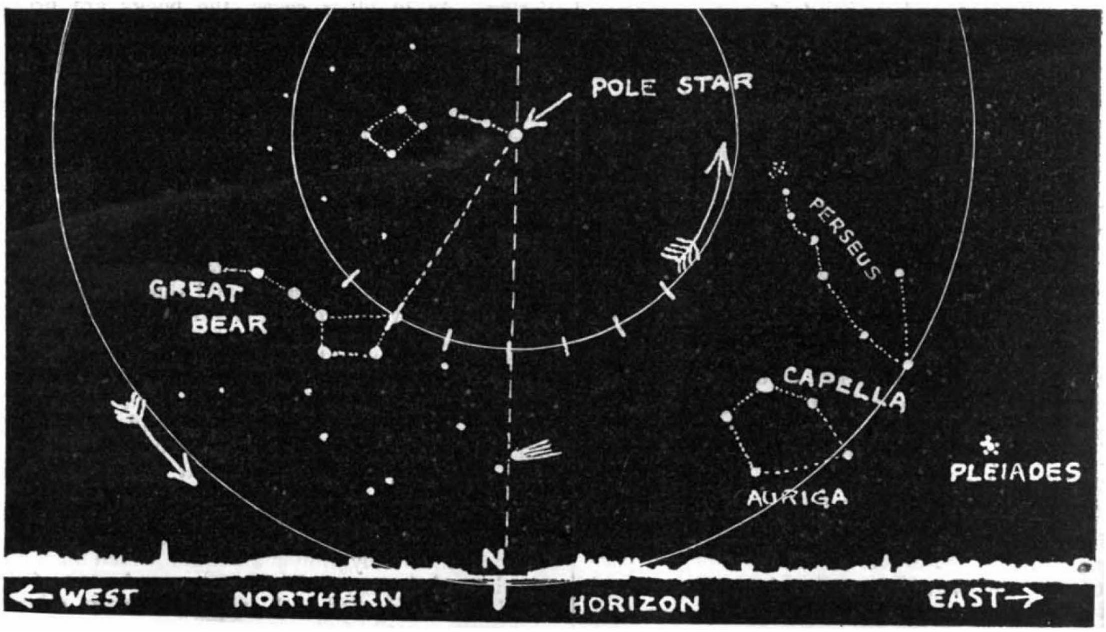
conspicuous object in the night sky. In this chart the observer is supposed to be looking due north, *i.e.* towards the pole star, the star being easily found by the direction of the two pointers in the Great Bear. If a vertical line be imagined drawn through the pole star towards the horizon, then this line will approximately pass through the comet at about ten o'clock in the evening. While the comet has not a great altitude, it is well above the northern horizon, as shown in the chart. To the left of the comet is the constellation of the Great Bear, and well to the right (eastwards) is the constellation of Auriga with the bright star Capella. It should not be forgotten that, owing to the rotation of the earth, the constellations have an apparent motion round the pole star, as indicated by the arrows marked near the circles in the chart. During the night, therefore, the stars and the comet, as one faces northward, describe concentric circles round the pole star, and consequently the stars *under* Polaris move from left to right. The diagram thus clearly indicates that the later the comet is observed, the higher above the horizon it will be situated, and the *best* time to observe it is between two o'clock and four o'clock in the morning. The divided part of the smaller circle in the diagram shows approximately the hourly apparent movement of the stars.



The comet is a naked eye object, and is a fine sight as seen even with a pair of field glasses or a small telescope. In the earlier part of the night, when the comet is low down, the tail is nearly horizontal, stretching out towards the east. As the morning approaches, the tail becomes more inclined, the head then being at a lower altitude than the tail. The comet has a very dense, almost stellar nucleus and a considerable length of tail. On August 26 the nucleus was estimated (visually) as being about magnitude five, and the tail about one degree long, but on August 28 the tail was judged to be equal to about four lunar diameters, *i.e.* about two degrees in length, and the nucleus of magnitude three. There is little doubt that this object is being closely followed at all observatories. Some good photographs of the spectrum of the nucleus were secured during the early mornings of last week at the Hill Observatory, Sidmouth, with a prismatic camera of 12 in. aperture and a 20° prism.

RADIAL MOTION IN SUNSPOTS.—The subject of the radial motion in sunspots is referred to in three communications to the July number (vol. xi., No. 1) of the *Astrophysical Journal*. The first is contributed by Prof. W. H. Julius, and the position he takes up is "to defend the attacked position which is by no means so weak as he represents it" in the criticism of Mr. St. John of the anomalous dispersion theory in explaining the observed phenomena. Space does not permit one even to summarise Prof. Julius' conclusions, but it may be stated that the paper covers thirty-two pages, and concludes with eleven paragraphs of summary. The communications by Messrs. Evershed and St. John deal with the question of the limits of the radial motion. Mr. St. John's investigations indicated that the usual course of the displaced lines over spots showed no sharp break, and the displacement did not suddenly cease at the periphery of the penumbra, but the line gradually returned to its normal course. Mr. Evershed's view is that there is an appreciable break or jolt in the lines at the points where they pass from the penumbra on to the surrounding photosphere. The communications suggest that these different views are to some extent one of degree, and possibly due to the differences caused by the instrumental equipment of these investigators. Mr. St. John hopes to utilise the next maximum period of sunspots to make a special examination of this question, and a programme has already been planned.

THE AUSTRALIAN MEETING OF THE BRITISH ASSOCIATION.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY PROF. SIR THOMAS H. HOLLAND, K.C.I.E., D.SC., F.R.S., PRESIDENT OF THE SECTION.

EXACTLY eighty-three years from the day of our arrival at Sydney, Edward Suess was born in London. Thus the day, as much as the circumstances of our meeting so far from home, serves to remind us of one who was great enough to recognise the fact that geological evidence from any part of the world has the same value as that obtained in the little continent which has been the most prolific in the products of nomenclature and the most productive in text-books.

Since the days of Charles Lyell no geologist has been so conspicuously successful in analysing the accumulated mass of evidence, in bringing together the essential facts from all hands, and in compensating for the local excesses of literature. Only those of us who, by long absence from Europe, have felt the full disadvantages of having to express our thoughts in alien terminology can appreciate the real value of Suess's great work. His death since our last meeting makes a conspicuous mark in the history of geological science.

A meeting of the British Association in Australia brings home forcibly to the members of Section C the fact that British Imperial geology is really "the science of the earth"; partly for this reason one feels inclined to get outside the science and take a survey of some of its suburbs. Not many of them have been left untraversed by my distinguished predecessors in this chair; but there has been of recent years a tendency to avoid the inner earth, which has rightly been described as "the inalienable playground of the imagination," and consequently, therefore, common land to the geologist as well as the geodesist, physicist, and mathematician.

The geologist who looks below the purely superficial phenomena of the crust is generally regarded as straying beyond his province; but the desire to see the birth certificate of some of the strange and often unacceptable "causes" which the mathematical physicist offers us is a pardonable form of curiosity. Our ideas regarding intra-telluric conditions are even proving to be of economic value, one of the most recent and unexpected results of the kind being that just established by Baron von Eötvös in Hungary,[1] whose predictions now bid fair to outstrip those of the "diviner"! Having noticed the low gravity values over the great cores of rock-salt in the Transylvanian "Schlier," he finds similar defects of gravity in the same region over certain of the Sarmatian and Pontian domes, which probably owe their shape to subterranean salt-plugs and are now found to be great storehouses of natural gas, which, with or without liquid petroleum, is commonly found with the saline "Mediterranean" facies of the Upper Tertiary in Eastern Europe. Baron von Eötvös also finds that on the eastern margin of the Great Hungarian Plain, where the younger Tertiary beds are completely concealed by a mantle of alluvium, mud-volcanoes and gas-springs are sometimes found in areas of marked gravity defect, and some of these are now also being drilled for natural gas.

When our ideas of the state of affairs below the surface thus begin to yield economic results, there is hope that they are at last steadying down, becoming more settled, and indeed more "scientific." It may not be unprofitable, therefore, to review some of the advances recently made in developing theoretical conceptions regarding the interior of the earth that are of direct importance to geologists. In undertaking this review I am conscious of the fact that I shall be traversing ground that is generally familiar to all, and much of it the special property of specialists whose views I hesitate to summarise and should not dare to criticise. As the author of the "Ingoldsby Legends" said of the only story that Mrs. Peters would allow her husband to finish, "The subject, I fear me, is not over new, but will remind my friends—

"Of something better they have seen before."

[1] *Comptes rendus*, XVIIème Conf. de l'Assoc. Géodés. Internat. Hamburg, 1912, pp. 427, 437.

The intensity and quantity of polemical literature on scientific problems frequently varies inversely as the number of direct observations on which the discussions are based: the number and variety of theories concerning a subject thus often form a coefficient of our ignorance. Beyond the superficial observations, direct and indirect, made by geologists, not extending below about one two-hundredth of the earth's radius, we have to trust to the deductions of mathematicians for our ideas regarding the interior of the earth; and they have provided us successively with every permutation and combination possible of the three physical states of matter—solid, liquid, and gaseous.

Starting, say, two centuries back with the astronomer Halley, geologists were presented with a globe of which the shell rotated at a rate different from that of its core. In more recent times this idea has been revived by Sir F. J. Evans (1878) to account for the secular variations in the declination of the magnetic needle.

