of the whole question. There is, I think, a practical outcome which may be made to issue from the controversy in a combination of sympathy and co-operation among geologists all over the world. A lasting service will be rendered to our science if by well-concerted effort we can place geological dynamics and geological chronology on a broader and firmer basis of actual experiment and measurement than has yet been laid.

To understand aright the origin and progress of the dispute regarding the value of time in geological speculation, we must take note of the attitude maintained towards this subject by some of the early fathers of the science. Among these pioneers none has left his mark more deeply graven on the foundations of modern geology than James Hutton. To him, more than to any other writer of his day, do we owe the doctrine of the high antiquity of our globe. No one before him had ever seen so clearly the abundant and impressive proofs of this remote antiquity recorded in the rocks of the earth's crust. In these rocks he traced the operation of the same slow and quiet processes which he observed to be at work at present in

* Re-appointed.

gradually transforming the face of the existing continents. When he stood face to face with the proofs of decay among the mountains, there seems to have arisen uppermost in his mind the thought of the immense succession of ages which these proofs revealed to him. His observant eye enabled him to see "the operations of the surface wasting the solid body of the globe, and to read the unmeasurable course of time that must have flowed during those amazing operations, which the vulgar do not see, and which the learned seem to see without wonder" ("Theory of the Earth," vol. i. p. 108). In contemplating the stupendous results achieved by such apparently feeble forces, Hutton felt that one great objection he had to contend with in the reception of his theory, even by the scientific men of his day, lay in the inability or unwillingness of the human mind to admit such large demands as he made on the past. "What more can we require?" he asks in summing up his conclusions; and he answers the question in these memorable words: "What more can we require? Nothing but time. It is not any part of the process that will be disputed; but after allowing all the parts, the whole will be denied; and for what?—only because we are not disposed to allow that quantity of time which the ablution of so much wasted mountain might require" (*op. cit.* vol. ii. p. 329).

Far as Hutton could follow the succession of events registered in the rocky crust of the globe, he found himself baffled by the closing in around him of that dark abysm of time into which neither eye nor imagination seemed able to penetrate. He well knew that, behind and beyond the ages recorded in the oldest of the primitive rocks, there must have stretched a vast earlier time, of which no record met his view. He did not attempt to speculate beyond the limits of his evidence. "I do not pretend," he said, "to describe the beginning of things; I take things such as I find them at present, and from these I reason with regard to that which must have been (*op. cit.* vol. i. p. 173, *note*). In vain could he look, even among the oldest formations, for any sign of the infancy of the planet. He could only detect a repeated series of similar revolutions, the oldest of which was assuredly not the first in the terrestrial history, and he concluded, as "the result of this physical inquiry, that we find no vestige of a beginning, no prospect of an end" (*op. cit.* vol. i. p. 200).

This conclusion from strictly geological evidence has been impugned from the side of physics, and, as further developed by Playfair, has been declared to be contradicted by the principles of natural philosophy. But if it be considered on the basis of the evidence on which it was originally propounded, it was absolutely true in Hutton's time and remains true to-day. That able reasoner never claimed that the earth has existed from all eternity, or that it will go on existing for ever. He admitted that it must have had a beginning, but he had been unable to find any vestige of that beginning in the structure of the planet itself. And notwithstanding all the multiplied researches of the century that has passed since the immortal "Theory of the Earth" was published, no relic of the first condition of our earth has been found. We have speculated much, indeed, on the subject, and our friends the physicists have speculated still more. Some of the speculations do not seem to me more philosophical than many of those of the older cosmogonists. As far as trustworthy evidence can be drawn from the rocks of the globe itself, we do not seem to be nearer the discovery of the beginning than Hutton was. The most ancient rocks that can be reached are demonstrably not the first-formed of all. They were preceded by others which we know must have existed, though no vestige of them may remain.

It may be further asserted that, while it was Hutton who first impressed on modern geology the conviction that for the adequate comprehension of the past history of the earth vast periods of time must be admitted to have elapsed, our debt of obligation to him is increased by the genius with which he linked the passage of these vast periods with the present economy of nature. He first realised the influence of time as a factor in geological dynamics, and first taught the efficacy of the quiet and unobtrusive forces of nature. His predecessors and contemporaries were never tired of invoking the more vigorous manifestations of terrestrial energy. They saw in the composition of the land and in the structure of mountains and valleys memorials of numberless convulsions and cataclysms. In Hutton's philosophy, however, "it is the little causes, long continued, which are considered as bringing about the greatest changes of the earth" ("Theory of the Earth," vol. ii. p. 205).

And yet, unlike many of those who derived their inspiration from his teaching, but pushed his tenets to extremes which he doubtless never anticipated, he did not look upon time as a kind of scientific fetich, the invocation of which would endow with efficacy even the most trifling phenomena. As if he had foreseen the use that might be made of his doctrine, he uttered this remarkable warning: "With regard to the effect of time, though the continuance of time may do much in those operations which are extremely slow, where no change, to our observation, had appeared to take place, yet, where it is not in the nature of things to produce the change in question, the unlimited course of time would be no more effectual than the moment by which we measure events in our observations" (*op. cit.* vol. i. p. 44).

We thus see that in the philosophy of Hutton, out of which so much of modern geology has been developed, the vastness of the antiquity of the globe was deduced from the structure of the terrestrial crust and the slow rate of action of the forces by which the surface of the crust is observed to be modified. But no attempt was made by him to measure that antiquity by any of the chronological standards of human contrivance. He was content to realise for himself and to impress upon others that the history of the earth could not be understood, save by the admission that it occupied prolonged though indeterminate ages in its accomplishment. And assuredly no part of his teaching has been more amply sustained by the subsequent progress of research.

Playfair, from whose admirable "Illustrations of the Huttonian Theory" most geologists have derived all that they know directly of that theory, went a little further than his friend and master in dealing with the age of the earth. Not restricting himself, as Hutton did, to the testimony of the rocks, which showed neither vestige of a beginning nor prospect of an end, he called in the evidence of the cosmos outside the limits of our planet, and declared that in the firmament also no mark could be discovered of the commencement or termination of the present order, no symptom of infancy or old age, nor any sign by which the future or past duration of the universe might be estimated ("Illustrations of the Huttonian Theory, § 118). He thus advanced beyond the strictly geological basis of reasoning, and committed himself to statements which, like some made also by Hutton, seem to have been suggested by certain deductions of the French mathematicians of his day regarding the stability of the planetary motions. His statements have been disproved by modern physics; distinct evidence, both from the earth and the cosmos, has been brought forward of progress from a beginning which can be conceived, through successive stages to an end which can be foreseen. But the disproof leaves Hutton's doctrine about the vastness of geological time exactly where it was. Surely it was no abuse of language to speak of periods as being vast, which can only be expressed in millions of years.

It is easy to understand how the Uniformitarian school, which sprang from the teaching of Hutton and Playfair, came to believe that the whole of eternity was at the disposal of geologists. In popular estimation, as the ancient science of astronomy was that of infinite distance, so the modern study of geology was the science of infinite time. It must be frankly conceded that geologists, believing themselves unfettered by any limits to their chronology, made ample use of their imagined liberty. Many of them, following the lead of Lyell, to whose writings in other respects modern geology owes so deep a debt of gratitude, became utterly reckless in their demands for time, demands which even the requirements of their own science, if they had adequately realised them, did not warrant. The older geologists had not attempted to express their vast periods in terms of years. The indefiniteness of their language fitly denoted the absence of any ascertainable limits to the successive ages with which they had to deal. And until some evidence should be discovered whereby these limits might be fixed and measured by human standards, no reproach could justly be brought against the geological terminology. It was far more philosophical to be content, in the meanwhile, with indeterminate expressions, than from data of the weakest or most speculative kind to attempt to measure geological periods by a chronology of years or centuries.

In the year 1862 a wholly new light was thrown on the question of the age of our globe and the duration of geological time by the remarkable paper on the Secular Cooling of the Earth communicated by Lord Kelvin (then Sir William Thomson) to the Royal Society of Edinburgh (*Trans. Roy. Soc. Edin.*, vol. xxiii., 1862). In this memoir he first developed his now well-known argument from the observed rate of increase of temperature downwards from the surface of the land. He astonished geologists by announcing to them that some definite limits to the age of our planet might be ascertained, and by declaring his belief that this age must be more than 20 millions, but less than 400 millions, of years.

Nearly four years later he emphasised his dissent from what he considered to be the current geological opinions of the day by repeating the same argument in a more pointedly antagonistic form in a paper of only a few sentences, entitled, "The Doctrine of Uniformity in Geology briefly refuted" (*Proc. Roy. Soc. Edin.*, vol. v. p. 512, December 18, 1865).

Again, after a further lapse of about two years, when, as President of the Geological Society of Glasgow, it became his duty to give an address, he returned to the same topic and arraigned more boldly and explicitly than ever the geology of the time. He then declared that "a great reform in geological speculation seems now to have become necessary," and he went so far as to affirm that "it is quite certain that a great mistake has been made—that British popular geology at the present time is in direct opposition to the principles of natural philosophy" (*Trans. Geol. Soc. Glasgow*, vol. iii., February 1868, pp. 1, 16). In pressing once more the original argument derived from the downward increase of terrestrial temperature, he now reinforced it by two further arguments, the one based on the retardation of the earth's angular velocity by tidal friction, the other on the limitation of the age of the sun.

These three lines of attack remain still those along which the assault from physics is delivered against the strongholds of geology. Lord Kelvin has repeatedly returned to the charge since 1868, his latest contribution to the controversy having been pronounced two years ago.[1] While his physical arguments remain the same, the limits of time which he deduces from them have been successively diminished. The original maximum of 400 millions of years has now been restricted by him to not much more than 20 millions, while Prof. Tait grudgingly allows something less than 10 millions ("Recent Advances in Physical Science," p. 174).

Soon after the appearance of Lord Kelvin's indictment of modern geology in 1868, the defence of the science was taken up by Huxley, who happened at the time to be President of the Geological Society of London. In his own inimitably brilliant way, half seriously, half playfully, this doughty combatant, with evident relish, tossed the physical arguments to and fro in the eyes of his geological brethren, as a barrister may flourish his brief before a sympathetic jury. He was willing to admit that "the rapidity of rotation of the earth *may* be diminishing, that the sun *may* be waxing dim, or that the earth itself *may* be cooling." But he went on to add his suspicion that "most of us are Gallios, 'who care for none of these things,' being of opinion that, true or fictitious, they have made no practical difference to the earth, during the period of which a record is preserved in stratified deposits" (Presidential Address, *Quart. Journ. Geol. Soc.*, 1869).

For the indifference which their advocate thus professed on their behalf most geologists believed that they had ample justification. The limits within which the physicist would circumscribe the earth's history were so vague, yet so vast, that whether the time allowed were 400 millions or 100 millions of years did not seem to them greatly to matter. After all, it was not the time that chiefly interested them, but the grand succession of events which the time had witnessed. That succession had been established on observations so abundant and so precise that it could withstand attack from any quarter, and it had taken as firm and lasting a place among the solid achievements of science as could be claimed for any physical speculations whatsoever. Whether the time required for the transaction of this marvellous earth-history was some millions of years more or some millions of years less did not seem to the geologists to be a question on which their science stood in antagonism with the principles of natural philosophy, but one which the natural philosophers might be left to settle at their own good pleasure.