Clairault's celebrated theorem (1743), on which Laplace based the most long-lived among many cosmogonies, gave us a globe of molten matter surrounded by a solid crust. Hopkins demanded a globe solid to the core, and, though his arguments were considered to be unsound, his conclusions have been revived on other grounds; while the high rigidity of the earth as a body has been maintained by Lord Kelvin, Sir George Darwin, Prof. Newcomb, Dr. Rudski, and especially by the recent observations of Dr. O. Hecker, supplemented by the mathematical reasoning of Prof. A. E. H. Love. Hennessy (1886), however, concluded that the astronomical demands could be satisfied by the old-fashioned molten earth in which the heavier substances conformed to the equatorial belt.

As long ago as 1858 Herbert Spencer suggested that, on account of its temperature being probably above the critical temperature of known elements, the centre of the earth is possibly gaseous. Late in the seventies Dr. Ritter revived the idea of a gaseous core surrounded by a solid crust, and this was modified in 1900 by the Swedish philosopher, Svante Arrhenius, whose globe with a solid crust, liquid substratum, and gaseous core is now a favourite among some geologists.

Wiechert (1897) supposed that the core of the earth, some 5,000 kilometres in radius, is composed mostly of iron with a density of 7·8, while this is surrounded by a shell of lithoidal material having a density of about 3·0 to 3·4; and this great contrast in density is about that which distinguishes the iron meteorites generally from those of the stony class. Arrhenius also assumes that iron forms the main part of the central three-quarters, and he shows that this distribution of substance may still be consistent with his theory of a gaseous core; indeed, he not only imagines that the whole of the iron nucleus is gaseous, but also most of the siliceous shell, for he leaves only 5 per cent. of the radius as the depth of the solid and liquid shells combined.

But the variety of ideas does not end with theories on the present constitution of the globe. Poisson required the process of solidification to begin from the centre and to progress outwards, while other mathematicians had been happy with the Leibnitzian *consistentior status* as the first external slaggy crust. Since the days of Laplace all naturalists have been forced to accept the idea of a solar system formed by the cooling and condensation of a spheroidal gaseous nebula; and all except those geologists who have vainly searched for traces of the primeval crust have been happy in this belief.

Recently, however, Dr. F. R. Moulton and Prof. T. C. Chamberlin in America have brought together arguments from different points of view to construct the solar system by the aggregation of innumerable small bodies, "planetesimals," which have gathered into knots to form the planets. Thus, the earth is supposed to have grown gradually by the accretion of meteoritic matter, and even now, although the process has nearly ceased, it receives much meteoritic material from outside.

With the Chamberlin-Moulton theory there must have been a time when the gravity of the earth was insufficient to hold an atmosphere of any but the heavier gases, such as carbon dioxide; later, the earth became heavy enough to retain oxygen, then nitrogen, water-vapour, and helium; while even now it may not be sufficiently attractive to prevent the light and agile molecule of hydrogen from flying off into space. With the growth of the young globe, the compression towards the centre produced heat enough to melt the accumulated fragments of meteoritic matter, and the molten material thus formed welled out at the surface. Such volcanic action is supposed to have predominated at the surface until an appreciable atmosphere was formed, and became charged with water, when the now familiar processes of weathering, erosion, and deposition produced the film of "rust" which geologists know as sedimentary rocks.

With this last addition to the variegated array of theories about the physical condition of the earth and about its genealogy, the scientific world began again to settle down into serenity, comforted by the happy feeling that all at any rate agree in regarding the earth as a gradually cooling body, with many millions of years still before it. Then came the discovery of radium, and, with it at first, an assurance that geologists were justified in claiming a long past, to be followed by a longer future than the most optimistic philosopher had dared before to assume with our apparently limited store of earth-heat. Now, however, Prof. Joly warns us that if the deeper parts of the globe contain anything near the proportion of radio-active bodies found by him in the superficial rocks, we may even be tending in the other direction; that, instead of a peaceful cooling, our descendants may have to face a catastrophic heating; the now inconspicuous little body known as the earth may indeed yet become famous through the universe as a new star.[2]

To add to the variety of ideas regarding the present state of the earth's interior, Prof. Schwarz, of Grahamstown,[3] concludes that our volcanic phenomena can be accounted for on the assumption that the main mass of the earth below a superficial layer is cold and solid throughout, being composed, like the meteorites, largely of unaltered ferromagnesian silicates and iron.

Thus, we see, whole fleets of hypotheses have been launched on this sea of controversy: some of the craft have been decoyed by the cipher-signals of the mathematician; some have foundered after bombardment by the heavy missiles classically reserved for use by militant geologists; others, though built in the dockyard of physicists, have suffered from the spontaneous combustion set up by an inadvertent shipment of radium. Still, some of these hypotheses are yet apparently seaworthy, and it may not be unprofitable to compare them with recently acquired data.

The nearest approach to actual observation with regard to the state of the earth's interior has been obtained by the seismograph, designed to record the movements of seismic waves at great distances from the disturbing earthquake. Some of the waves sent forth from an earthquake-centre travel through the earth, and some travel around by the superficial crust, the former reaching the distant seismograph before the latter. The seismograph, by its record of the waves that travel *through* the earth, has thus given a certain amount of information regarding the state of the earth's interior which R. D. Oldham aptly regards as analogous to that given by the spectroscope[4] with regard to the inaccessible atmosphere of the sun.

[2] J. Joly, "Radio-activity and Geology," 1909, pp. 168–172.
[3] E. H. L. Schwarz, "Causal Geology," 1910.

The existence of two groups of earthquake-waves —those passing through, and those passing near the surface around the earth—has long been recognised; but R. D. Oldham[5] has shown that the waves passing through the earth are of two kinds, travelling at two different speeds.

The record on the distant seismograph thus shows three well-marked phases: the first phase, due to waves of compression passing through the earth's interior; the second phase, due to waves of distortion,[6] also passing through the earth's interior; and the third phase, recorded by the waves which pass around the arc along the superficial crust.

The third phase is always recorded at a time after the occurrence of the shock proportional to the arcual distance of the recording seismograph from the earthquake centre, the records of several large earthquakes showing an average speed for the waves of about three kilometres a second. The rates of propagation of the waves giving the first and second phases are both much greater than of those forming the third phase; and up to an arcual distance of about 120° from the earthquake's centre the rate of their propagation increases with the distance. It is thus assumed that the waves giving rise to the first and second phases in each distant seismographic record, by following approximately along the chord of the arc between the place of origin and the instrument, pass through deeper layers of the earth when the seismograph is farther away, the material at greater depths being presumably more elastic as well as denser.

But Oldham[7] has shown that when the seismograph is as much as 150° from the earthquake centre there is a remarkable decrease in the mean apparent rate of propagation of the waves giving the second phase in the record, from more than six to about four and a half kilometres a second. There is also a drop, although not nearly so marked, in the apparent speed of the waves of the first phase when transmitted to a seismograph 150° or more distant from the earthquake origin. Oldham concludes that this decrease of apparent rate for waves travelling through the earth to places much more than 120° distant is due to their passing into a central core, four-tenths of the radius in thickness, composed of matter which transmits the waves at a markedly slow speed. Thus the earthquake waves which emerge at a distance not greater than 120° from their origin do not enter this central core, while those which pass into the earth to a greater depth than six-tenths of the radius are supposed to be refracted on entering, and again on leaving, the postulated core, in which the rate of transmission of an elastic wave of distortion is very much slower than in the main mass of the earth around. In consequence of the refraction of these waves on passing through the central core, places situated at about 140° from an earthquake origin should be in partial shadow, due to the great dispersion of the distortional waves, and the few records made so far by seismographs thus situated with regard to great

[4] In his presidential address to the Geological Society of London in 1909, Prof. W. J. Sollas (Proc. Geol. Soc., 1900, p lxxxvii.) credits H. Benndorf (Mitth. Geol. Gesellsch. Wien, I., 1908, 336) with this pretty analogy, but Oldham has the precedence by just two years (*cf.* Quart. Journ. Geol. Soc., vol. lxii., 1906, p. 456).
[5] Phil. Trans., Ser. A., vol. cxciv. (1900), pp. 135–74.
[6] There is more complete agreement regarding the fact that two distinct sets of waves give rise to the so-called preliminary tremors indicated by a seismographic record than about the nature of the waves. Confer. R. D. Oldham, Phil. Trans., *loc. cit.*, and O. Fisher, Proc. Cambr. Phil. Soc., vol. xii., pp. 354–61.
[7] Quart. Journ. Geol. Soc., vol. lxii., pp. 456–475 (1906).

earthquakes show that there is either no, or at most a doubtful, record for the second phase, which is known to be due to the so-called distortional waves.

Oldham's deductions are based confessedly on a small number of earthquake records—he considered fourteen examples only—but the conclusions based on a small number of trustworthy records, from which variations due to the different methods of marking the phases are eliminated, are more trustworthy than those for which there are imperfect distant records as well as doubts regarding the exact times of the disturbances. If these observations, however, be confirmed by further records, we are justified in assuming that below the heterogeneous crust there is a thick shell of elastic material, fairly homogeneous to about six-tenths of the radius, surrounding a central core, four-tenths in thickness, which possesses physical properties utterly unlike those of the outer layers; for in this core the "distortional" waves are either damped completely or are transmitted at very much lower speeds than in the shell.

One cannot consider this interesting inference from the seismographic data without being reminded of the contention of Ritter, Arrhenius, and Wilde regarding the possibility of a persistent gaseous core still above the critical temperature of the substances of which it is composed. According to Ritter,[8] the gaseous core is surrounded by a solid shell. Dr. Wilde[9] postulates the existence of a liquid substratum and a gaseous core within a solid crust, the two outer shells having a thickness that is "not very considerable." Arrhenius assumes from purely physical considerations that the solid crust is only about twenty-five miles thick, that below this it is possibly in a molten condition for about a hundred and fifty miles, and that the rest is a gas largely composed of iron under a pressure so great that its compressibility is not much less than that of steel.

The whole of these conclusions, being based on assumptions regarding the physical properties of matter under conditions of temperature and pressure that are well beyond those of actual experience, must be put on a plane of science well below that occupied by the investigations initiated by Oldham, who opens up a line of research in which, as said before, the seismograph may justifiably be compared with the spectroscope as an instrument for observing some inaccessible regions of nature.

The mathematician apparently finds it just as easy to prove that the earth is solid throughout as to show by extrapolation from known physical values that it must be largely gaseous. As Huxley said in his presidential address to the Geological Society in 1869, the mathematical mill is a mill which grinds you stuff of any degree of fineness, but, nevertheless, it can grind only what is put into it; and the seismograph thus offers a new source of substantial grist. Now that it is fairly certain that some of the earthquake-waves pass through the deeper parts of the earth, it is obvious that a fruitful development of science will follow successful efforts to introduce precision in recording, and uniformity of expression in reading, seismographic records.

Oldham[10] has pointed out another way in which analysis of seismographic records may lead to information regarding intra-telluric conditions by comparing the records of waves that pass under the oceanic depressions with those that are sub-continental for the whole or most of their paths. By comparing the records in Europe of the Colombian earthquake of January 31, 1906, with those of the San Francisco earthquake in the following April, there was a greater interval noticed between the first and second phases of the Californian earthquake—an interval greater than can be accounted for by mere difference of distance between the origin of the shock and the recording instruments. The seismic waves which passed from Colombia to Europe must have travelled under the broadest and deepest part of the North Atlantic basin, whilst those from California ran under the continent of North America, crossed the North Atlantic not far south of Iceland, and approached Europe from the north-west, the wave paths throughout being under continents or the continental shelf of the North Atlantic. There is thus suggested some difference between the elastic conditions of the sub-oceanic and the sub-continental parts of the crust—a difference which, judging by the particular instances discussed, may extend to a depth of one-quarter of the radius, but is not noticeable in the waves which penetrate to one-third of the radius below the surface.

Obviously these data must be multiplied many times before they can be regarded as a trustworthy index to a natural law; but it is significant that this indication of a difference between the physical nature of the sub-oceanic and sub-continental parts of the crust is in rough correspondence with the conclusions previously suggested on quite other grounds.

In his presidential address to the Geographical Section of the British Association at Dover in 1899, the late Sir John Murray directed attention to the chemical differentiation which has been going on between the continents and the oceans since the processes of weathering and denudation commenced. By these processes the more siliceous and specifically lighter constituents are left behind on the continents, while the heavier bases are carried out to the ocean. It is to this process that Prof. T. C. Chamberlin[11] also ascribes the origin of the depressions in which the oceanic waters have accumulated. As a corollary of the planetesimal theory, Chamberlin assumes that water began to be forced out of the porous surface blocks of the accumulated meteoritic material when the earth's radius was between 1500 and 1800 miles shorter than it is now; at that time pools of water began to be formed on the surface, and the atmosphere, just commencing its work, began the operation of leaching the heavier bases out of the highlands. Growth of the world proceeded by the infall of planetesimals, and while those meteorities that fell on the highlands became deprived of their soluble bases, those that fell into the young ocean were merely buried unaltered. Thus, by the time the earth reached its present size its crust under the oceanic depressions must have developed a chemical composition differing from that under the continents. According to the deduction suggested by Oldham from the seismographic records, there is a noticeable difference in the sub-oceanic areas to depths of between 1000 and 1300 miles—a layer in which the followers of Chamberlin's theory might reasonably expect some physical expression of the partially developed chemical differentiation.

The occurrence of denser material below the oceans has, of course, long been assumed from the deflection of the plumb-line, and was accepted by Pratt for his theory of compensation, as well as by Dutton as a wide expression of the theory of isostasy. Cham-

[8] A. Ritter, "Untersuchungen über die Höhe der Atmosphäre und die Constitution gasformiger Weltkörper," Wiedemann's Ann. d. Phys. und Chem., vol. v. 405, 543 (1878); vol. vi. 135 (1879); vii. 304 (1879); vol. viii. 157 (1879).

[9] "On the Causes of the Phenomena of Terrestrial Magnetism," Pamphlet, 1890, p. 2. The idea that the Earth's magnetism is due to the electricity generated by the friction between the shell and the core, rotating with a different motion, was suggested by Dr. Wilde in 1902 (Mem. Manch. Lit. and Phil. Soc., vol. xlvi., part iv. p. 8, 1902). A similar suggestion based also on Halley's conception of a separately rotating inner core was made previously by Sir F. J. Evans in 1878 ('Remarkable Changes in the Earth's Magnetism," NATURE, vol. xviii. p. 80).

[10] Quart. Journ. Geol. Soc., vol. lxiii, 344–350 (1907).

[11] Chamberlin and Salisbury, "Geology," vol. ii. 1906, 106–111.

berlin[12] thus explains the general prevalence of basic lavas in oceanic volcanoes.

The apparent heterogeneity indicated in the outer shell of the earth to depths of 1000 miles is naturally in conflict with the assumption that from thirty miles or so down the materials are in a liquid condition; at any rate, the idea conflicts with Fisher's extreme conception of the liquid substratum, in which the fluidity is supposed to be sufficient for the production of convection currents, upwards beneath the oceanic depressions, spreading horizontally towards the continents, and thence downwards to complete the circuit.

The idea that changes of azimuth and of latitude may be brought about by the sliding of the earth's crust over its core has been put forward more than once to account for the climatic changes of past geological ages—the occurrence of temperate or even warm climates on parts of the crust now within the polar circles, and glacial conditions at the sea-level in countries like India, Australia, Africa, and South America, which are now far from the polar ice-sheets, and in some cases near or within the tropics. Prof. E. Koken, of Tübingen,[13] in an elaborate memoir entitled "Indisches Perm und die Permische Eiszeit," attributes the idea of a sliding crust to Mr. R. D. Oldham; but a similar suggestion was put forward by the late Sir John Evans twenty years before the publication of Mr. Oldham's paper,[14] and when the theory was restated in more precise form, ten years later,[15] it was subjected to mathematical criticism by J. F. Twisden, E. Hill, and O. Fisher.[16]

Sir John Evans suggested that this movement of the crust was inevitable as a consequence of the moulding of the orographical features and consequent redistribution of weights; but Twisden came to the conclusion that the rearrangement of the great inequalities on the earth's surface would be insufficient to produce any appreciable sliding of the order required to make material differences in the climate of any place.

Oldham,[17] who was writing at the time in the field in India and thus away from literature, put forward the idea in 1886 as an independent thought, and made use of Fisher's new theory regarding the existence of a fluid stratum between the solid crust and the supposed solid core to account for the shifting of places relative to the axis of rotation from the equatorial region even to the polar circles. Oldham directed attention to the recorded small changes of latitude at certain observatories and to the probable changes of azimuth in the Pyramids of Egypt—evidences of a kind which have since been greatly enlarged by the work of Sir Norman Lockyer and others.

The movements assumed to have taken place during the human period are of course small; and to project from them changes as great as the transfer of lands from the polar circle to the tropics has the objection that characterises a surveyor's use of "unfavourable" triangles in a trigonometrical survey. Before admitting, therefore, that these small changes of latitude and of azimuth may be classed with the palæo-glacialists' evidence as data of the same kind, though so utterly different in magnitude, it is desirable briefly to examine the geological evidence regarding past ice-ages in extra-polar areas.

12 "Geology," ii. 1906, p. 120.
13 N. Jahrb. für. Min. u. s. w., 1907. 537.
14 J. Evans, "On a Possible Geological Cause of Changes in the Position of the Axis of the Earth's Crust," Proc. Roy. Soc., xv. 46 (1866).
15 J. Evans, Presidential Address, Proc. Geol. Soc., 1876, p. 105.
16 J. F. Twisden, "On Possible Displacements of the Earth's Axis of figure produced by Elevations and Depressions of her Surface," Quart. Journ. Geol. Soc., xxxiv. 35 (1877). E. Hill, "On the Possibility of Changes in the Earth's Axis," *Geol. Mag.*, 1878, 262 and 479. O. Fisher, "On the Possibility of Changes in the Latitude of Places on the Earth's Surface," *Geol. Mag.*, 1878, pp. 291 and 551.
17 *Geol. Mag.*, 1886, 304.

From the records of ancient glaciations we might omit those of the pre-Cambrian rocks of North Ontario and the pre-Upper Cambrian of Norway, as these areas are nearer the poles than many places which were certainly covered with ice-sheets during the youngest, or often so-called Great, Ice Age. But besides these we have evidence of glaciation in the Cambrian or possibly pre-Cambrian rocks of South Australia at a latitude of 35° or less; in South Africa there were two or more distinct glacial periods before Lower Devonian times in slightly lower latitudes; while in China similar records are found among rocks of the Lower Cambrian, or possibly of older age, at a latitude of 31° N.

The glacial boulder-beds found at the base of our great coal-bearing system in India belong to the same stratigraphical horizon as the glacial beds found in South Africa, certain parts of Australia, and in parts of Brazil and São Paulo near or within the southern tropic.