For myself, I may be permitted here to say that I have never shared this feeling of indifference and unconcern. As far back as the year 1868, only a month after Lord Kelvin's first presentation of his threefold argument in favour of limiting the age of the earth, I gave in my adhesion to the propriety of restricting

[1] "The Age of the Earth," being the Annual Address to the Victoria Institute, June 2, 1897. *Phil. Mag.*, January 1899, p. 66.

the geological demands for time. I then showed that even the phenomena of denudation, which, from the time of Hutton downwards, had been most constantly and confidently appealed to in support of the inconceivably vast antiquity of our globe, might be accounted for, at the present rate of action, within such a period as 100 millions of years.[1] To my mind it has always seemed that whatever tends to give more precision to the chronology of the geologist, and helps him to a clearer conception of the antiquity with which he has to deal, ought to be welcomed by him as a valuable assistance in his inquiries. And I feel sure that this view of the matter has now become general among those engaged in geological research. Frank recognition is made of the influence which Lord Kelvin's persistent attacks have had upon our science. Geologists have been led by his criticisms to revise their chronology. They gratefully acknowledge that to him they owe the introduction of important new lines of investigation, which link the solution of the problems of geology with those of physics. They realise how much he has done to dissipate the former vague conceptions as to the duration of geological history, and even when they emphatically dissent from the greatly restricted bounds within which he would now limit that history, and when they declare their inability to perceive that any reform of their speculations in this subject is needful, or that their science has placed herself in opposition to the principles of physics, they none the less pay their sincere homage to one who has thrown over geology, as over so many other departments of natural knowledge, the clear light of a penetrating and original genius.

When Lord Kelvin first developed his strictures on modern geology he expressed his opposition in the most uncompromising language. In the short paper to which reference has already been made he announced, without hesitation or palliation, that he "briefly refuted" the doctrine of Uniformitarianism which had been espoused and illustrated by Lyell and a long list of the ablest geologists of the day. The severity of his judgment of British geology was not more marked than was his unqualified reliance on his own methods and results. This confident assurance of a distinguished physicist, together with a formidable array of mathematical formulæ, produced its effect on some geologists and palæontologists who were not Gallios. Thus, even after Huxley's brilliant defence, Darwin could not conceal the deep impression which Lord Kelvin's arguments had made on his mind. In one letter he wrote that the proposed limitation of geological time was one of his "sorest troubles." In another, he pronounced the physicist himself to be "an odious spectre" (Darwin's "Life and Letters," vol. iii. pp. 115, 146).

The same self-confidence of assertion on the part of some, at least, of the disputants on the physical side has continued all through the controversy. Yet when we examine the three great physical arguments in themselves, we find them to rest on assumptions which, though certified as "probable" or "very sure," are nevertheless admittedly assumptions. The conclusions to which these assumptions lead must depend for their validity on the degree of approximation to the truth in the premisses which are postulated.

Now it is interesting to observe that neither the assumptions nor the conclusions drawn from them have commanded universal assent even among physicists themselves. If they were as self-evident as they have been claimed to be, they should at least receive the loyal support of all those whose function it is to pursue and extend the applications of physics. It will be remembered, however, that thirteen years ago Prof. George Darwin, who has so often shown his inherited sympathy in geological investigation, devoted his presidential address before the Mathematical Section of this Association to a review of the three famous physical arguments respecting the age of the earth. He summed up his judgment of them in the following words: "In considering these three arguments I have adduced some reasons against the validity of the first (tidal friction); and have endeavoured to show that there are elements of uncertainty surrounding the second (secular cooling of the earth); nevertheless they undoubtedly constitute a contribution of the first importance to physical geology. Whilst, then, we may protest against the precision with which Prof. Tait seeks to deduce results from them, we are fully justified in following Sir William Thomson, who says that "the existing state of things on the earth, life on the earth—all geological history showing con-

[1] *Trans. Geol. Soc. Glasgow*, vol. iii. (March 26, 1868), p. 189. Sir W. Thomson acknowledged my adhesion in his reply to Huxley's criticism. *Op. cit.* p. 221.

tinuity of life—must be limited within some such period of past time as 100,000,000 years" (*Rep. Brit. Assoc.*, 1886, p. 517).

More recently Prof. Perry has entered the lists, from the physical side, to challenge the validity of the conclusions so confidently put forward in limitation of the age of the earth. He has boldly impugned each of the three physical arguments. That which is based on tidal retardation, following Mr. Maxwell Close and Prof. Darwin, he dismisses as fallacious. In regard to the argument from the secular cooling of the earth, he contends that it is perfectly allowable to assume a much higher conductivity for the interior of the globe, and that this assumption would vastly increase our estimate of the age of the planet. As to the conclusions drawn from the history of the sun, he maintains that, on the one hand, the sun may have been repeatedly fed by infalling meteorites, and that, on the other, the earth, during former ages, may have had its heat retained by a dense atmospheric envelope. He thinks that "almost anything is possible as to the present internal state of the earth," and he concludes in these words: "To sum up, we can find no published record of any lower maximum age of life on the earth, as calculated by physicists, than 400 millions of years. From the three physical arguments, Lord Kelvin's higher limits are 1000, 400, and 500 million years. I have shown that we have reasons for believing that the age, from all these, may be very considerably under-estimated. It is to be observed that if we exclude everything but the arguments from mere physics, the *probable* age of life on the earth is much less than any of the above estimates; but if the palæontologists have good reasons for demanding much greater times, I see nothing from the physicist's point of view which denies them four times the greatest of these estimates" (NATURE, vol. li. p. 585, April 18, 1895).

This remarkable admission from a recognised authority on the physical side re-echoes and emphasises the warning pronounced by Prof. Darwin in the address already quoted—"at present our knowledge of a definite limit to geological time has so little precision that we should do wrong to summarily reject any theories which appear to demand longer periods of time than those which now appear allowable" (*Rep. Brit. Assoc.*, 1886, p. 518).

This "wrong," which Prof. Darwin so seriously deprecated, has been committed, not once, but again and again in the history of this discussion. Lord Kelvin has never taken any notice of the strong body of evidence adduced by geologists and palæontologists in favour of a much longer antiquity than he is now disposed to allow for the age of the earth. His own three physical arguments have been successively re-stated, with such corrections and modifications as he has found to be necessary, and no doubt further alterations are in store for them. He has cut off slice after slice from the allowance of time which at first he was prepared to grant for the evolution of geological history, his latest pronouncement being that "it was more than twenty and less than forty million years, and probably much nearer twenty than forty."[1] But in none of his papers is there an admission that geology and palæontology, though they have again and again raised their voices in protest, have anything to say in the matter that is worthy of consideration.

It is difficult satisfactorily to carry on a discussion in which your opponent entirely ignores your arguments, while you have given the fullest attention to his. In the present instance, geologists have most carefully listened to all that has been brought forward from the physical side. Impressed by the force of the physical reasoning, they no longer believe that they can make any demands they may please on past time. They have been willing to accept Lord Kelvin's original estimate of 100 millions of years as the period within which the history of life upon the planet must be comprised, while some of them have even sought in various ways to reduce that sum nearer to his lower limit. Yet there is undoubtedly a prevalent misgiving, whether in thus seeking to reconcile their requirements with the demands of the physicist they are not tying themselves down within limits of time which on any theory of evolution would have been insufficient for the development of the animal and vegetable kingdoms.

It is unnecessary to recapitulate before this Section of the British Association, even in briefest outline, the reasoning of geologists and palæontologists which leads them to conclude that the history recorded in the crust of the earth must have required for its transaction a much vaster period of time than that to which the physicists would now restrict it.[1] Let me merely remark that the reasoning is essentially based on observations of the present rate of geological and biological changes upon the earth's surface. It is not, of course, maintained that this rate has never varied in the past. But it is the only rate with which we are familiar, which we can watch and in some degree measure, and which, therefore, we can take as a guide towards the comprehension and interpretation of the past history of our planet.

It may be, and has often been, said that the present scale of geological and biological processes cannot be accepted as a trustworthy measure for the past. Starting from the postulate, which no one will dispute, that the total sum of terrestrial energy was once greater than it is now and has been steadily declining, the physicists have boldly asserted that all kinds of geological action must have been more vigorous and rapid during bygone ages than they are to-day; that volcanoes were more gigantic, earthquakes more frequent and destructive, mountain-upthrows more stupendous, tides and waves more powerful, and commotions of the atmosphere more violent, with more ruinous tempests and heavier rainfall. Assertions of this kind are temptingly plausible and are easily made. But it is not enough that they should be made; they ought to be supported by some kind of evidence to show that they are founded on actual fact and not on mere theoretical possibility. Such evidence, if it existed, could surely be produced. The chronicle of the earth's history, from a very early period down to the present time, has been legibly written within the sedimentary formations of the terrestrial crust. Let the appeal be made to that register. Does it lend any support to the affirmation that the geological processes are now feebler and slower than they used to be? If it does, the physicists, we might suppose, would gladly bring forward its evidence as irrefragable confirmation of the soundness of their contention. But the geologists have found no such confirmation. On the contrary, they have been unable to discover any indication that the rate of geological causation has ever, on the whole, greatly varied during the time which has elapsed since the deposition of the oldest stratified rocks. They do not assert that there has been no variation, that there have been no periods of greater activity, both hypogene and epigene. But they maintain that the demonstration of the existence of such periods has yet to be made. They most confidently affirm that whatever may have happened in the earliest ages, in the whole vast succession of sedimentary strata nothing has yet been detected which necessarily demands that more violent and rapid action which the physicists suppose to have been the order of nature during the past.

So far as the potent effects of prolonged denudation permit us to judge, the latest mountain-upheavals were at least as stupendous as any of older date whereof the basal relics can yet be detected. They seem, indeed, to have been still more gigantic than those. It may be doubted, for example, whether among the vestiges that remain of Mesozoic or Palæozoic mountain-chains any instance can be found so colossal as those of Tertiary times, such as the Alps. No volcanic eruptions of the older geological periods can compare in extent or volume with those of Tertiary and recent date. The plication and dislocation of the terrestrial crust are proportionately as conspicuously displayed among the younger as among the older formations, though the latter, from their greater antiquity, have suffered during a longer time from the renewed disturbances of successive periods.

As regards evidence of greater violence in the surrounding envelopes of atmosphere and ocean, we seek for it in vain among the stratified rocks. Among the very oldest formations of these islands, the Torridon sandstone of North-west Scotland presents us with a picture of long-continued sedimentation, such as may be seen in progress now round the shores of many a mountain-girdled lake. In that venerable deposit, the enclosed pebbles are not mere angular blocks and chips, swept by a sudden flood or destructive tide from off the surface of the land, and huddled together in confused heaps over the floor of the sea. They have been rounded and polished by the quiet operation of running water, as stones are rounded and polished

[1] "The Age of the Earth,' Presidential Address to the Victoria Institute for 1897, p. 10; also in *Phil. Mag.*, January 1899.

[1] The geological arguments are briefly given in my Presidential Address to the British Association at the Edinburgh Meeting of 1892. The biological arguments were well stated, and in some detail, by Prof. Poulton, in his Address to the Zoological Section of the Association at the Liverpool Meeting of 1896.

now in the channels of brooks or on the shores of lake and sea. They have been laid gently down above each other, layer over layer, with fine sand sifted in between them, and this deposition has taken place along shores which, though the waters that washed them have long since disappeared, can still be followed for mile after mile across the mountains and glens of the North-west Highlands. So tranquil were these waters that their gentle currents and oscillations sufficed to ripple the sandy floor, to arrange the sediment in laminæ of current-bedding, and to separate the grains of sand according to their relative densities. We may even now trace the results of these operations in thin darker layers and streaks of magnetic iron, zircon, and other heavy minerals, which have been sorted out from the lighter quartz-grains, as layers of iron-sand may be seen sifted together by the tide along the upper margins of many of our sandy beaches at the present day.

In the same ancient formation there occur also various intercalations of fine muddy sediment, so regular in their thin alternations, and so like those of younger formations, that we cannot but hope and expect that they may eventually yield remains of organisms which, if found, would be the earliest traces of life in Europe.

It is thus abundantly manifest that even in the most ancient of the sedimentary registers of the earth's history, not only is there no evidence of colossal floods, tides and denudation, but there is incontrovertible proof of continuous orderly deposition, such as may be witnessed to-day in any quarter of the globe. The same tale, with endless additional details, is told all through the stratified formations down to those which are in the course of accumulation at the present day.

Not less important than the stratigraphical is the palæontological evidence in favour of the general quietude of the geological processes in the past. The conclusions drawn from the nature and arrangement of the sediments are corroborated and much extended by the structure and manner of entombment of the enclosed organic remains. From the time of the very earliest fossiliferous formations there is nothing to show that either plants or animals have had to contend with physical conditions of environment different, on the whole, from those in which their successors now live. The oldest trees, so far as regards their outer form and internal structure, betoken an atmosphere neither more tempestuous nor obviously more impure than that of to-day. The earliest corals, sponges, crustaceans, molluscs, and arachnids were not more stoutly constructed than those of later times, and are found grouped together among the rocks as they lived and died, with no apparent indication that any violent commotion of the elements tried their strength when living, or swept away their remains when dead.

But, undoubtedly, most impressive of all the palæontological data is the testimony borne by the grand succession of organic remains among the stratified rocks as to the vast duration of time required for their evolution. Prof. Poulton has treated this branch of the subject with great fulness and ability. We do not know the present average rates of organic variation, but all the available evidence goes to indicate their extreme slowness. They may conceivably have been more rapid in the past, or they may have been liable to fluctuations according to vicissitudes of environment.[1] But those who assert that the rate of biological evolution ever differed materially from what it may now be inferred to be, ought surely to bring forward something more than mere assertion in their support. In the meantime, the most philosophical course is undoubtedly followed by those biologists who in this matter rest their belief on their own experience among recent and fossil organisms.

So cogent do these geological and palæontological arguments appear, to those at least who have taken the trouble to master them, that they are worthy of being employed, not in defence merely, but in attack. It seems to me that they may be used with effect in assailing the stronghold of speculation and assumption in which our physical friends have ensconced themselves and from which, with their feet, as they believe, planted well within theinterior of the globe and their heads in the heart of the sun, they view with complete unconcern the efforts made by those who endeavour to gather the truth from the surface and crust of the earth. That portion of the records of terrestrial history which lies open to our investigation has been diligently studied in all parts of the world. A vast body of facts has been gathered together from this extended and combined research. The chronicle registered in the earth's crust, though not complete, is legible and consistent. From the latest to the earliest of its chapters the story is capable of clear and harmonious interpretation by a comparison of its pages with the present condition of things. We know infinitely more of the history of this earth than we do of the history of the sun. Are we then to be told that this knowledge, so patiently accumulated from innumerable observations and so laboriously coordinated and classified, is to be held of none account in comparison with the conclusions of physical science in regard to the history of the central luminary of our system? These conclusions are founded on assumptions which may or may not correspond with the truth. They have already undergone revision, and they may be still further modified as our slender knowledge of the sun, and of the details of its history, is increased by future investigation. In the meantime, we decline to accept them as a final pronouncement of science on the subject. We place over against them the evidence of geology and palæontology, and affirm that unless the deductions we draw from that evidence can be disproved, we are entitled to maintain them as entirely borne out by the testimony of the rocks.

[1] See an interesting and suggestive paper by Prof. Le Conte on "Critical Periods in the History of the Earth," *Bull. Dept. Geology, University of California*, vol. i. (1895), p. 313; also one by Prof. Chamberlin on "The Ulterior Basis of Time-divisions and the Classification of Geological History," *Journal of Geology*, vol. vi. (1898), p. 449.

Until, therefore, it can be shown that geologists and palæontologists have misinterpreted their records, they are surely well within their logical rights in claiming as much time for the history of this earth as the vast body of evidence accumulated by them demands. So far as I have been able to form an opinion, one hundred millions of years would suffice for that portion of the history which is registered in the stratified rocks of the crust. But if the palæontologists find such a period too narrow for their requirements, I can see no reason on the geological side why they should not be at liberty to enlarge it as far as they may find to be needful for the evolution of organised existence on the globe. As I have already remarked, it is not the length of time which interests us so much as the determination of the relative chronology of the events which were transacted within that time. As to the general succession of these events, there can be no dispute. We have traced its stages from the bottom of the oldest rocks up to the surface of the present continents and the floor of the present seas. We know that these stages have followed each other in orderly advance, and that geological time, whatever limits may be assigned to it, has sufficed for the passage of the long stately procession.

We may, therefore, well leave the dispute about the age of the earth to the decision of the future. In so doing, however, I should be glad if we could carry away from it something of greater service to science than the consciousness of having striven our best in a barren controversy, wherein concession has all to be on one side and the selection of arguments entirely on the other. During these years of prolonged debate I have often been painfully conscious that in this subject, as in so many others throughout the geological domain, the want of accurate numerical data is a serious hindrance to the progress of our science. Heartily do I acknowledge that much has been done in the way of measurements and experiments for the purpose of providing a foundation for estimates and deductions. But infinitely more remains to be accomplished. The field of investigation is almost boundless, for there is hardly a department of geological dynamics over which it does not extend. The range of experimental geology must be widely enlarged, until every process susceptible of illustration or measurement by artificial means has been investigated. Field-observation needs to be supplemented where possible by instrumental determinations, so as to be made more precise and accurate, and more capable of furnishing trustworthy numerical statistics for practical as well as theoretical deductions.

The subject is too vast for adequate treatment here. But let me illustrate my meaning by selecting a few instances where the adoption of these more rigid methods of inquiry might powerfully assist us in dealing with the rates of geological processes and the value of geological time. Take, for example, the wide range of lines of investigation embraced under the head of Denudation. So voluminous a series of observations has been made in this subject, and so ample is the literature devoted to it, that no department of geology, it might be thought, has been more abundantly and successfully explored. Yet if we look through the pile of memoirs, articles and books, we cannot but be struck with the predominant vagueness of their statements, and with the general absence of such numerical data determined by accurate, systematic and prolonged measurement as would alone furnish a satisfactory basis for computations of the rate at which denudation takes place. Some instrumental observations of the greatest value have indeed been made, but, for the most part, observations of this kind have been too meagre and desultory.

A little consideration will show that in all branches of the investigation of denudation opportunities present themselves on every side of testing, by accurate instrumental observation and measurement, the rate at which some of the most universal processes in the geological *régime* of our globe are carried on.

It has long been a commonplace of geology that the amount of the material removed in suspension and solution by rivers furnishes a clue to the rate of denudation of the regions drained by the rivers. But how unequal in value, and generally how insufficient in precision, are the observations on this topic! A few rivers have been more or less systematically examined, some widely varying results have been obtained from the observations, and while enough has been obtained to show the interest and importance of the method of research, no adequate supply of materials has been gathered for the purposes of accurate deduction and generalisation. What we need is a carefully organised series of observations carried out on a uniform plan, over a sufficient number of years, not for one river only, but for all the important rivers of a country, and indeed for all the greater rivers of each continent. We ought to know as accurately as possible the extent of the drainage-area of each river, the relations of river-discharge to rainfall and to other meteorological as well as topographical conditions; the variation in the proportions of mechanical and chemical impurities in the river-water according to geological formations, form of the ground, season of the year and climate. The whole geological *régime* of each river should be thoroughly studied. The admirable report of Messrs. Humphreys and Abbot on the "Physics and Hydraulics of the Mississippi," published in 1861, might well serve as a model for imitation, though these observers necessarily occupied themselves with some questions which are not specially geological and did not enter into others on which, as geologists, we should now gladly have further information.

Again, the action of glaciers has still less been subjected to prolonged and systematic observation. The few data already obtained are so vague that we may be said to be still entirely ignorant of the rate at which glaciers are wearing down their channels and contributing to the denudation of the land.

The whole of this inquiry is eminently suitable for combined research. Each stream or glacier, or each well-marked section of one, might become the special inquiry of a single observer, who would soon develop a paternal interest in his valley and vie with his colleagues of other valleys in the fulness and accuracy of his records.

Nor is our information respecting the operations of the sea much more precise. Even in an island like Great Britain, where the waves and tides effect so much change within the space of a human life-time, the estimates of the rate of advance or retreat of the shore-line are based for the most part on no accurate determinations. It is satisfactory to be able to announce that the Council of this Association has formed a committee for the purpose of obtaining full and accurate information regarding alterations of our coasts, and that with the sanction of the Lords of the Admiralty, the co-operation of the coast-guard throughout the three kingdoms has been secured. We may therefore hope to be eventually in possession of trustworthy statistics on this interesting subject.

The disintegration of the surface of the land by the combined agency of the subaërial forces of decay is a problem which has been much studied, but in regard to whose varying rates of advance not much has been definitely ascertained. The meteorological conditions under which it takes place differ materially according to latitude and climate, and doubtless its progress is equally variable. An obvious and useful source of information in regard to atmospheric denudations is to be found in the decay of the material of buildings of which the time of erection is known, and in dated tombstones. Twenty years ago I called attention to the rate at which marble gives way in such a moist climate as ours, and cited the effects of subaërial waste as these can be measured on the monuments of our graveyards and cemeteries (*Proc. Roy. Soc. Edin.*, vol. x. 1879–80, p. 518). I would urge upon town geologists, and those in the country who have no opportunities of venturing far afield, that they may do good service by careful scrutiny of ancient buildings and monuments. In the churchyards they will find much to occupy and interest them, not, however, like Old Mortality, in repairing the tombstones, but in tracing the ravages of the weather upon them, and in obtaining definite measures of the rate of their decay.

The conditions under which subaërial disintegration is effected in arid climates, and the rate of its advance, are still less known, seeing that most of our information is derived from the chance observations of passing travellers. Yet this branch of the subject is not without importance in relation to the denudation, not only of the existing terrestrial surface, but of the lands of former periods, for there is evidence of more than one arid epoch in geological history. Here, again, a diligent examination of ancient buildings and monuments might afford some, at least, of the required data. In such a country as Egypt, for instance, it might eventually be possible to determine from a large series of observations what has been the average rate of surface-disintegration of the various kinds of stone employed in human constructions that have been freely exposed to the air for several thousand years.

Closely linked with the question of denudation is that of the deposition of the material worn away from the surface of the land. The total amount of sediment laid down must equal the amount of material abstracted, save in so far as the soluble portions of that material are retained in solution in the sea. But we have still much to learn as to the conditions, and especially as to the rate, of sedimentation. Nor does there appear to be much hope of any considerable increase to our knowledge until the subject is taken up in earnest as one demanding and justifying a prolonged series of well-planned and carefully executed observations. We have yet to discover the different rates of deposit, under the varying conditions in which it is carried on in lakes, estuaries, and the sea. What, for instance, would be a fair average for the rate at which the lakes of each country of Europe are now being silted up? If this rate were ascertained, and if the amount of material already deposited in these basins were determined, we should be in possession of data for estimating, not only the probable time when the lakes will disappear, but also the approximate date at which they came into existence.

But it is not merely in regard to epigene changes that further more extended and concerted observation is needed. Even among subterranean movements there are some which might be watched and recorded with far more care and continuity than have ever been attempted. The researches of Prof. George Darwin and others have shown how constant are the tremors, minute but measurable, to which the crust of the earth is subject (*Report Brit. Assoc.*, 1882, p. 95). Do these phenomena indicate displacements of the crust, and, if so, what in the lapse of a century is their cumulative effect on the surface of the land?

More momentous in their consequences are the disturbances which traverse mountain-chains and find their most violent expression in shocks of earthquake. The effects of such shocks have been studied and recorded in many parts of the world, but their cause is still little understood. Are the disturbances due to a continuation of the same operation which at first gave birth to the mountains? Should they be regarded as symptoms of growth or of collapse? Are they accompanied with even the slightest amount of elevation or depression? We cannot tell. But these questions are probably susceptible of some more or less definite answer. It might be possible, for instance, to determine with extreme precision the heights above a given datum of various fixed points along such a chain as the Alps, and by a series of minutely accurate measurements to detect any upward or downward deviation from these heights. It is quite conceivable that throughout the whole historical period some deviation of this kind has been going on, though so slowly, or by such slight increments at each period of renewal, as to escape ordinary observation. We might thus learn whether, after an Alpine earthquake, an appreciable difference of level is anywhere discoverable, whether the Alps as a great mountain-chain are still growing or are now subsiding, and we might be able to ascertain the rate of the movement. Although changes of this nature may have been too slight during human experience to be ordinarily appreciable, their very insignificance seems to me to supply a strong reason why they should be sought for and carefully measured. They would not tell us, indeed, whether a mountain-chain was called into being in one gigantic convulsion, or was raised at wide intervals by successive uplifts, or was slowly elevated by one prolonged and continuous movement. But they might furnish us with suggestive information as to the rate at which upheaval or depression of the terrestrial crust is now going on.