These glacial beds are often referred to in geological literature as Permo-Carboniferous in age; but Prof. Koken regarded the formation in India as Permian. Other valuations of palæontological evidence, similar to that relied on by Prof. Koken, place these beds at a distinctly lower horizon in the European stratigraphical scale, and recent work by officers of the Geological Survey of India in Kashmir tends to confirm this latter view; we now regard the base of our great coal-bearing system in India—the horizon of the glacial-boulder-beds—as not much, if at all, younger than the Upper Coal Measures of Britain.[18] The precise age of the horizon is not very important for our present consideration: the important point is that in or near Upper Carboniferous times a widespread glaciation occurred throughout the area now occupied by India, Australia, and South Africa. The records of this great glaciation are thus found stretching northwards beyond the northern as well as southwards beyond the southern tropic.

Now, on the assumption that the cold climate in this region was due to a movement of the crust over the nucleus, Prof. Koken has produced an elaborate map of the World, showing the distribution of land and sea during the period, with the directions of ocean-currents and of ice-sheets. The Permian South Pole he places at the point of intersection of the present 20th parallel S. and 80th meridian E.—that is, at a point in the Indian Ocean about equidistant from the glaciated regions of India, Australia, and South Africa. The Permian North Pole is thus forced to take up its position in the centre of Mexico, while the Equator strikes through Russia, Italy, West Africa, down through the South Atlantic and round by Fiji to Vladivostock.

The very precision of this map reduces the theory on which it is based to a condition of unstable equilibrium. If glacial conditions were developed in India, Australia, and South Africa by a 70° movement of the crust, were the movements to and from its assumed position in Permian times so rapid that the glaciation of these widely separated areas appear to be geologically contemporaneous? If such movements had occurred, instead of evidences of glaciation over a wide area at the *same* period, we ought rather to find that the glaciation in each of the widely separated points occurred during distinctly *different* geological periods.

But that is not the only weak spot in the evidence. The Permian (or Permo-Carboniferous) glaciation of Australia took place on the east and south-east of the continent as well as in Western Australia, and

18 H. H. Hayden, Rec. Geol. Surv. Ind., vol. vxxvi., p. 23, 1907.

the eastern ice-sheets would thus have been active within 30° of Prof. Koken's Permian equator. There are still three other serious pieces of colour-discord in this picture. In the State of São Paulo—that is, within Koken's "Permian" tropics—Dr. Orville Derby has described beds which strikingly recall the features of the Upper Palæozoic glacial beds of India and South Africa. It is possible that these are due to the work of glaciers at a high level; but, since the publication of Prof. Koken's memoir, other occurrences of the kind have been described by Dr. I. C. White in different parts of Brazil, and there is a general correspondence between the phenomena in South America and those in the formations of the same age in the Indian, Australian, and African regions.

Then, too, if we accept this expression of the physical geography during Upper Palæozoic times, we must carefully explain away the suspicious breccias and brockrams which have been regarded by many geologists as evidences of a cold climate during Permian times in the Urals, the Thüringerwald, the English midland and northern counties, Devonshire and Armagh—places that would lie on or near Koken's "Permian" equator. Finally, we find the hypothetical Permian North Pole in a locality which has failed to produce any signs of glaciation.

To attempt a discussion of the explanations offered to account for the great Upper Palæozoic glaciation would lead us far from the present theme. The question is raised merely to show that the phenomena are not consistent with the supposed movement of a solid shell over a solid core assisted by an intermediate molten lubricant. Geologists may be compelled to hand back the theory of a molten substratum to the mathematicians and physicists for further repair; but it does not necessarily follow that a foundation theory is unsound merely because it has been overloaded beyond its compressive strength.

The extraordinarily great distances between the areas that show signs of glaciation in Permo-Carboniferous times form a serious stumbling-block to most of the explanations which have hitherto been offered. One is almost tempted in despair even to ask if it is not possible that these fragments of the old Gondwana continent are now more widely separated from one another than they were in Upper Palæozoic times. It is a bold suggestion indeed that one can safely put aside as absurd in geomorphology. There is nothing else apparently left for us but the assumption of a general refrigeration.

The idea of the greater inequalities of the globe being in approximately static equilibrium has been recognised for many years: it was expressed by Babbage and Herschel; it was included in Archdeacon Pratt's theory of compensation; and it was accepted by Fisher as one of the fundamental facts on which his theory of mountain structure rested. But in 1889 Captain C. E. Dutton presented the idea "in a modified form, in a new dress, and in greater detail"; he gave the idea orthodox baptism and a name, which seems to be necessary for the respectable life of any scientific theory. "For the condition of equilibrium of figure, to which gravitation tends to reduce a planetary body, irrespective of whether it be homogeneous or not," Dutton[19] proposed "the name *isostasy*." The corresponding adjective would be isostatic—the state of balance between the ups and downs on the earth.

For a long time geologists were forced to content themselves with the conclusion that the folding of strata is the result of the crust collapsing on a cooling and shrinking core; but Fisher pointed out that the amount of radial shrinking could not account even for the present great surface inequalities of the lithosphere, without regard to the enormous lateral shortening indicated by the folds in great mountain regions, some of which, like the Himalayan folds, were formed at a late date in the earth's history, folds which in date and direction have no genetic relationship to G. H. Darwin's primitive wrinkles. Then, besides the folding and plication of the crust in some areas, we have to account for the undoubted stretching which it has suffered in other places, stretching of a kind indicated by faults so common that they are generally known as normal faults. It has been estimated by Claypole that the folding of the Appalachian range resulted in a horizontal compression of the strata to a belt less than 65 per cent. of the original breadth. According to Heim the diameter of the northern zone of the central Alps is not more than half the original extension of the strata when they were laid down in horizontal sheets. De la Beche, in his memoir on Devon and Cornwall, which anticipated many problems of more than local interest, pointed out that, if the inclined and folded strata were flattened out again, they would cover far more ground than that to which they are now restricted on the geological map. Thus, according to Dutton, Fisher, and others, the mere contraction of the cooling globe is insufficient to account for our great rock-folds, especially great folds like those of the Alps and the Himalayas, which have been produced in quite late geological times. It is possible that this conclusion is in the main true; but in coming to this conclusion we must give due value to the number of patches which have been let into the old crustal envelope—masses of igneous rock, mineral veins and hydrated products which have been formed in areas of temporary stretching, and have remained as permanent additions to the crust, increasing the size and bagginess of the old coat, which, since the discovery of radium, is now regarded as much older than was formerly imagined by non-geological members of the scientific world.

The peculiar nature of rock-folds presents also an obstacle no less formidable from the qualitative point of view. If the skin were merely collapsing on its shrinking core we should expect wrinkles in all directions; yet we find great folded areas like the Himalayas stretching continuously for 1400 miles, with signs of a persistently directed overthrust from the north; or we have folded massss like the Appalachians of a similar order of magnitude stretching from Maine to Georgia, with an unmistakable compression in a north-west to south-east direction. The simple hypothesis of a collapsing crust is thus "quantitatively insufficient," according to Dutton, though this is still doubtful, and it is "qualitatively inapplicable," which is highly probable.

In addition to the facts that rock-folds are maintained over such great distances and that later folds are sometimes found to be superimposed on older ones, geologists have to account for the conditions which permit of the gradual accumulation of enormous thicknesses of strata without corresponding rise of the surface of deposition.

On the other hand, too, in folded regions there are exposures of beds superimposed on one another with a total thickness of many miles more than the height of any known mountain, and one is driven again to conclude that uplift has proceeded *pari passu* with the removal of the load through the erosive work of atmospheric agents.

It does not necessarily follow that these two processes are the direct result of loading in one case and

[19] Dutton, "On Some of the Greater Problems of Physical Geology," Bull. Phil. Soc. Wash., xi., 53, 1889.

of relief in the other; for slow subsidence gives rise to the conditions that favour deposition and the uplifting of a range results in the increased energy of eroding streams.

Thus there was a natural desire to see if Dutton's theory agreed with the variations of gravity. If the ups and downs are balanced, the apparently large mass of a mountain-range ought to be compensated by lightness of material in and below it. Dutton was aware of the fact that this was approximately true regarding the great continental plateaux and oceanic depressions; but he imagined that the balance was delicate enough to show up in a small hill-range of 3000 to 5000 feet.

The data required to test this theory, accumulated during the triangulation of the United States, have been made the subject of an elaborate analysis by J. F. Hayford and W. Bowie.[20] They find that, by adopting the hypothesis of isostatic compensation, the differences between the observed and computed deflections of the vertical caused by topographical inequalities are reduced to less than one-tenth of the mean values which they would have if no isostatic compensation existed. According to the hypothesis adopted, the inequalities of gravity are assumed to die out at some uniform depth, called the depth of compensation, below the mean sea-level. The columns of crust material standing above this horizon vary in length according to the topography, being relatively long in highlands and relatively short under the ocean. The shorter columns are supposed to be composed of denser material, so that the product of the length of each column by its mean density would be the same for all places. It was found that, by adopting 122 kilometres as the depth of compensation, the deflection anomalies were most effectually eliminated, but there still remained unexplained residuals or local anomalies of gravity to be accounted for.

Mr. G. K. Gilbert,[21] who was one of the earliest geologists to turn to account Dutton's theory of isostasy, has recently offered a plausible theory to account for these residual discrepancies between the observed deflections and those computed on the assumption of isostatic compensation to a depth of 122 kilometres. An attempt had already been made by Hayford and Bowie to correlate the distribution of anomalies with the main features of the geological map and with local changes in load that have occurred during comparatively recent geological times. For example, they considered the possibility of an increased load in the low Mississippi valley, where there has been in recent times a steady deposition of sediment, and therefore possibly the accumlation of mass slightly in advance of isostatic adjustment. One would expect in such a case that there would be locally shown a slight excess of gravity, but, on the contrary, there is a general prevalence of negative anomalies in this region. In the Appalachian region, on the other hand, where there has been during late geological times continuous erosion, with consequent unloading, one would expect that the gravity values would be lower, as isostatic compensation would naturally lag behind the loss of overburden; this, however, is also not the case, for over a greater part of the Appalachian region the anomalies are of the positive order. Similarly, in the north central region, where there has been since Pleistocene times a removal of a heavy ice-cap, there is still a general prevalence of positive anomalies.