The vexed questions of the origin of raised beaches and sunk Forests might in like manner be elucidated by well-devised measurements. It is astonishing upon what loose and untrustworthy evidence the elevation or depression of coast-lines has often been asserted. On shores where proofs of a recent change of level are observable it would not be difficult to establish by accurate observation whether any such movements are taking place now, and, if they are, to determine their rate. The old attempts of this kind along the coasts of Scandinavia might be resumed with far more precision and on a much more extended scale. Methods of instrumental research have been vastly improved since the days of Celsius and Linnæus. Mere eye-observations would not supply sufficiently accurate results. When the datum-line has been determined with rigorous accuracy, the minutest changes of level, such as would be wholly inappreciable to the senses, might be detected and recorded. If such a system of watch were maintained along coasts where there is reason to believe that some rise or fall of land is taking place, it would be possible to follow the progress of the movement and to determine its rate.

But I must not dwell longer on examples of the advantages which geology would gain from a far more general and systematic adoption of methods of experiment and measurement in elucidation of the problems of the science. I have referred to a few of those which have a more special bearing on the question of geological time, but it is obvious that the same methods might be extended into almost every branch of geological dynamics. While we gladly and gratefully recognise the large amount of admirable work that has already been done by the adoption of these practical methods, from the time of Hall, the founder of experimental geology, down to our own day, we cannot but feel that our very appreciation of the gain which the science has thus derived increases the desire to see the practice still further multiplied and extended. I am confident that it is in this direction more than in any other that the next great advances of geology are to be anticipated.

While much may be done by individual students, it is less to their single efforts than to the combined investigations of many fellow-workers that I look most hopefully for the accumulation of data towards the determination of the present rate of geological changes. I would therefore commend this subject to the geologists of this and other countries as one in which individual, national and international co-operation might well be enlisted. We already possess an institution which seems well adapted to undertake and control an enterprise of the kind suggested. The International Geological Congress, which brings together our associates from all parts of the globe, would confer a lasting benefit on the science if it could organise a system of combined observation in any single one of the departments of inquiry which I have indicated or in any other which might be selected. We need not at first be too ambitious. The simplest, easiest and least costly series of observations might be chosen for a beginning. The work might be distributed among the different countries represented in the Congress. Each nation would be entirely free in its selection of subjects for investigation, and would have the stimulus of co-operation with other nations in its work. The Congress will hold its triennial gathering next year in Paris, and if such an organisation of research as I have suggested could then be inaugurated a great impetus would thereby be given to geological research, and France, again become the birthplace of another scientific movement, would acquire a fresh claim to the admiration and gratitude of geologists in every part of the globe.

SECTION D.

ZOOLOGY.

OPENING ADDRESS BY ADAM SEDGWICK, M.A., F.R.S., PRESIDENT OF THE SECTION.

Variation and some Phenomena connected with Reproduction and Sex.

IN the following Address an attempt is made to treat the facts of variation and heredity without any theoretical preconceptions. The ground covered has already been made familiar to us by the writings of Darwin, Spencer, Galton, Weismann, Romanes and others I have not thought it advisable to discuss the theories of my predecessors, not from a want of appreciation of their value, but because I was anxious to look at the facts themselves and to submit them to an examination which should be as free as possible from all theoretical bias.

Zoology is the science which deals with animals. Knowledge regarding animals is, for convenience of study, classified into several main branches, amongst the most important of which may be mentioned: (1) the study of structure; (2) the study of the functions of the parts or organs; (3) the arrangement of animals in a system of classification; (4) the past history of animals; (5) the relations of animals to their environment; (6) the distribution of animals on the earth's surface. That part of the science of zoology which deals with the functions of organs, particularly of the organs of the higher animals, is frequently spoken of as physiology, and separated more or less sharply from the rest of zoology under that heading. So strong is the line of cleavage between the work of the physiologist and that of other zoologists, that this Association has thought it advisable to establish a special Section for the discussion of physiological subjects, leaving the rest of zoology to the consideration of the old established Section, D. In calling attention to this fact, I do not for one moment wish to question the advisability of the course of action which the Association has taken. The science of physiology in its modern aspects includes a vast body of facts of great importance and great interest which no doubt require separate treatment. But what I do wish to point out is that it is quite impossible for us here to abrogate all our functions as physiologists Some of the most important problems of the physiological side of zoology still remain within the purview of this Section.

For instance, the important and far-reaching problems connected with reproduction and variation are very largely left to this Section, and that large group of intricate and almost entirely physiological phenomena connected with the adaptations of organisms to their environment are dealt with almost exclusively here. Indeed, we may go further, and say that apart altogether from practical questions of convenience, which make it desirable to separate a part of physiological work from the Zoological Section, it is, as a matter of fact, impossible to divorce the intelligent study of structure from that of function. The two are indissolubly connected together. The differentiation of structure involves the differentiation of function, and the differentiation of function that of structure. The conceptions of structure and function are as closely associated as those of matter and force. A zoologist who confined himself to the study of the structure of organisms, and paid no attention to the functions of the parts, would be as absurd a person as a philologist who studied the structure of words and took no account of their meaning. In the early part of this century, when the subject-matter of zoology was not so vast as it is at present, this aspect of the case was fully recognised, and one of the greatest zoologists of the century, whether considered from the point of view of modern anatomy, or of modern physiology, was Professor of Anatomy and Physiology at the University of Berlin.

Having said that much as to the various aspects of living nature, of natural history, if you like, which it falls within the province of this Section to deal with, I may now proceed to the subject of my address. And when I mention to you what that subject is, you will be able to make some allowance for the somewhat commonplace remarks with which I have treated you. For that subject, though it has its important morphological aspects, is in the main a physiological one; at any rate, no study which does not take account of the physiological aspect of it can ever hope to satisfy the intellect of man. The subject, then, to which I wish to draw your attention at the outset of our proceedings, is the great subject of Variation of Organisms.

As every one knows, there is a vast number of different kinds of organisms. Each kind constitutes a species, and consists of an assemblage of individuals which resemble one another more closely than they do other animals, which transmit their characteristics in reproduction and which habitually live and breed together. But the members of a species, though resembling one another more closely than they resemble the members of other species, are not absolutely alike. They present differences, differences which make themselves apparent even in members of the same family, *i.e.* in the offspring of the same parents. It is these differences to which we apply the term *variation*. The immense importance of the study of variations may be judged from the fact that, according to the generally received evolution theory of Darwin, it is to them that the whole of the variety of living and extinct organisms is due. Without variation there could have been no progress, no evolution in the structure of organisms. If offspring had always exactly resembled their parents and presented no points of difference, each succeeding generation would have resembled those previously existing, and no change, whether backwards or forwards, could have occurred. This phenomenon of genetic variation forms the bedrock upon which all theories of evolution must rest, and it is only by a study of variations, of their nature and cause, that we can ever hope to obtain any real insight into the actual way in which evolution has taken place. Notwithstanding its importance, the subject is one which has scarcely received from zoologists the attention which it merits.

Though much has been written on the causes of variation, too little attention has of late years been paid to the phenomenon. Since the publication of Darwin's great work on the "Variation of Animals and Plants under Domestication," there have been but few books of first-rate importance dealing with the subject. The most important of these is Mr. William Bateson's work, entitled "Materials for the Study of Variation." I have no hesitation in saying that I regard this work as a most important contribution to the literature of the evolution theory. In it attention is called, with that emphasis which the subject demands, to the supreme importance of the actual study of variation to the evolutionist, and a systematic attempt is made to classify variations as they occur in nature. In preparing this book Mr. Bateson has performed a very real service to zoology, not the least part of which is that he has made a most effective protest against that looseness of speculative reasoning which, since the publication of the "Origin of Species," has marred the pages of so many zoological writers.

The variations of organisms may be grouped under two heads, according to their nature and source: (1) There are those variations which appear to have no relation to the external conditions, for they take place when these remain unchanged, *e.g.* in members of the same litter; they are inherent in the constitution of the individual. These we shall call constitutional variations, or as their appearance seems nearly always to be connected with reproduction, they may be called *genetic* (congenital, blastogenic) *variations*. (2) The second kind of variations are those which are caused by the direct action of external conditions. These variations constitute the so-called *acquired characters*.

My first object is to consider these two kinds of variations, their nature, their causes, and their results on subsequent generations, and to inquire whether there are any fundamental differences between them. In this connection it is of particular importance that we should inquire whether acquired modifications are transmitted in reproduction. As is well known, there are two schools of thought holding directly opposite views as to this matter. The one of these schools—the so-called Lamarckian school—holds that they may be transmitted as such in reproduction; the other school, on the other hand, maintains that acquired modifications affect only the individual concerned, and are not handed on as such in reproduction. That the decision of the matter is not only theoretically important, but also practically, is evident, for upon it depends the answer to the question whether mental or other facilities acquired by the laborious exercise of the individual are ever transmitted to the offspring—whether the facility which the individual acquires in resisting temptation makes it any easier for the offspring to do the same, whether the effects of education are cumulative in successive generations. To put the matter as Francis Galton has put it, Is nature stronger than nurture, or nurture than nature?

We have then two kinds of variation to consider: (1) genetic variation, (2) acquired modification. It is the former of these—namely, genetic variation—with which I wish primarily to deal. Let us examine more fully the mode of its occurrence.

Genetic Variation.

Organised beings present, as you are aware, two main kinds of reproduction, the sexual and the asexual. These two kinds of reproduction present certain differences, of which the most important, and the only one which concerns us now, is the fact that genetic variation is essentially associated with sexual reproduction, and is rarely, if ever, found in asexual reproduction. In other words, whereas the offspring resulting from asexual reproduction as a rule exactly resemble the parent, they are always different from the parents in sexual reproduction. I am aware that I am treading on disputed ground. You will observe that I do not make the assertion that asexually produced offspring *always* exactly resemble the parent, and never present genetic variations. To say that would be going too far in the present state of our knowledge. Therefore I have put the matter less strongly, and merely assert that whereas asexual reproduction is on the whole characterised by identity between the offspring and the parent, sexual reproduction is always characterised by differences more or less marked between the two; and I reserve the question as to whether genetic variations are ever found in asexual reproduction for later consideration.

This modified form of the statement will, I think, be admitted on all hands, but before going on I will illustrate my meaning by reference to actual examples.

Asexual reproduction is a phenomenon comparatively rare in the animal kingdom, and when it does occur it is exceedingly difficult to investigate from this particular point of view. In the vegetable kindom, on the other hand, it is quite common. All, or almost all, plants possess this power, and in a very great many of them the result of its exercise can be fully followed out, and contrasted with that of sexual reproduction. Let us follow it out in the potato-plant. The potato can and does normally propagate itself asexually by means of its underground tubers. As you will know, if you take one of these and plant it, it gives rise to a plant exactly resembling the parent. If the tuber (seed as it is sometimes erroneously called) be that of the Magnum Bonum, it gives rise to a plant with foliage, flowers and tubers of the Magnum Bonum variety; if it be of the Snowdrop, the foliage, flowers, habit and tubers are totally different from the Magnum Bonum, and are easily identified as Snowdrops. In this way a favourable variety of potato can be reproduced to almost any extent with all its peculiarities of earliness or lateness, pastiness or mealiness, power of resisting disease and so forth. By asexual reproduction the exact facsimile of the parent may always be obtained, provided the conditions remain the same.

Now let us turn to the results of sexual reproduction—the seeds, *i.e.* the real seeds, which as you know are produced in the flowers, are the means by which sexual reproduction is effected. They are produced in great quantity by most plants, and when placed in the ground under the proper conditions they germinate and produce plants. But these plants do not resemble the parent. Try the seed of the Magnum Bonum potato, and raise plants from it. Do you think that any of them will be the Magnum Bonum with all its properties of keeping, resisting disease, and so forth? Not a bit of it. The probability is, that not one of your seedling plants will exactly reproduce the parents; they will all be different. Again, take the apple; if you sow the seed of the Blenheim Orange and raise young apple-trees, you will not get a Blenheim Orange. All your plants will be different, and probably not one will give you apples with the peculiar excellence of the parent. If you want to propagate your Blenheim Orange and increase the number of your trees, you must proceed by grafting or by striking cuttings, which are the methods by which such a tree may be asexually reproduced. And so on. Examples might be multiplied indefinitely. Every horticulturist knows that variety characterises seedlings, *i.e.* sexual offspring, whereas identity is found in slips, grafts and offsets, *i.e.* in asexual offspring; and that if you want to get a new plant you must sow seeds, while if you want to increase your stock of an old one you must strike cuttings, plant tubers or proceed in some analogous manner.

An apparent exception to this rule is afforded by so-called bud variation, but it is not certain that this is really an exception. In so far as these bud variations are not of the nature of acquired variations produced by a change of external conditions, and disappearing as soon as the old conditions are renewed, they are probably stages in the growth and development of the organism. That is to say, they are of the same nature as those peculiarities in animals which appear at a particular time of life, such as a single lock of hair of a different colour from the rest of the hair,[1] the change in colour of hair with growth,[2] the appearance of insanity or of epilepsy at a particular age. There

[1] Darwin, "Variation," vol. i. p. 449.

[2] As an example I may refer to the Himalayan rabbit; Darwin, "Variation," vol. i. p. 114.

is nothing more remarkable in a single bud on a tree departing from the usual character at a particular time of life, than in a particular hair of a mammal doing the same thing.

We have seen that, speaking broadly, genetic variation is connected with sexual reproduction, and it becomes necessary to examine this mode of reproduction a little more fully. What is the essence of sexual reproduction, and how does it differ from asexual? What I am now going to say applies generally to the phenomenon whether it occurs in plants or animals. Sexual reproduction is generally carried on by the co-operation of two distinct individuals—these are called the male and female respectively. They produce, by a process of unequal fission which takes place at a part of their body called the reproductive gland, a small living organism called the reproductive cell. The reproductive cell produced by the male is called in animals the spermatozoon, and is different in form from the corresponding cell produced by the female, and called in animals the ovum. The object with which these two organisms are produced is to fuse with one another and give rise to one resultant uninucleated organism or cell, which we may call the *zygote*. This process of fusion between the two kinds of reproductive cells, which are termed *gametes*, is called conjugation. The difference in structure between the male and female gamete is a matter of secondary importance only, and is connected with the primary function of coming into contact and fusing. The same may be said with regard to the so-called sexual differences of the parents of the two kinds of gametes, and to the powerful instincts which regulate their action. The conjugation of the male and female gamete, or the fertilisation of the ovum, as it is sometimes called, consists in the fusion of two distinct masses of protoplasm which are nearly always produced by different individuals. In the case of hermaphrodites, the term applied to organisms which produce both male and female gametes in the same individual, there is generally some arrangement which tends to prevent the male gamete from conjugating with the female gamete of the same parent; but this phenomenon is not absolutely excluded, and takes place as a normal phenomenon in many plants and possibly in some animals.

This fusion of the protoplasm of the two gametes gives us a uninucleated organism—for the fusion of the nuclei of the two gametes seems to be an essential part of the process—in which the potencies of the two gametes are blended. The *zygote*, as the mass formed of the fused gametes is called, is formed by the combination of two individualities, and is therefore essentially a new individuality, the characters of which will be different from the characters of both of the parents. This fact, which is not apparent in the zygote when first established, because the parts are hardly distinguishable by our senses, becomes obvious as soon as organs, with the appearance of which we are familiar, are formed. As a general rule this cannot be said to have occurred until what we call maturity has been nearly reached, because we are not familiar enough with the features of immature organisms to detect individual differences. But you may rest assured that such differences exist at all stages of growth from that of the uninucleated zygote till death. How the characters of the two parents will combine in the zygote it is impossible to predict, and the result is never the same even though the conjugations have been between gametes of identical origin. There may be an almost perfect mixture, the blending extending to even quite minute details; or the characters of the one parent may predominate—be prepotent, as we call it—over those of the other; or they may blend in such a way that the zygote offers characters found in neither parent. Or, finally, the features of one parent may come out at one stage of growth, those of the other at another stage. But, however the characters may blend, the product never exactly resembles the parents. The extent to which it differs from them is the measure of the variation.

To resume, it will be observed that in the method of reproduction sometimes called sexual two distinct processes occur. One of these is the real reproductive act, which consists in the production by fission of uninuclear individuals called gametes; the second is the fusion of the gametes to form the zygote. The gametes are of two kinds, and the reason of there being two kinds is intelligible when we consider the parts they have to play. The male gamete is nearly always endowed with locomotive power, and the female gamete is stored with food material to be used by the zygote in the first stages of growth. The destiny of these two uninucleated organisms is to fuse with one another, and so to give rise to a zygote which ultimately assumes the typical form of the species. As a general rule, the gametes have but a limited duration[1] of life unless they conjugate, and this is quite intelligible when we remember that they have no organs, *e.g.* digestive organs, suitable for the maintenance of life. It is rarely found that they have the power of assuming the form of their parent, unless they conjugate. This never happens in the case of the male gamete (at any rate in animals), and only rarely in that of the female. When it occurs—that is to say, when the ovum develops without conjugation—we call the phenomenon parthenogenesis. Parthenogenesis is found more commonly in Arthropods than in other groups, but it may be more common than is supposed.[2]

In sexual reproduction then, in addition to the real reproductive act, which is the division by fission of the parent into two unequal parts, the one of which continues to be called the parent, while the other is the gamete, there is the subsequent conjugation process. It is to this conjugation process that that important concomitant of sexual reproduction must be attributed, namely, genetic variation. We have thus traced genetic variation to its lair. We have seen that it is due to the formation of a new individuality by the fusion of two distinct individualities. We have also seen that in the higher animals it is always associated with the reproductive act.

Let us now take a wider survey and endeavour to ascertain whether this most important process, a process upon which depends the improvement as well as the degradation of races, ever takes place independently of the reproductive act. In the Metazoa, to which for our present purpose I allude under the term higher animals, conjugation never takes place except in connection with reproduction. It is impossible from the nature of the process that it should do so, as I hope to explain later on. But among the Protozoa, the simplest of all animals, it is conceivable that conjugation might take place apart from reproduction, and as a matter of fact it does do so. Let us now examine a case in which this occurs. Amongst the free-swimming ciliated Infusoria it frequently happens that two individuals become applied together, and that the protoplasm of their bodies becomes continuous. They remain in this condition of fusion for some days, retaining, however, their external form and not undergoing complete fusion. While the continuity lasts there is an exchange of living substance between the two bodies, in which exchange a bit of the nucleus of each participates. It thus happens that at the end of conjugation, when the two animals separate, they are both different from what they were at the commencement; each has received protoplasm and a nucleus from its fellow, and the introduced nucleus fuses as we know with the nucleus which has not moved. It would therefore appear that all the essential features of the conjugation process, as we learnt them in the case of the conjugation of the gametes in the Metazoa, are present, and it is impossible to doubt that we have here an essentially similar phenomenon. The phenomenon differs, however, from the conjugation first described in this interesting and important respect, that the two animals separate and resume their ordinary life. It is true that their constitution must have been profoundly changed, but they retain their general form. I say that the constitution of the exconjugates, as we may call them after they are separated, must be different from what it was before conjugation, but so far as I know no difference in structure corresponding with this difference in constitution has been recorded. I feel no sort of doubt, however, that structural differences, *i.e.* variations, will be detected when the exconjugates are closely scrutinised and compared with the animals before conjugation, and I would suggest that definite observations be made with a view to testing the point. Here, then, we have a case of conjugation entirely dissociated from reproduction. Other cases of a similar character are known among the Protozoa, though as a general rule the fusion between the conjugating organisms is complete and permanent. Among plants conjugation is generally associated with reproduction, but not always, for in certain fungi[3] fusion of hyphæ and consequent intermingling of protoplasm occurs, and is not followed by any

[1] Under favourable conditions, they may live a considerable time—*e.g.* the spermatozoon of certain ants, which are stated by Sir John Lubbock to live in some cases for seven years.

[2] It may be mentioned as a curious fact that parthenogenesis is rarely found in the higher plants, and, as I have said, is not known for the male gamete among animals.

[3] It must be mentioned, however, that in the case of these fungi the fusion of nuclei has not been observed, nor has it been noticed whether the habit, structure, or constitution of the conjugating plants are altered after the fusion.

form of reproduction. Among bacteria alone, so far as I know, has the phenomenon of conjugation never been observed.

To sum up, we have seen that the phenomenon of conjugation is very widely distributed. Excluding the bacteria, there is reason to believe that it forms a part of the vital phenomena of all organisms. Its essential features are a mixture and fusion of the protoplasm of two different organisms, accompanied by a fusion of their nuclei. It results in the formation of a new individuality, which differs from the individualities of both the conjugating organisms. This difference manifests itself in differences in habit, constitution, form, and structure, such differences constituting what we have called genetic variations.

The conjugation of the ovum and spermatozoon in the higher animals, and the corresponding process in the higher plants, are special cases of this conjugation, in which special conjugating individuals are produced, the ordinary individuals being physically incapable of the process. The phenomenon of sex, with all its associated complications, which is so characteristic of the higher animals and plants, is merely a device to ensure the coming together of the two gametes. In the lower animals, it is possible for the ordinary organism to conjugate; consequently the phenomenon does not form the precursor of developmental change, and is in no way associated with reproduction. Indeed, in such cases it is often the opposite of reproduction, inasmuch as it brings about a reduction in the number of individuals, two separate individuals fusing to form one.

Acquired Characters.

We now come to the consideration of the second kind of variations—namely, those which owe their origin to the direct action of external agencies upon the particular organism which shows the variation; or, as Darwin puts it, to the definite action of external conditions. These are the variations which I have called acquired variations or acquired characters. This is not a good name for them, but at the present moment, when I am about to submit them to a critical examination, I do not know of any other which could be suitably applied. Later on, when I sum up the various effects of the direct action of external agencies upon the organism, I may be able to use a more suitable term.

The main peculiarities of acquired variations are two in number; (*a*) they make their appearance as soon as the organism is submitted to the changed conditions; (*b*) speaking generally, they are more or less the same in all the individuals of the species acted upon. As examples of this kind of variations, I may mention the effect of the sun upon the skin of the white man; the Porto Santo rabbit, an individual of which recovered the proper colour of its fur in four years under the English climate;[1] the change of *Artemia salina* to *Artemia milhausenii*; the increase in size of muscles as the result of exercise; and the development of any special facility in the central nervous system. Among plants, variations of this kind are very easily acquired, by altering the soil and climate to which the individuals are submitted. So common are they, that it is quite possible that a large number of species are really based upon characters of this kind; characters which are produced solely by the external conditions, and which frequently disappear when the old conditions are reverted to.

With regard to these variations, we want to ask the following question: Do they ever last after the producing cause of them is removed, and are they transmitted in reproduction? In a great number of cases they either cease when the cause which has produced them is removed, or if they last the life of the individual they are not transmitted in reproduction. But is this always the case? That is the important question we now have to consider.

But before doing so let us inquire what acquired characters really are. The so-called adults of all animals have, as part of their birthright, a certain plasticity in their capacity of reacting to external influences; they all have a certain power of acquiring bodily and mental characters under the influence of appropriate stimuli. This power varies in degree and in quality in different species. In plants, for instance, it is mainly displayed in habit of growth, form of foliage, &c.; in man, in mental acquirements, and so on. But however it is displayed, it is this property of organisms which permits of the acquisition of those modifications of structure which have been so widely discussed as *acquired* characters. Now this power, when closely considered, is in reality only a portion of that capacity for development which all organisms possess, and with which they become endowed at the act of conjugation. A newly formed zygote possesses a certain number of hidden properties which are not able to manifest themselves unless it is submitted to certain external stimuli. It is these stimuli which constitute the external conditions of existence, and the properties of the organism which are only displayed under their influence are what we call acquired characters. They are acquired in response to the external stimuli.