These anomalies must, therefore, remain unexplained by any of the obvious phenomena at the command of the geologist. G. K. Gilbert now suggests that, while it may be true that the product of the length of the unit column by its mean density may be the same, the density variations within the column may be such as to give rise to different effects on the pendulum. If, for instance, one considers two columns of the same size and of exactly the same weight, with, in one case, the heavy material at a high level and in the other case with the heavy material a low level, the centre of gravity of the former column, being nearer the surface, will manifest itself with a greater pull on the pendulum; these columns would be, however, in isostatic adjustment.[22]

Gilbert's hypothesis thus differs slightly from the conception put forth by Hayford and Bowie; for Gilbert assumes that there is still appreciable heterogeneity in the more deep-seated parts of the earth, while Hayford and Bowie's hypothesis assumes that in the nuclear mass density anomalies have practically disappeared, and that there is below the depth of compensation an adjustment such as would exist in a mass composed of homogeneous concentric shells.

In order to make the Indian observations comparable with those of the United States as a test of the theory of isostasy, Major H. L. Crosthwait[23] has adopted Hayford's system of computation and has applied it to 102 latitude stations and 18 longitude stations in India. He finds that the unexplained residuals in India are far more pronounced than they are in the United States, or, in other words, it would appear that isostatic conditions are much more nearly realised in America than in India.

The number of observations considered in India is still too small for the formation of a detailed map of anomalies, but the country can be divided into broad areas which show that the mean anomalies are comparable with those of the United States only over the Indian peninsula, which, being a mass of rock practically undisturbed since early geological times, may be regarded safely as having approached isostatic equilibrium. To the north of the peninsula three districts form a wide band stretching west-north-westwards from Calcutta, with mean residual anomalies of a positive kind, while to the north of this band lies the Himalayan belt, in which there is always a large negative residual.

Colonel Burrard[24] has considered the Himalayan and sub-Himalayan anomalies in a special memoir, and comes to the conclusion that the gravity deficiency is altogether too great to be due to a simple geosynclinal depression filled with light alluvium such as we generally regard the Gangetic trough to be. He suggests that the rapid change in gravity values near the southern margin of the Himalayan mass can be explained only on the assumption of the existence of a deep and narrow rift in the sub-crust parallel to the general Himalayan axis of folding. A single

20 J. F. Hayford, "The Figure of the Earth and Isostasy," U.S. Coast and Geodetic Survey, Washington, 1909. "Supplementary Investigation," Washington, 1910. See also "Science," New Series, vol. xxxiii., p. 199, 1911. J. F. Hayford and W. Bowie, "The Effect of Topography and Isostatic Compensation upon the Intensity of Gravity," "U.S. Coast and Geodetic Survey Special Publication No. 10," Washington, 1912.

21 "Interpretation of Anomalies of Gravity," "U.S. Geol. Surv. Professional Paper," 85-C, 1913, p. 29.

22 It is interesting to note that the idea suggested by G. K. Gilbert in 1913 was partly anticipated by Major H. L. Crosthwait in 1912 ("Survey of India, Professional Paper," No. 13, p. 5). Major Crosthwait, in discussing the similar gravity anomalies in India, remarks parenthetically: "Assuming the doctrine of isostasy to hold, is it not possible that in any two columns of matter extending from the surface down to the depth of compensation there may be the same mass, and yet that the density may be very differently distributed in the two columns? These two columns, though in isostatic equilibrium, would act differently on the plumb-line owing to the unequal distribution of mass.

"The drawback to treating this subject by hard and fast mathematical formulæ is that we are introducing into a discussion of the constitution of the earth's crust a uniform method when, in reality, probably no uniformity exists."

23 "Survey of India, Professional Paper," No. 13, 1912.

24 "Survey of India, Professional Paper," No. 12, 1912.

large rift of the kind and size that Colonel Burrard postulates is a feature for which we have no exact parallel; but one must be careful not to be misled by the use of a term which, while conveying a definite mental impression to a mathematician, appears to be incongruous with our geological experience. There may be no such thing as a single large rift filled with light alluvial material, but it is possible that there may still be a series of deep-seated fissures that might afterwards become filled with mineral matter.

With this conception of a rift or a series of rifts, Colonel Burrard is led to reverse the ordinary mechanical conception of Himalayan folding. Instead now of looking upon the folds as due to an overthrust from the north, he regards the corrugations to be the result of an under-creep of the sub-crust towards the north. Thus, according to this view, the Himalaya, instead of being pushed over like a gigantic rock-wave breaking on to the Indian *Horst*, is in reality being dragged away from the old peninsula, the depression between being filled up gradually by the Gangetic alluvium. So far as the purely stratigraphical features are concerned, the effect would be approximately the same whether there is a superficial overthrust of the covering strata or whether there is a deep-seated withdrawal of the basement which is well below the level of observation.

Since the Tibetan expedition of ten years ago we have been in possession of definite facts which show that to the north of the central crystalline axis of the Himalaya there lies a great basin of marine sediments forming a fairly complete record from Palæozoic to Tertiary times, representing the sediments which were laid down in the great central Eurasian ocean to which Suess gave the name *Tethys*. We have thus so far been regarding the central crystalline axis of the Himalaya as approximately coincident with the old northern coast-line of Gondwanaland; but, if Colonel Burrard's ideas be correct, the coast-line must have been very much further to the south before the Himalayan folding began.

Representing what the Geological Survey of India regards as the orthodox view, Mr. H. H. Hayden[25] has directed attention to some conclusions which, from our present geological knowledge, appear to be strange and improbable in Colonel Burrard's conclusions, and he also offers alternative explanations for the admitted geodetic facts. Mr. Hayden suggests, for instance, that the depth of isostatic compensation may be quite different under the Himalayan belt from that under the regions to the south. His assumptions, however, in this respect are, as pointed out by Colonel G. P. Lenox Conyngham,[26] at variance with the whole theory of isostasy. Mr. Hayden then suggests that most of the excessive anomalies would disappear if we took into account the low specific gravity of the sub-Himalayan sands and gravels of Upper Tertiary age as well as of the Pleistocene and recent accumulations of similar material filling the Indo-Gangetic depression. It would not be at all inconsistent with our ideas derived from geology to regard the Gangetic trough as some three or four miles deep near its northern margin, thinning out gradually towards the undisturbed mass of the Indian peninsula, and Mr. R. D. Oldham,[27] with this view, has also calculated the effect of such a wedge of alluvial material of low specific gravity, coming to the conclusion that the rapid change in deflection, on passing from the Lower Himalaya southward towards the peninsula, can mainly be explained by the deficiency of mass in the alluvium itself.

It is obvious that, before seeking for any unusual cause for the gravity anomalies, we ought to take into account the effect of this large body of alluvium which lies along the southern foot of the range. It is, however, by no means certain that a thick mass of alluvial material, accumulated slowly and saturated with water largely charged with carbonate of lime, would have a specific gravity so appreciably lower than that of the rocks now exposed in the main mass of the Himalaya as to account for the residual anomalies. Some of the apparent deficiency in gravity is due to this body of alluvium, but it will only be after critical examination of the data and more precise computation that we shall be in a position to say if there is still room to entertain Colonel Burrard's very interesting hypothesis.

By bringing together the geological and geodetic results we notice five roughly parallel bands stretching across northern India. There is (1) a band of abnormal high gravity lying about 150 miles from the foot of the mountains, detected by the plumb-line and pendulum; (2) the great depression filled by the Gangetic alluvium; (3) the continuous band of Tertiary rock, forming the sub-Himalaya, and separated by a great boundary overthrust from (4) the main mass of the Outer and Central Himalaya of old unfossiliferous rock, with the snow-covered crystalline peaks flanked on the north by (5) the Tibetan basin of highly fossiliferous rocks formed in the great Eurasian mediterranean ocean that persisted up to nearly the end of Mesozoic times.

That these leading features in North India can scarcely be without generic relationship one to another is indicated by the geological history of the area. Until nearly the end of the Mesozoic era the line of crystalline, snow-covered peaks now forming the Central Himalaya was not far from the shore-line between Gondwanaland, stretching away to the south, and Tethys, the great Eurasian ocean. Near the end of Mesozoic times there commenced the great outwelling of the Deccan Trap, the remains of which, after geological ages of erosion, still cover an area of 200,000 square miles, with a thickness in places of nearly 5000 ft. Immediately after the outflow of this body of basic lava, greater in mass than any known eruption of the kind, the ocean flowed into North-West India and projected an arm eastwards to a little beyond the point at which the Ganges now emerges from the hills. Then followed the folding movements that culminated in the present Himalayan range, the elevation developing first on the Bengal side, and extending rapidly to the north-west until the folds extended in a great arc for some 1400 miles from south-east to north-west.

New streams developed on the southern face of the now rising mass, and although the arm of the sea that existed in early Tertiary times became choked with silt, the process of subsidence continued, and the gradually subsiding depression at the foot of the hills as fast as it developed became filled with silt, sand, gravel, and boulders in increasing quantities as the hills became mountains and the range finally reached its present dimensions, surpassing in size all other features of the kind on the face of the globe.