It would appear, then, that every feature which successively appears in an organism in the march from the uninucleated zygote to death is an acquired character. At first the stimuli which are necessary are quite simple, being little more than appropriate heat and moisture; later on they become more complicated, until finally, when the developmental period is over and the mature life begins, the necessary conditions attain their greatest complexity, and their fulfilment constitutes what we call in the higher animals education. Education is nothing more than the response of the nearly mature organism to external stimuli, the penultimate response of the zygote to external stimuli, the ultimate being those of senile decay, which end in natural death. Acquired properties, it will be seen, are really stages in the developmental history. They differ in the complexity of the stimulus required to bring them out. For instance, the segmentation of the egg requires little more than heat and moisture, the walking of the chick the stimulus of light and sound and gravity, the evolutions of an acrobat the same in greater complexity, and, lastly, the action of a statesman requires the stimulation of almost every sense in the greatest complexity. Moreover, not only are there differences in the complexity of the stimulus required, but also in the rapidity with which the organism reacts to it. The chick undergoes its whole embryonic development in three weeks, a man in nine months; the chick develops its walking mechanism in a few minutes, while a man requires twelve months or more to effect the same end. Chickens are much cleverer than human beings in this respect. There is the same kind of difference between them that there is between the power of learning displayed by a Macaulay and that displayed by a stupid child.

An instinct is nothing more than an internal mechanism which is developed with great rapidity in response to an appropriate stimulus. It is difficult for us to understand instincts, because with us almost all developmental processes are extremely slow and gradual. This particularly applies to the development of those nervous mechanisms, the working of which we call reason.

Within certain limits the external conditions may vary without harming the organism, but such variations are generally accompanied by variations in the form in which the properties of the zygote are displayed. If the variations of the conditions are too great, their action upon the organism is injurious, and results in abortions or death. And in no case can the external conditions call out properties with which the zygote was not endowed at the act of conjugation.

It would thus appear that acquired characters are merely phases of development; they are the manifestations of the properties of the zygote, and are called forth only under appropriate stimulation; moreover, they are capable of varying within certain limits, according to the nature of the stimulus, and it is to these variations that the term acquired character has been ordinarily applied.

A genetic character, on the other hand, is the possibility of acquiring a certain feature under the influence of a certain stimulus; it is not the feature itself—that is an acquired character—but it is the possibility of producing the feature. Now as the possibility of producing the feature can only be proved to exist by actually producing it, the term genetic character is frequently applied to the feature itself, which is, as we have seen, an acquired character. In consequence of this fact, that we can only determine genetic characters by examining acquired characters, a certain amount of confusion may easily arise, and has indeed often arisen, in dealing with this subject. This can be avoided by remembering that in describing genetic characters account must always be taken of the conditions. For example, the white fur of the Arctic hare is an acquired character, acquired in response to a certain stimulus; while the power of so responding to the particular stimulus when applied at the correct time is a genetic character. Thus a genetic character is a character which depends upon the nature of the organism,

[1] Darwin, "Variation," ed. 2, vol. i. p. 119.

hile an acquired character depends on the nature of the timulus.

If we imagine a zygote to be a machine capable of working out certain results on material supplied to it, then we should properly apply the term genetic character to the features of the machinery itself, and the words acquired character to the results achieved by its working. These clearly will depend primarily on the structure of the machinery, and secondarily upon the material and energy supplied to it—that is to say, upon the way in which it is worked.

Variations in genetic characters are variations in the machinery of different zygotes—that is to say, in the constitution—while variations in acquired characters are variations in the results of the working of one zygote according to the conditions under which it is worked.

For instance, let us take the case of those twins which arise by the division of one zygote, and are consequently identical in genetic characters, *i.e.* in constitution. If they are submitted to different conditions, they will develop differences which will depend entirely upon the conditions and the time of life when the differentiation in the conditions occurred. These differences, then, will be a function of the external conditions, *i.e.* of the manner in which the machinery is worked, and constitute what we call variation in acquired characters.

Are Acquired Characters Transmissible as such in Reproduction?

To return to our question, are the so-called acquired characters ever transmitted in reproduction? Let us consider what this question means in the light of the preceding discussion. Acquired characters are features which arise in the zygote in response to external stimuli. Now the zygote at its first establishment has none of the characters which are subsequently acquired. All it has is the power of acquiring them. Clearly, then, acquired characters are not transmitted. The power of producing them is all that can be transmitted; and this power resides in the reproductive organs and in the gametes to which the reproductive organs give rise, so that the question must be put in another form. Is it possible by submitting an organism to a certain set of conditions, and thus causing it to acquire certain characters, so to modify its reproductive organs that the same characters will appear in its offspring as the result of the application of a different and simpler stimulus?

For instance, the power of reading conferred by education, the hardness of the hands and increased size of the muscles produced by manual labour: is it possible that these characters, now produced by complex external stimuli applied at a particular period of life, should ever in future ages be produced by the simpler stimuli found within the uterus, so that a man may be born able to read or write, or with hands horny and hard like those of a navvy?

In trying to find an answer to this question let us first of all look into the probabilities of the case, to see if we can relate the question to any other class of phenomena about which we have, or think we have, definite knowledge.

When an organism is affected by external agents the action may apply to the whole organisation or principally to one organ. Let us take a case in which one organ only appears to be affected, *e.g.* the enlargement by exercise of the right arm of a man. Now, although in this case it is only the muscles of the arm which appear at first sight to be affected, we must not forget that the organs of the body are correlated with one another, and an alteration of one will produce an alteration in others. By exercise of the right arm the muscles of that arm are obviously enlarged, but other changes not so obvious must also have taken place. The bones to which the muscles are attached will be altered; the blood-vessels supplying the muscles will be enlarged, and the nerves which act upon the muscles, and probably the part of the central nervous system from which they proceed, will also be altered. These are some of the more obvious correlated changes which will have occurred; no doubt there will have been others—indeed it is not perhaps too much to say that all the organs of the body will have reacted to the enlargement of the arm—but the effect on organs not in functional correlation with the muscles of the right arm will be imperceptible, and may be neglected. Thus the colour of the hair, the length and character of the alimentary canal, size of the leg muscles, the renal organs, &c., will not show appreciable alteration. Above all, the other arm will not be affected, or if it is affected the alteration will be so slight as not to be noticeable. Now, we know that homologous parts, whether symmetrically homologous or serially so, are in some kind of close connection. For instance, when one member of an homologous series varies, it is commonly found that other members of the same series will also vary. Yet in spite of this connection which exists between the right and left arms and between the right arm and right leg, there is no similar alteration either in the left arm or in the right leg. Now, if parts which from these facts we may suppose to be in some connection are not affected, how can we expect the reproductive organs, not only to be modified, but also to be so modified that the germs which are about to be budded off from them will be so affected as to produce exactly the same character—in this case enlarged muscle, &c.—without the application of the same stimulus, viz. exercise? Thus, while I freely admit that every alteration of an organ in response to external agents will react through the whole organisation, affecting each organ in functional correlation with the affected organ in a way which will depend upon the function of the correlated organ, and possibly other organs not in functional correlation in an indefinite way and to a slight extent, yet I maintain that it is very hard to believe that it will have such a sharp and precise effect upon every spermatozoon and ovum subsequently produced that not merely will these products be altered generally in all their properties, but that one particular part of them—and that part of them always the same—will be so altered that the organisms which develop from them will be able to present the same modification on the application of a different stimulus. It is inconceivable; unless, indeed, we suppose that the very molecules of the incipient organs in the germ are more closely correlated with corresponding parts of the parent body than are the homologous parts of the parent body with one another.

Now, to prove the existence of such a remarkable and intimate correlation would surely require the very strongest and most conclusive evidence. Is there any such strong evidence? I think I may fairly answer this question in the negative. The evidence which has been brought forward in favour of the so-called inheritance of acquired characters is far from conclusive. That such evidence[1] exists I do not deny, but it is all, or almost all, capable of receiving other interpretations.

Effect of Changed Conditions upon the Reproductive Organs.

On the other hand, all the certain evidence we have concerning what happens when the reproductive organs are affected, either directly or by correlation, by a change of conditions—and, as we have seen above, they must be affected if there is to be any change in the offspring—tends to show that there is not any relation between the effect produced on the parent and that appearing in the offspring.

The only means of judging whether the reproductive organs are affected by external conditions is by observing any change which may occur in their function. Now, only two such physiological effects of a change of conditions are certainly known; these are (1) the production of sterility or of partial sterility; (2) the production of an increased but indefinite variability in the offspring. With regard to the first of these effects: One of the most common, or at any rate one of the most noticeable, alterations in an organism, effected by change in the external conditions, is an alteration of the reproductive system, an alterationof such a kind that organisms which had previously freely interbred with one another are no longer able to do so. One of the most common results of removing organisms from their natural surroundings is to induce sterility or partial sterility. There is no reason to doubt that this sterility or tendency to sterility is, broadly speaking, due to an affection of the reproductive system. In the case of the higher animals, it may in some cases be due to an action upon the instincts, but in the lower animals and in plants we can hardly doubt that it is due to a direct action upon the reproductive organs. Indeed in plants these organs are often visibly affected. Among animals, however, there does not appear to be any satisfactory evidence on the point, and it is not known what organs are affected, whether it is the actual gametes, or the reproductive glands, or some of the other organs concerned.[2]

The other result of changed conditions which is certainly known is to induce an increased amount of variability of the genetic kind, though not immediately, often indeed not until

[1] For a good statement and discussion of the evidence in favour of this view, see Romanes' "Darwin and after Darwin," vol. ii chaps. 3 and 4.

[2] The exact cause of this sterility in the higher animals is a point which specially needs investigation.

after the lapse of some generations. On this point Darwin says: "Universal experience shows us that when new flowers are first introduced into our gardens they do not vary; but ultimately all, with the rarest exceptions, vary to a greater or less extent" ("Variation," 2, p. 249)[1] With regard to the variability thus induced, it is to be noticed that it is not confined to any particular organ, nor does it show itself in any particular way. On the contrary, the whole organisation is affected, and the variations are quite indefinite.

To sum up the argument as it at present stands: (1) a change in conditions cannot affect the next generation unless the reproductive organs are affected; (2) from a consideration of the facts of the case, it is almost inconceivable that the effect produced upon any organ of a given organism by a change of conditions should so modify the reproductive organs of that organism as to lead to a corresponding modification in the offspring without the latter being exposed to the same conditions; (3) the only effects, which are certainly known, of changed conditions upon the reproductive organs are (*a*) the production of sterility; (*b*) an increase in genetic variability.

As far, then, as our certain knowledge goes, it would appear that a change of conditions may have one or both of the following effects:—

(1) A definite change, of the same character, or nearly so, in all the individuals acted upon. Such changes may be adaptive or non-adaptive, but they are not permanent, lasting only so long as the change of conditions, or at most during the life of the individual acted upon. They are not transmitted in reproduction, and do not appear in the offspring unless it is submitted to the same conditions. These variations are the direct result of the action of the environment upon the individual, with the exception of the reproductive organs.

(2) Increase in the variations of the genetic kind These are seen, not in the generation[2] first submitted to the changed condition, but in the next or some subsequent generations. The effect is produced through the reproductive organs. These variations are non-adaptive, and different in each individual.

If the reproductive organs are affected we get an increase in the variations of the genetic kind. These, we have seen, are usually of an indefinite character; they are different in every case, and their nature cannot be predicted from experience. But we still have to ask: Is this is a universal rule? Does it never happen that a change of conditions so affects the reproductive organs as to produce a definite non-adaptive change of the same character or nearly so in all the descendants of the individual acted upon? This is the most obscure question connected with the study of variations. If such changes occur, they might be cumulative, being increased in amount by the continued action of the conditions. They would be non-adaptive, their nature depending on the constitution of the reproductive cells and having no functional relation to the original stimulus.

As possible examples of such variation, I may recall those variations referred to by Darwin as 'fluctuating variations which sooner or later become constant through the nature of the organism and of surrounding conditions, but not through natural selection" ("Origin," ed. 6, p. 176); to the variations in turkeys and ducks which take place as the result of domestication ("Variation," 2, p. 250); to those variations which Darwin had in his mind when he wrote the following sentence ("Origin," p. 72): "There can be little doubt that the tendency to vary in the same manner has often been so strong that all the individuals of the same species have been similarly modified without the aid of selection."

It is, however, as I have said, extremely doubtful if variations of this kind really occur. The appearance of them may be caused by the combination of the two other kinds of variation. In all cases which might be cited in support of their occurrence, there are the following doubtful elements: (1) no clear statement as to whether the variations showed themselves in the individuals first acted upon; (2) no history of the organisms when transported back to the old conditions.

[1] The phenomenon of increased variability following upon change or conditions has most often been observed when the change has been from a state of nature to a state of cultivation. Hence the conclusion has been drawn that the kind of change involved in domestication alone induces variation. But there is no evidence in favour of this view. The evidence shows that change of conditions in itself may induce greater variability.

[2] No doubt the individuals of the generation first submitted to the changed conditions would be affected as regards their reproductive organs, which would be altered in structure, but this has not been made out, though there are indications of such an effect in certain plants, *vide* Appendix.

Moreover, a general consideration of the facts of the case renders it improbable that such similar and definite genetic variations should often occur at any rate in sexual reproduction. For although the effect upon the reproductive organs may possibly be almost the same in nearly all the individuals acted upon, it must not be forgotten that the reproductive elements have to combine in the act of conjugation, and that it is the essence of this act to produce products which differ in every case.

Effect of Changed Conditions in Asexual Reproduction.

This brings us to the consideration of the question reserved on p. 503: Are genetic variations ever found in asexual reproduction?

If the views expressed in the earlier part of this address are correct, it would seem to follow that genetic variations are variations in the actual constitution, and are inseparably connected with the act of conjugation. The act of conjugation gives us a new constitution, a new individuality, and it is the characters of this new individual in so far as they differ from the characters of the parents which constitute what we have called genetic variations. According to this, the answer to our question would be that genetic variations cannot occur in asexual reproduction, and that if any indefinite variability recalling genetic variability makes its appearance[1] it must be part of the genetic variability and directly traceable to the zygote from which the asexual generations started.

But if genetic variability is not found in asexual reproduction, the question still remains, Can the other kind of variations—namely, those due to the direct action of external forces upon the organism—be transmitted in asexual reproduction? Now we have already seen that the effect of external agencies acting upon the organism must be regarded under two heads, according as to whether the reproductive organs are or are not affected. If the reproductive organs are not affected, then variations caused by the impact of external forces will not be transmitted; if, on the other hand, they are affected, the next generation will show the effect. We have further seen that in the case of sexual reproduction a modification of the reproductive organs will, because of the intervention of conjugation, appear as an increase in genetic variability only. How will the matter stand in the case of asexual reproduction? First, with regard to modifications which do not affect the reproductive system—they, as in sexual reproduction, will not be transmitted. Secondly, as regards modifications which do affect the reproductive organs—they will be transmitted, *i.e.* they will affect the next generation; and the question arises, How will they be transmitted? For here we have the opportunity wanting in the case of sexual reproduction of studying the transmission of modifications of the reproductive system without the complications introduced by the act of conjugation.

In considering this matter, it must be remembered that the reproductive organs are, with regard to external influences, exactly as any other organ. They can be modified either directly or indirectly, though they are in animals often less liable to direct modification by reason of their internal position.[2] These modifications may, as in the case of other organs, be obvious to the eye of the observer, or they may be so slight as only to be detected by an alteration of function. Now, in the case of the reproductive organs this alteration of function will show itself in the individuals of the next generation (if not before) which proceed directly and without any complication from the affected tissue. How will these individuals be affected? Will they all be affected in the same kind of way or will they they be affected in different ways? Finally, will the modi-

[1] Weismann, "On Heredity," vol. ii. English edition, p. 161. Warren E., "Observation on Heredity in Parthenogenesis," *Proc. Roy. Soc.*, 65, 1899, p. 154. These are the only observations I know of on this subject. They tend to show the presence of a slight variability, but they are not entirely satisfactory. In connection with this matter, I may refer to Weismann's view that *Cypris reptans*, the species upon which his observations were made, reproduces entirely by parthenogenesis, and has lost the power of sexual reproduction. This view is based on the fact that he has bred forty consecutive parthenogenetic generations and has never seen a male. As Weismann bases some important conclusions on this view, with regard to the importance of conjugation in rejuvenescence of organisms, I may point out that the fact that he has bred forty successive generations and has never seen a male cannot be regarded as conclusive evidence that males never appear. We know of many cases in which reproduction can continue for more than forty generations without the intervention of conjugation, *e.g.* ciliated infusoria, many plants, and of other species of crustacea in which the male is very rare and only appears after long intervals.

[2] How far the abnormal position of the testes of mammalia may receive its explanation in this connection is a question worthy of consideration.

fication last their lives only, or will it continue into subsequent asexually produced generations?

Let us endeavour to answer these questions:—

(1) How will the offspring be affected? That will depend entirely upon how the reproductive organ was affected. Will the modification in the offspring have an adaptive relation whatever to the external cause? Now here we have a capital opportunity, an opportunity not afforded at all by sexual reproduction, of examining by experiment and observation the Lamarckian position. My own opinion is that there will be no relation of an adaptive kind between the external cause and the modification of the offspring. For instance, let us imagine, as an experiment, that a number of parthenogenetically reproducing organisms are submitted to a temperature lower than that at which they are accustomed to live. Let us suppose that the cold affects their reproductive organs and produces a modification of the offspring. Will the modification be in the direction of enabling the offspring to flourish in a lower temperature than the parent? My own opinion, as I have said, is that there will probably be no such tendency in the offspring, if all possibility of selection be excluded. But that is only an opinion. The question is unsettled, and must remain unsettled until it is tested upon asexually reproducing organisms.

(2) Will they all be affected in the same kind of way? Yes, presumably they will. I arrive at this conclusion, not by experiment, but by reasoning from analogy. In the case of other organs of the body, the same external cause produces in all individuals acted upon, roughly speaking, the same kind of effect, *e.g.* action of sun upon skin, effect of transplanting maize, Porto Santo rabbits, &c. The question, however, cannot be settled in this way. It requires an experimental answer

(3) Will the modification last beyond the life of the individuals produced by the affected reproductive organ? I can give no answer to this question. We have no data upon which to form a judgment. We cannot say whether it is possible permanently to modify the constitution of an organism in this way, or whether, however strong the cause may be, consistently of course with the non-destruction of life, the effects will gradually die away—it may be in one, it may be in two or more generations. There are cases known which might assist in settling these questions, but I must leave to another opportunity the task of examining them. I refer to such cases as *Artemia salina*, various cases of bud variation which cannot be included under the head of growth variation.

Senile Decay and Rejuvenescence of Organisms.

Another question, also of the utmost importance, confronts us at this point. As is well known, organisms are liable to wear and tear, sooner or later some part or parts essential to the maintenance of the vital functions wear out and are not renewed by the reparative processes which are supposed to be continually taking place in the organism. This constitutes what we call senile decay, and leads to the death of the organism. As a good example of the kind of cause of senile decay, we may mention the wearing out of the teeth, which in mammals at any rate are not replaced; the wearing out of the elastic tissue of the arterial wall, which is probably not replaced. There is no reason to suppose that the reparative process of any organism is sufficiently complete to prevent senile decay. There is probably always some part or parts which cannot be renewed, even in the simplest organisms. Maupas has shown that this holds for the ciliated Infusoria, and he has also shown how the renewal of life, which of course must be effected if the species is to continue, is brought about. He has shown that it is brought about by conjugation, during which process the organism may be said to be put into the melting-pot and reconstituted. For instance, many of the parts of the conjugating individuals are renewed, including the whole nuclear apparatus, which there is every reason to believe is of the greatest importance to living matter.

On reconsidering the life of the Metazoa in light of the facts established by Maupas for the Infusoria, we see that all Metazoa are in a continual state of fission, as are the ciliated Infusoria. They are continually dividing into two unequal parts, one of which we call the parent and the other the gamete. The parent Metazoon must eventually die; it cannot be put into the melting-pot; its parts cannot be completely renovated. The gamete can be put into the melting-pot of conjugation, and give rise to an entirely reconstituted organism, with all the parts and organs brand new and able to last for a certain time, which is the length of life of the individual of the species.

Is there any other way than that of conjugation by which an organism can acquire a complete renewal of its organs? Is the renewal furnished by the development of all the parts afresh which takes place in a parthenogenetic ovum such a complete renewal? This question cannot now be certainly answered, but the balance of evidence is in favour of a negative answer. And this view of the matter is borne out by a consideration of the facts of the case. In all cases of conjugation which have been thoroughly investigated, the nuclear apparatus is completely renewed. It would appear, indeed, as though the real explanation of the uninuclear character of the Metazoon gamete is to be sought in the necessity of getting the nuclear apparatus into the simplest possible form for renewal. Now in the development of a parthenogenetic ovum the ordinary process of renewal of the nucleus is often in partial abeyance. As a rule, it only divides once instead of twice, and there is, of course, no reinforcement by nuclear fusion. It is, of course, possible that the reinforcement by nuclear fusion which occurs in conjugation may have a different explanation from the nuclear reconstitution which takes place in the formation of polar bodies and similar structures. On the other hand, it may all be part of the same process. We cannot tell. So that we are unable to answer the question whether for complete rejuvenescence a new formation of all parts of the organism is sufficient, or whether a reconstitution of the nuclear apparatus of the kind which takes place in the maturation of the Metazoon ovum and the division of the micro-nucleus of Paramœcium is also required; or, finally, whether in addition to the latter phenomenon a reinforcement and reconstitution by fusing with another nucleus is also necessary for that complete rejuvenescence which enables an organism to begin the life cycle again and to pass through it completely.

With regard to buds in plants, there is reason to believe that they share in the growing old of the parent. That is to say, if we suppose the average life of the individual to be 100 years, a bud removed at 50 will be 50 years of age, and only be able to live on the graft for 50 more years.

Heredity.

Having now spoken at some length of the phenomenon of variation, I must proceed to consider from the same general point of view the phenomenon of heredity.

As we have seen, in asexual reproduction heredity appears, as a general rule, if not always, to be complete. The offspring do not merely present resemblances to the parent—they are identical with it. And this fact does not appear to be astonishing when we consider the real nature of the process. Asexual reproduction consists in the separation off of a portion of the parent, which, like the parent, is endowed with the power of growth. In virtue of this property it will assume, if it does not already possess it, and if the conditions are approximately similar, the exact form of the parent. It is a portion of the parent; it is endowed with the same property of growth; the wonder would be if it assumed any other form than that of the parent. Indeed, it is doubtful if the word heredity would ever have been invented if the only form of increase of organisms was the asexual one, because there being no variation to contrast with it, it would not have struck us as a quality needing a name, any more than we have a name for that property of the number two which causes it to make four when duplicated.

The need for the word heredity only becomes apparent when we consider that other form of reproduction in which the real act of reproduction is associated with the act of conjugation. Looking at reproduction from a broad point of view, we may sum up the difference between the two kinds, the sexual and the asexual, by saying that, whereas the essence of sexual reproduction is the formation of a new individuality, asexual reproduction merely consists in increasing the number of one kind of individual. From this point of view sexual reproduction is better termed the creation of a new individuality, for that, and not the increase in the number of individuals, is its real result. Inasmuch as conjugation of two organisms is the essential feature of sexual reproduction, it would appear that the number of individuals would be actually diminished as a result of it; and this does really happen, though in a masked manner, for we are not in the habit of looking upon the spermatozoon and ovum as individuals, though it is absurd not to do so, as they contain latent all the properties of the species, and are sometimes able to manifest these properties (parthenogenetic ova) without conjugating. In some of the lower organisms the fact that conjugation does not result in an increase of the number of individuals, but only in the production of a new individuality, is quite apparent, for in them two of the ordinary individuals of the species fuse to form one (many Protozoa).

So that sexual reproduction gives us a new individuality which can spread to almost any extent by asexual reproduction. This asexual reproduction gives us a group of organisms which is quite different from a group of organisms produced by sexual reproduction. Whereas the latter groups constitute what we call species, the former group has, so far as I know, no special name, unless it be variety; but variety is not a satisfactory name, for it has been used in another sense by systematisers.