Now, it is important to remember that for ages before the great outburst of Deccan Trap occurred there was a continual unloading of Gondwanaland, and a continual consequent overloading of the ocean beds immediately to the north; that this process went on with a gradual rise on one side and a gradual depression on the other; and that somewhere near and parallel to the boundary line the crust must have been undergoing stresses which resulted in strain, and, as I suggest, the development of those fissures that let loose the floods of Deccan Trap and brought to an end the delicate isostatic balance.

25 "Rec. Geol Surv. Ind.," vol. xliii., part 2, p. 138, 1913.
26 "Records of the Survey of India," vol. v., p. 1.
27 Proc. Roy. Soc., Series A, vol. xc., p. 32, 1914.

During the secular subsidence of the northern shore line of Gondwanaland, accompanied by the slow accumulation of sediment near the shore and the gradual filing away of the land above sea-level, there must have been a gradual creep of the crust in a northerly direction. Near the west end of the Himalayan arc this movement would be towards the north-west for a part of the time; at the east end the creep would be towards the north-north-east and north-east. Thus there would be a tendency from well back in Palæozoic times up to the end of the Cretaceous period for normal faults—faults of tension—to develop on the land, with a trend varying from W.S.W.-E.N.E. to W.N.W.-E.S.E. across the northern part of Gondwanaland. We know nothing of the evidence now pigeon-holed below the great mantle of Gangetic alluvium, while the records of the Himalayan region have been masked or destroyed by later foldings. But in the stratified rocks lying just south of the southern margin of the great alluvial belt we find a common tendency for faults to strike in this way across the present Peninsula of India. These faults have, for instance, marked out the great belt of coalfields stretching for some 200 miles from east to west in the Damuda valley. On this, the east side of India, the fractures of tension have a general trend of W.N.W.-E.S.E. We know that these faults are later than the Permian period, but some of them certainly were not much later.

If now we go westwards across the Central Provinces and Central India into the eastern part of the Bombay Presidency, we find records of this kind still more strikingly preserved; for where the Gondwana rocks, ranging from Permo-Carboniferous to Liassic in age, rest on the much older Vindhyan series, we find three main series of these faults. One series was developed before Permo-Carboniferous times; another traverses the lower Gondwanas, which range up to about the end of Permian times; while the third set affects the younger and Upper Gondwanas of about Rhætic or Liassic age. Although the present topography of the country follows closely the outlines of the geological formations, it is clear from the work of the Geological Survey of India that these outlines were determined in Mesozoic times, and that the movements which formed the latest series of faults were but continuations of those which manifested themselves in Palæozoic times. According to Mr. J. G. Medlicott, the field data showed "that a tendency to yield in general east and west or more clearly north-east and south-west lines existed in this great area from the remote period of the Vindhyan fault." [28] The author of the memoir and map on this area was certainly not suspicious of the ideas of which I am now unburdening my mind; on the contrary, he attempted, and, with apologies, failed to reconcile his facts to views then being pushed by the weight of "authority" in Europe. This was not the last time that facts established in India were found (to use a field-geologist's term) unconformably to lie on a basement of geological orthodoxy as determined by authority in Europe. It is important to notice that the series of faults referred to in the central parts of India are not mere local dislocations, but have a general trend for more than 250 miles.

A fault must be younger, naturally, than the strata which it traverses, but how much younger can seldom be determined. Intrusive rocks of known age are thus often more useful in indicating the age of the fissures through which they have been injected, and consequently the dykes which were formed at the time of the eruption of the great Deccan Trap give another clue to the direction of stresses at this critical time, that is, towards the end of the Cretaceous period, when the northerly creep had reached its maximum, just before Gondwanaland was broken up. If, now, we turn to the geological maps of the northern part of Central India, the Central Provinces, and Bengal, we find that the old Vindhyan rocks of the Narbada valley were injected with hundreds of trap-dykes which show a general W.S.W.-E.N.E. trend, and thus parallel to the normal tension faults, which we know were formed during the periods preceding the outburst of the Deccan Trap. This general trend of faults and basic dykes is indicated on many of the published geological maps of India covering the northern part of the peninsula, including Ball's maps of the Ramgarh and Bokaro coalfields [29] and of the Hutar coalfield, [30] Hughes's Rewa Gondwana basin, [31] Jones's southern coalfields of the Satpura basin, [32] and Oldham's general map of the Son valley. [33]

We see, then, that the development of fissures with a general east-west trend in the northern part of Gondwanaland culminated at the end of the Cretaceous period, when they extended down, probably, to the basic magma lying below the crust either in a molten state, or in a state that would result in fluxion on the relief of pressure. That the molten material came to the surface in a superheated and liquid condition is shown by the way in which it has spread out in horizontal sheets over such enormous areas. Throughout this great expanse of lava there are no certain signs of volcanic centres no conical slopes around volcanic necks; and one might travel for more than 400 miles from Poona to Nagpur over sheets of lava which are still practically horizontal. There is nothing exactly like this to be seen elsewhere to-day. The nearest approach to it is among the Hawaiian calderas, where the highly mobile basic lavas also show the characters of superfusion, glowing, according to J. D. Dana, [34] with a white heat, that is, at a temperature not less than about 1300° C.

Mellard Reade has pointed out that the earth's crust is under conditions of stress analogous to those of a bent beam, with, at a certain depth, a "level of no strain." Above this level there should be a shell of compression, and under it a thicker shell of tension. The idea has been treated mathematically by C. Davison, G. H. Darwin, O. Fisher, and M. P. Rudski, and need not be discussed at present. Prof. R. A. Daly has taken advantage of this view concerning the distribution of stresses in the crust to explain the facility for the injection of dykes and batholiths from the liquid, or potentially liquid, gabbroid magma below into the shell of tension. [35] He also shows that the injection of large bodies of basic material into the shell of tension tends on purely mechanical grounds to the formation of a depression, or geosyncline. If this be so, are we justified in assuming that the heavy band following the southern margin of the Gangetic geosyncline is a "range" of such batholiths? The idea is not entirely new; for O. Fisher made the suggestion more than twenty years ago that the abnormal gravity at Kalianpur was due to "some peculiar influence (perhaps of a volcanic neck of basalt)." [36]

Daly's suggestion, however, taken into account with the history of Gondwanaland, may explain the peculiar alignment of the heavy subterranean band, parallel to the Gangetic depression and parallel to the general trend of the peninsular tension-faults and fissures that

28 "Mem. Geol. Surv. Ind.," vol. ii., 1860, part 2, p. 256.

29 *Ibid.*, vol. vi., part 2.
30 *Ibid.*, vol. xv.
31 *Ibid.*, vol. xxi., part 3.
32 *Ibid.*, vol. xxiv.
33 *Ibid.*, vol. xxxi., part 1.
34 "Characteristics of Volcanoes," 1891. p. 200.
35 R. A. Daly, "Abyssal Igneous Injection as a Causal Condition and as an Effect of Mountain Building," *Amer. Journ. Sci.*, xxii., September, 1906, p. 205.
36 "Physics of the Earth's Crust," 2nd ed., 1889, p. 216.

followed the unloading of Gondwanaland and the heavy loading of the adjoining ocean bed along a band roughly parallel to the present Himalayan folds.

R. S. Woodward objected that isostasy does not seem to meet the requirements of geological continuity, for it tends rapidly towards stable equilibrium, and the crust ought therefore to reach a stage of repose early in geologic time.[37] If the process of denudation and rise, with adjoining deposition and subsidence, occurred on a solid globe, this objection might hold good. But it seems to me that the break-up of Gondwanaland and the tectonic revolutions that followed show how isostasy can defeat itself in the presence of a subcrustal magma actually molten or ready to liquefy on local relief of pressure. It is possible that the protracted filing off of Gondwanaland brought nearer the surface what was once the local level of no-strain and its accompanying shell of tension.

The conditions existing in northern Gondwanaland before late Mesozoic times must have been similar to those in south-west Scotland before the occurrence of the Tertiary eruptions, for the crust in this region was also torn by stresses in the S.W.–N.E. direction with the formation of a remarkable series of N.W.–S.E. dykes which give the 1-in. geological maps in this region a regularly striped appearance.

There is no section of the earth's surface which one can point to as being now subjected to exactly the same kind and magnitude of treatment as that to which Gondwanaland was exposed for long ages before the outburst of the Deccan Trap; but possibly the erosion of the Brazilian highlands and the deposition of the silt carried down by the Amazon, with its southern tributaries, and by the more eastern Araguay and Tocantins, may result in similar stresses which, if continued, will develop strains, and open the way for the subjacent magma to approach the surface or even to become extravasated, adding another to the small family of so-called fissure-eruptions.

The value of a generalisation can be tested best by its trustworthiness as a basis for prediction. Nothing shows up the shortcomings of our knowledge about the state of affairs below the superficial crust so effectually as our inability to make any useful predictions about earthquakes or volcanic eruptions. For many years to come in this department of science the only worker who will ever establish a claim to be called a prophet will be one in Cicero's sense—"he who guesses well."

SECTION D.

ZOOLOGY.

OPENING ADDRESS BY PROF. ARTHUR DENDY, D.Sc., F.R.S., PRESIDENT OF THE SECTION.

Progressive Evolution and the Origin of Species.

THE opening years of the present century have witnessed a remarkable development of biology as an experimental science, a development which, however full of promise it may be for the future, for the time being appears to have resulted in a widespread disturbance of ideas which have themselves only recently succeeded in gaining general acceptance. The theory of organic evolution, plainly enough enunciated at the close of the eighteenth and the beginning of the nineteenth century by Buffon, Lamarck, and Erasmus Darwin, remained unconvincing to the great majority of thinking men until the genius of Charles Darwin not only brought together and presented the evidence in such a manner that it could no longer be ignored, but elaborated a logical explanation of the way in which organic evolution might be supposed to have taken place. Thanks to his labours and those of Alfred Russel Wallace, supported by the powerful influence of such men as Huxley and Hooker, the theory was placed upon a firm foundation, in a position which can never again be assailed with any prospect of success.

This statement is, I believe, entirely justified with regard to the theory of organic evolution itself, but the case is very different when we come to investigate the position of the various subsidiary theories which have been put forward from time to time with regard to what may perhaps be termed the *modus operandi*, the means by which organic evolution has been effected. It is in this field that controversy rages more keenly than ever before. Lamarck told us that evolution was due to the accumulated results of individual effort in response to a changing environment, and also to the direct action of the environment upon the organism. Darwin and Wallace taught us that species originated by the natural selection of favourable variations, and under the influence of Weismann's doctrine of the non-inheritance of acquired characters the theory of natural selection is in danger of becoming crystallised into an inflexible dogma. In recent years De Vries has told us that species arise by sudden mutations, and not by slow successive changes, while one of the most extreme exponents of "Mendelism," Prof. Lotsy, lately informed us that all species arise by crossing, and seriously suggested that the vertebrate type arose by the crossing of two invertebrates!

This curious and many-sided divergence of opinion amongst expert biologists is undoubtedly largely due to the introduction of experimental methods into biological science. Such methods have proved very fruitful in results which at first sight seem to be mutually contradictory, and each group of workers has built up its own theory mainly on the basis of observations in its own restricted field.

Prof. Bateson has said in his recently published "Problems of Genetics": "When . . . we contemplate the problem of evolution at large the hope at the present time of constructing even a mental picture of that process grows weak almost to the point of vanishing. We are left wondering that so lately men in general, whether scientific or lay, were so easily satisfied. Our satisfaction, as we now see, was chiefly founded on ignorance."[1]

In view of this striking pronouncement on the part of one who has devoted his life with signal success to the experimental investigation of evolutionary problems, the remarks which I propose to lay before you for your consideration to-day may well appear rash and ill-advised. I cannot believe, however, that the position is really quite so black as it is painted. We must perforce admit that the divers theories with regard to the working of organic evolution cannot all be correct in all their details, but it may be that each contains its own elements of truth, and that if these elements can but be recognised and sorted out, they may perhaps be recombined in such a form as to afford at any rate a plausible working hypothesis. We must bear in mind from the outset that in dealing with such a complex problem many factors have to be taken into account, and that widely different views on the question may be merely one-sided and not necessarily mutually exclusive.

I take it there are three principal facts, or groups of facts, that have to be accounted for by any theory of organic evolution:—

(1) The fact that, on the whole, evolution has taken

37 "Address to the Sect. of Mathematics and Astronomy of the Amer. Assoc.," 1889. Smithsonian Report, 1890, p. 196.

1 "Problems of Genetics," p. 97.

place in a progressive manner along definite and divergent lines.

(2) The fact that individual animals and plants are more or less precisely adapted in their organisation and in their behaviour to the conditions under which they have to live.

(3) The fact that evolution has resulted in the existence on the earth to-day of a vast number of more or less well-defined groups of animals and plants which we call species.

The first of these facts appears to me to be the most fundamental, and at the same time the one to which least attention is usually paid. The great question, after all, is, Why do organisms progress at all instead of remaining stationary from generation to generation? To answer this question it is not necessary to go back to the beginning and consider the case of the first terrestrial organisms, whatever they may have been, nor are we obliged to take as illustrations the lowest organisms known to us as existing at the present day. We may consider the problem at any stage of evolution, for at each stage progress is, or may be, still taking place. We may even begin by considering what is usually regarded as the highest stage of all, man himself; and indeed this seems the most natural thing to do, for we certainly know more about the conditions of progress in man than in any other organism. I refer, of course, at the moment, not to progress in bodily organisation, but to progress in the ordinary sense of the word, the progress, say, of a family which rises in the course of a few generations from a position of obscure poverty to one of wealth and influence. You may perhaps say that such a case has no bearing upon the problem of organic evolution in a state of nature, and that we ought to confine our attention to the evolution of bodily structure and function. If so, I must reply that you have no right to limit the meaning of the term evolution in this manner; the contrast between man and nature is purely arbitrary; man is himself a living organism, and all the improvements that he effects in his own condition are part of the progress of evolution in his particular case. At any rate I must ask you to accept this case as our first illustration of a principle that may be applied to organisms in general.

If we inquire into the cause of the progress of our human family I think there can be only one answer—it is due to the accumulation of capital, or, as I should prefer to put it, to the accumulation of potential energy, either in the form of material wealth or of education. What one generation saves is available for the next, and thus each succeeding generation gets a better start in life, and is able to rise a little higher than the preceding one.

Every biologist knows, of course, that there are many analogous cases amongst the lower animals, and also amongst plants. The accumulation of food-yolk in the egg has undoubtedly been one of the chief factors in the progressive evolution of animals, although it has been replaced in the highest forms by a more effective method of supplying potential energy to the developing offspring. It may indeed be laid down as a general law that each generation, whether of animals or of plants, accumulates more energy than it requires for its own maintenance, and uses the surplus to give the next generation a start in life. There is every reason to believe that this has been a progressive process throughout the whole course of evolution, for the higher the degree or organisation the more perfect do we find the arrangements for securing the welfare of the offspring.

We cannot, of course, trace this process back to its commencement, because we know nothing of the nature of the earliest living things, but we may pause for a moment to inquire whether any phenomena occur amongst simple unicellular organisms that throw any light upon the subject. What we want to know is—How did the habit of accumulating surplus energy and handing it on to the next generation first arise?

Students of Prof. H. S. Jennings's admirable work on the "Behaviour of the Lower Organisms" will remember that his experiments have led him to the conclusion that certain Protozoa, such as Stentor, are able to learn by experience how to make prompt and effective responses to certain stimuli; that after they have been stimulated in the same way a number of times they make the appropriate response at once without having to go through the whole process of trial and error by which it was first attained. In other words, they are able by practice to perform a given action with less expenditure of energy. Some modification of the protoplasm must take place which renders the performance of an act the easier the oftener it has been repeated. The same is, of course, true in the case of the higher animals, and we express the fact most simply by saying that the animal establishes habits. From the mechanistic point of view we might say that the use of the machine renders it more perfect and better adapted for its purpose. In the present state of our knowledge I think we cannot go beyond this, but must content ourselves with recognising the power of profiting by experience as a fundamental property of living protoplasm.

It appears to me that this power of profiting by experience lies at the root of our problem, and that in it we find a chief cause of progressive evolution. Jennings speaks of the principle involved here as the "Law of the readier resolution of physiological states after repetition," and, similarly, I think we must recognise a "Law of the accumulation of surplus energy" as resulting therefrom. Let us look at the case of the accumulation of food-yolk by the egg-cell a little more closely from this point of view. Every cell takes in a certain amount of potential energy in the form of food for its own use. If it leads an active life, either as an independent organism or as a constituent part of an organism, it may expend by far the greater part, possibly even the whole, of that energy upon its own requirements, but usually something is left over to be handed down to its immediate descendants. If, on the other hand, the cells exhibits very little activity and expends very little energy, while placed in an environment in which food is abundant, it will tend to accumulate surplus energy in excess of its own needs. Such is the case with the egg-cells of the multicellular animals and plants. Moreover, the oftener the process of absorbing food-material is repeated the easier does it become; in fact, the egg-cell establishes a habit of storing up reserve material or food-yolk. Inasmuch as it is a blastogenic character, there can be no objection to the supposition that this habit will be inherited by future generations of egg-cells. Indeed we are obliged to assume that this will be the case, for we know that the protoplasm of each succeeding generation of egg-cells is directly continuous with that of the preceding generation. We thus get at any rate a possibility of the progressive accumulation of potential energy in the germ-cells of successive generations of multicellular organisms, and, of course, the same argument holds good with regard to successive generations of Protista.

It would seem that progressive evolution must follow as a necessary result of the law of the accumulation of surplus energy in all cases where there is nothing to counteract that law, for each generation gets a better start than its predecessor, and is able to carry on a little further its struggle for existence with the environment. It may be said that this argument proves too much, that if it were correct all organisms would by this time have attained to a high degree of organisation, and that at any rate we should not expect to find such simple organisms as bacteria and Amœbæ still surviving. This objection, which, of course, applies equally to other theories of organic evolution, falls to the ground when we consider that there must be many factors of which we know nothing which may prevent the establishment of progressive habits and render impossible the accumulation of surplus energy. Many of the lower organisms, like many human beings, appear to have an inherent incapacity for progress, though it may be quite impossible for us to say to what that incapacity is due.

It will be observed that in the foregoing remarks I have concentrated attention upon the storing up of reserve material by the egg-cells, and in so doing have avoided the troublesome question of the inheritance of so-called acquired characters. I do not wish it to be supposed, however, that I regard this as the only direction in which the law of the accumulation of surplus energy can manifest itself, for I believe that the accumulation of surplus energy by the body may be quite as important as a factor in progressive evolution as the corresponding process in the germ-cells themselves. The parents, in the case of the higher animals, may supply surplus energy, in the form of nutriment or otherwise, to the offspring at all stages of its development, and the more capital the young animal receives the better will be its chances in life, and the better those of its own offspring.