Heredity, then, is really applicable only to the appearance in a zygote of some of the properties of the gametes. A zygote has this property of one of the precedent gametes, and that property of the other, in virtue of the operation of what we call heredity; it has a third property possessed by neither of the precedent gametes in virtue of the action of variation, the nature of which we have already examined. It is impossible to say which property of a gamete will be inherited, and it is impossible to predict what odd property will result from the combination of the properties of the two gametes. Of one thing only are we certain, that they are never the same in zygotes formed by gametes produced in immediate succession from the same parent.

We may thus regard the activities of the zygote as the resultant of the dashing together of the activities of the gametes.

Conjugation, then, is a process of the utmost importance in biology; it provides the mechanism by which organisms are able to vary, independently of the conditions in which they live. It lies, therefore, at the very root of the evolution problem; the power of combining to form a zygote is one of the fundamental properties of living matter.

Species.

Now let us consider one of the effects of this property upon organisms. The effect to which I refer is the division of animals into groups called species. Species are groups of organisms the gametes of which are able to conjugate and produce normal zygotes. Now in nature there appear to be many causes which prevent gametes from conjugating. First and most important of all is some physical incompatibility of the living matter which prevents that harmonious blending of the two gametes which is essential for the formation of a normal zygote. Very little is known as to the real nature of this incompatibility; in fact, it is hardly an exaggeration to say that nothing is known. It may be that there is actual repulsion between the gametes, or it may be, in some cases, at least, that the gametes are able to fuse, but not to undergo that intimate blending which is necesary for the production of a perfect zygote. In some cases we know that something like this happens; for instance, a blend can be obtained between the horse and the ass, but it is not a perfect blend, the product or zygote being imperfect in one most important particular—namely, reproductive power.

A second cause which prevents conjugation is a purely mechanical one—viz. some obstacle which prevents the two gametes from coming together. As an instance of this I may refer to those cases amongst plants in which conjugation is impossible, because the pollen tube is not long enough to reach the ovule. In yet other cases conjugation is impossible because the organisms are isolated from one another either geographically or in consequence of their habits. There are probably many causes which prevent conjugation, but, whatever they may be, the effect of them is to break up organisms into specific groups, the gametes of which do normally conjugate with one another.

In many cases, no doubt, the gametes of organisms which are kept apart in nature by mechanical barriers will conjugate fully if brought together. But in the great majority of cases it is probable no amount of proximity will bring about complete conjugation. There is physical incompatibility. Here is a fruitful opening for investigation. Observations are urgently needed as to the real nature of this incompatibility.

Importance of the Study of Variation.

Another and most important effect of conjugation is, as we have seen, the much-spoken-of constitutional or genetic variations. They are, as we have already insisted, of the utmost importance to the evolutionist. Evolution would have been impossible without them, for it is made up of their summation. It becomes, therefore, desirable to find out to what extent a species is capable of varying. This can only be done, as Mr. Bateson has pointed out, by recording all variations found. Mr. Bateson, in his work already referred to, has carried this out, and has shown the way to a collection of these most important data. In order to carry it further, I would suggest that the collection be made, not only for structure, but also for function. This has been done largely for the nervous functions by psychologists and naturalists who pay special attention to the instincts of animals; but we want a similar collection for other functions. For instance, the variations in the phenomena of heat and menstruation, and of rut amongst mammals, and so on. To do this is really only to apply the methods of comparative anatomy and comparative physiology to the members of a species, as they have already been applied to the different species and larger groups of the animal kingdom. Such investigations cannot fail to be of the greatest interest. Indeed, when we have learnt the normal habits and structure of a species, what more interesting study can there be than the study of the possibilities of variation contained within it? Then, when we know the limits of variability of any given specific group, we proceed to try if we can by selective breeding or alteration of the conditions of life alter the variability, and perhaps call into existence a kind of variation quite different in character from that previously obtained as characteristic of the species.

The Evolution of Heredity and the Origin of Variation.

These remarks bring me to the consideration of a point to which I am anxious to call your attention, and which is an important aspect of our subject. Has the variability of organisms ever been different from what it is now? Has or has not evolution had its influence upon the property of organisms as it is supposed to have had upon their other properties? There is only one possible answer to this question. Undoubtedly the variability of organisms must have altered with the progress of evolution. It would be absurd to suppose that organisms have remained constant in this respect while they have undergone alteration in all their other properties. If the variability of organisms has altered, it becomes necessary to inquire in what direction has it altered? Has the alteration been one of diminution, or has it been one of increase? Of course, it is possible that there has been no general alteration in extent with the course of evolution, and that the alteration, on the whole, has been one of quality only. But passing over this third possibility, let us consider for the moment which of the two first-named alternatives is likely to have occurred.

According to the Darwinian theory of evolution, one of the most important factors in determining the modification of organisms has been natural selection. Selection acts by preserving certain favourable variations, and allowing others less favourable to be killed off in the struggle for existence. It thus will come about that certain variations will be gradually eliminated. Meanwhile the variations of the selected organisms will themselves be submitted to selection, and certain of these will be in their turn eliminated. In this way a group of organisms becomes more and more closely adapted to its surroundings; and unless new variations make their appearance as the old unfavourable ones are eliminated, the variability of the species will diminish as the result of selection. Is it likely that new variations will appear in the manner suggested? To answer this question we must turn to the results obtained by human agency in the selective breeding of animals. The experience of breeders is that continued selection tends to produce a greater and greater purity of stock, characterised by small variability, so that if the selective breeding is carried too far, variation almost entirely ceases, and there is little opportunity left for the exercise of the breeder's art. When this condition has been arrived at, he is obliged, if he wants to produce any further modifications of his animals, to introduce new blood—*i.e.* to bring in an individual which has either been bred to a different standard, or one in which the variability has not been so completely extinguished.

It would thus appear, and I think we are justified in holding this view, at any rate provisionally, that the result of continued selection will be to diminish the variability of a species; and if carried far enough, to produce a race with so little variability, and so closely adapted to its surroundings, that the slightest alteration in the conditions of life will cause extinction.[1]

If selection tends to diminish the variability of a species, then it clearly follows that as selection has been by hypothesis the most important means of modifying organisms, variation must have been much greater in past times than it is now. In fact, it must have been progressively greater the farther we go back from the present time.

The argument which I have just laid before you points, if carried to its logical conclusion—and I see no reason why it should not be so carried—to the view that at the first origin of life upon the earth the variability of living matter consequent upon the act of conjugation must have been of enormous range: in other words, it points to the view that heredity was a much less important phenomenon than it is at present. Following out the same train of thought, we are inevitably driven to the conclusion that one of the most important results of the evolutionary change has been the gradual increase and perfection of heredity as a function of organisms and a gradual elimination of variability.

This view, if it can be established, is of the utmost importance to our theoretical conception of evolution, because it enables us to bring our requirements as to time within the limits granted by the physicists. If variation was markedly greater in the early periods of the existence of living matter, it is clear that it would have been possible for evolutionary change to have been effected much more rapidly than at present—especially when we remember that the world was then comparatively unoccupied by organisms, and that with the change of conditions consequent on the cooling and differentiation of the earth's surface, new places suitable for organic life were continually being formed. It will be observed that the conclusion we have now reached, viz. that variation was much greater near the dawn of life than it is now, and heredity a correspondingly less important phenomenon, is a deduction from the selection theory. It becomes, therefore, of some interest to inquire whether a suggestion obtained by a perfectly legitimate mode of reasoning receives any independent confirmation from other sources. The first source of facts to which we turn for such confirmation must obviously be palæontology. But palæontology unfortunately affords us no help. The facts of this science are too meagre to be of any use. Indeed, they are wanting altogether for the period which most immediately concerns us—namely, the period when the existing forms of life were established. This took place in the prefossiliferous period, for in the earliest fossiliferous rocks examples of almost all existing groups of animals are met with.

But although palæontology affords us no assistance, there is one class of facts which, when closely scrutinised, do lend some countenance to the view that when evolutionary change was at its greatest activity, *i.e.* when the existing forms of life were being established, variation was considerably greater than it is at the present day.

But as this address has already exceeded all reasonable limits, and as the question which we are now approaching is one of very great complexity and difficulty, I am reluctantly compelled to defer the full consideration and treatment of it to another occasion. I can only hope that the far-reaching importance of my subject and the interest of it may to some extent atone for the great length which this address has attained.

APPENDIX.

The following observations on the condition of the male reproductive organs in highly variable plants are quoted from Darwin's "Variation of Animals and Plants under Domestication," vol. ii. p. 256 *et seq.*

In certain plant hybrids which are highly variable, it is known that the anthers contain many irregular pollen-grains. Exactly the same fact has been noticed by Max Wichura in many of our highly cultivated plants which are extremely variable, and which there is no reason to believe have been hybridised, such as the hyacinth, tulip, snapdragon, potato, cauliflower, &c.

[1] The expression extinction of species seems to be used in two senses which are generally confused. Firstly, a species may become modified so that the form with which we are familiar gradually gives place to one or more forms which have been gradually produced by its modification. That is to say, a character or series of characters becomes gradually modified or lost in successive generations. This is not really extinction, but development. Secondly, a species may gradually lose its variability, and become fixed in character. If the conditions then change, it is unable to adapt itself to them, and becomes truly extinct. In this case it leaves no descendants. We have to do with death, and not with development.

The same observer also "finds in certain wild forms the same coincidence between the state of the pollen and a high degree of variability, as in many species of *Rubus*; but in *R. caesius* and *idaeus*, which are not highly variable species, the pollen is sound." A little further on Darwin says "these facts indicate that there is some relation between the state of the reproductive organs and a tendency to variability; but we must not conclude that the relation is strict." Finally he sums up the matter in these words: "On the whole it is probable that any cause affecting the organs of reproduction would likewise affect their product—that is, the offspring thus generated."

NOTES.

IN his address to the French Association, at the recent Boulogne meeting, Dr. P. Brouardel took as his theme "Hygiene and its Progress during the last 100 Years." He paid special homage to the memory of the great Englishman and Frenchman, Jenner and Pasteur, who had done so much for the promotion of medical science. The first operation in vaccination made in France was performed at Boulogne, June 18, 1800. A public monument—a statue of Jenner—records the event. Referring to some preventive diseases, Dr. Brouardel remarked that in the French army the mortality from typhoid fever is now about 12 in 10,000, and in the present state of the water supply of many towns it is believed that this mortality will not be much reduced. In the German army, however, the mortality from typhoid fever is as low as 1 and 2 per 10,000, owing doubtless to the fact that an order of a Government authority addressed to any municipal body is immediately carried out, so that an impure water supply has soon to be replaced by a better one. But though some French municipalities are indifferent to their responsibilities, others do their duty well, and the mortality from typhoid fever for the whole of France is only 3 per 10,000. Dr. Brouardel referred to several other subjects which came within the range of preventive medicine.

THE application of the Jenner Institute of Preventive Medicine for permission to alter the memorandum of association so as to enable the institute to avail itself of Lord Iveagh's gift of 250,000*l.* was granted by Mr. Justice Cozens Hardy on September 13.

AFTER four months' work on his yacht, Dr. H. C. Sorby, F.R.S., has returned to Sheffield with many hundred specimens of marine animals, preserved by his new methods, so as to show lifelike character and natural colour.

THE Director of the Marine Observatory of San Fernando announces that the Spanish Minister of Finance has given instructions that all instruments intended for observations of the eclipse of the sun on May 27, 1900, are to be admitted free of duty.

SIR WILLIAM PREECE, K.C.B., has recently been making experiments with an electromagnetic system of wireless telegraphy in the Menai Straits. Using a telephone as a receiver, he has succeeded in establishing communication between stations half a mile apart, the messages heard being signals on the Morse code.

WE learn from the *Scientific American* that Prof. J. B. Hatcher, of Princeton University, has just returned from his geological expedition to Patagonia. The primary object of Prof. Hatcher's expedition was to make the most extensive collections possible of fossils of Patagonia. He also devoted considerable attention to gathering ethnological, botanical and zoological specimens. The first Mesozoic mammals ever discovered were found in Patagonia on this expedition, and upward of thirty cases of Mesozoic vertebrates were shipped