In all these processes, no doubt, natural selection plays an important part, but, in dealing with the accumulation of food material by the egg-cells, one of my objects has been to show that progressive evolution would take place even if there were no such thing as natural selection, that the slow successive variations in this case are not chance variations, but due to a fundamental property of living protoplasm and necessarily cumulative.

Moreover, the accumulation of surplus energy in the form of food-yolk is only one of many habits which the protoplasm of the germ-cells may acquire in a cumulative manner. It may learn by practice to respond with increased promptitude and precision to other stimuli besides that of the presence of nutrient material in its environment. It may learn to secrete a protective membrane, to respond in a particular manner to the presence of a germ-cell of the opposite sex, and to divide in a particular manner after fertilisation has taken place.

Having thus endeavoured to account for the fact that progressive evolution actually occurs by attributing it primarily to the power possessed by living protoplasm of learning by experience and thus establishing habits by which it is able to respond more quickly to environmental stimuli, we have next to inquire what it is that determines the definite lines along which progress manifests itself.

Let us select one of these lines and investigate it as fully as the time at our disposal will permit, with the view of seeing whether it is possible to formulate a reasonable hypothesis as to how evolution may have taken place. Let us take the line which we believe has led up to the evolution of air-breathing vertebrates. The only direct evidence at our disposal in such a case is, of course, the evidence of palæontology, but I am going to ask you to allow me to set this evidence, which, as you know, is of an extremely fragmentary character, aside, and base my remarks upon the ontogenetic evidence, which, although indirect, will, I think, be found sufficient for our purpose. One reason for concentrating our attention upon this aspect of the problem is that I wish to show that the recapitulation of phylogenetic history in individual development is a logical necessity if evolution has really taken place.

We may legitimately take the nucleated Protozoon cell as our starting point, for, whatever may have been the course of evolution that led up to the cell, there can be no question that all the higher organisms actually start life in this condition.

We suppose, then, that our ancestral Protozoon acquired the habit of taking in food material in excess of its own requirements, and of dividing into two parts whenever it reached a certain maximum size. Here again we must, for the sake of simplicity, ignore the facts that even a Protozoon is by no means a simple organism, and that its division, usually at any rate, is a very complicated process. Each of the daughter-cells presently separates from its sister-cell and goes its own way as a complete individual, still a Protozoon. It seems not improbable that the separation may be due to the renewed stimulus of hunger, impelling each cell to wander actively in search of food. In some cases, however, the daughter-cells remain together and form a colony, and probably this habit has been rendered possible by a sufficient accumulation of surplus energy in the form of food-yolk on the part of the parent rendering it unnecessary for the daughter-cells to separate in search of food at such an early date. One of the forms of colony met with amongst existing Protozoa is the hollow sphere, as we see it, for example, in Sphærozoum and Volvox, and it is highly probable that the assumption of this form is due largely, if not entirely, to what are commonly called mathematical causes, though we are not in a position to say exactly what these causes may be. The widespread occurrence of the blastosphere or blastula stage in ontogeny is a sufficiently clear indication that the hollow, spherical Protozoon colony formed a stage in the evolution of the higher animals.

By the time our ancestral organism has reached this stage, and possibly even before, a new complication has arisen. The cells of which the colony is composed no longer remain all alike, but become differentiated, primarily into two groups, which we distinguish as somatic-cells and germ-cells respectively.

From this point onwards evolution ceases to be a really continuous process, but is broken up into a series of ontogenies, at the close of each of which the organism has to go back and make a fresh start in the unicellular condition, for the somatic cells sooner or later become exhausted in their conflict with the environment and perish, leaving the germ-cells behind to take up the running. That the germ-cells do not share the fate of the somatic cells must be attributed to the fact that they take no part in the struggle for existence to which the body is exposed. They simply multiply and absorb nutriment under the protection of the body, and therefore retain their potential energy unimpaired. They are in actual fact, as is so often said, equivalent to so many protozoa, and, like the protozoa, are endowed with a potential immortality.

We know that, if placed under suitable conditions, or in other words, if exposed to the proper environmental stimuli, these germ-cells will give rise to new organisms, like that in the body of which they were formerly enclosed. One of the necessary conditions is, with rare exceptions, the union of the germ-cells in pairs to form zygotes or fertilised ova; but I propose, in the first instance, for the sake of simplicity, to leave out of account the existence of the sexual process and the results that follow therefrom, postponing the consideration of these to a later stage of our inquiry. I wish, moreover, to make it quite clear that organic evolution must have taken place if no such event as amphimixis had ever occurred.

What, then, may the germ-cells be expected to do?

How are they going to begin their development? In endeavouring to answer this question we must remember that the behaviour of an organism at any moment depends upon two sets of factors—the nature of its own constitution on one hand, and the nature of its environment on the other. If these factors are identical for any two individual organisms, then the behaviour of these two individuals must be the same. If the germ-cells of any generation are identical with those of the preceding generation, and if they develop under identical conditions, then the soma of the one generation must also be identical with that of the other.[2] Inasmuch as they are parts of the same continuous germ-plasm—leaving out of account the complications introduced by amphimixis—we may assume that the germ-cells of the two generations are indeed identical in nearly every respect; but there will be a slight difference, due to the fact that those of the later generation will have inherited a rather larger supply of initial energy and a slightly greater facility for responding to stimuli of various kinds, for the gradual accumulation of these properties will have gone a stage further. The environment also will be very nearly identical in the two cases, for we know from experiment that if it were not the organism could not develop at all.

Throughout the whole course of its ontogeny the organism must repeat with approximate accuracy the stages passed through by its ancestors, because at every stage there will be an almost identical organism exposed to almost identical stimuli. We may, however, expect an acceleration of development and a slight additional progress at the end of ontogeny as the result of the operation of the law of the accumulation of surplus energy and of the slightly increased facility in responding to stimuli. The additional progress, of course, will probably be so slight that from one generation to the next we should be quite unable to detect it, and doubtless there will be frequent backslidings due to various causes.

We can thus formulate a perfectly reasonable explanation of how it is that the egg first undergoes segmentation and then gives rise to a blastula resembling a hollow protozoon colony; it does so simply because at every stage it must do what its ancestors did under like conditions. We can also see that progressive evolution must follow from the gradual accumulation of additions at the end of each ontogeny, these additions being rendered possible by the better start which each individual gets at the commencement of its career.

Let us now glance for a moment at the next stage in phylogeny, the conversion of the hollow spherical protozoon colony into the cœlenterate type of organisation, represented in ontogeny by the process of gastrulation. Here again it is probable that this process is explicable to a large extent upon mechanical principles. According to Rhumbler,[3] the migration of endoderm cells into the interior of the blastula is partly due to chemotaxis and partly to changes of surface tension, which decreases on the inner side of the vegetative cells owing to chemical changes set up in the blastocœl fluid.

We may, at this point, profitably ask the question, Is the endoderm thus formed an inherited feature of the organism? The material of which it is composed is, of course, derived from the egg-cell continuously by repeated cell-division, but the way in which that material is used by the organism depends upon the environment, and we know from experiment that modifications of the environment actually do produce corresponding modifications in the arrangement of the material. We know, for example, that the addition of salts of lithium to the water in which certain embryos are developing causes the endoderm to be protruded instead of invaginated, so that we get a kind of inside-out gastrula, the well-known lithium larva.

It appears, then that an organism really inherits from its parents two things: (1) a certain amount of protoplasm loaded with potential energy, with which to begin operations, and (2) an appropriate environment. Obviously the one is useless without the other. An egg cannot develop unless it is provided with the proper environment at every stage. Therefore, when we say that an organism inherits a particular character from its parents, all we mean is that it inherits the power to produce that character under the influence of certain environmental stimuli.[4] The inheritance of the environment is of at least as much importance as the inheritance of the material of which the organism is composed. The latter, indeed, is only inherited to a very small extent, for the amount of material in the egg-cell may be almost infinitesimal in comparison with the amount present in the adult, nearly the whole of which is captured from the environment and assimilated during ontogeny.

From this point of view the distinction between somatogenic and blastogenic characters really disappears, for all the characters of the adult organism are acquired afresh in each generation as a result of response to environmental stimuli during development. This is clearly indicated by the fact that you cannot change the stimuli without changing the result.

Time forbids us to discuss the phylogenetic stages through which the cœlenterate passed into the cœlomate type, the cœlomate into the chordate, and the chordate into the primitive vertebrate. We must admit that as yet we know nothing of the particular causes that determined the actual course of evolution at each successive stage. What we do know, however, about the influence of the environment, both upon the developing embryo and upon the adult, is sufficient to justify us in believing that every successive modification must have been due to a response on the part of the organism to some environmental change. Even if the external conditions remained practically identical throughout long periods of time, we must remember that the internal conditions would be different in each generation, because each generation starts with a slightly increased capital and carries on its development a little further under internal conditions modified accordingly.

At this point it may be asked, Is the response to environmental stimuli a purely mechanical one, and, if so, how can we account for the fact that at every stage in its evolution the organism is adapted to its environment? We shall have to return to this question later on, but it may be useful to point out once more that there is good reason to believe—especially from the experimental work of Jennings—that the response of even a unicellular organism to stimuli is to a large extent purposive; that the organism learns by experience, by a kind of process of trial and error, how to make the response most favourable to itself under any given change of conditions; in other words, that the organism selects those modes of response that are most conducive to its own well-being. Under the term response to stimuli we must, of course, include those responses of the living protoplasm which result in modifications of bodily structure, and hence the evolution of bodily structure will, on the whole, be

[2] This is, of course, a familiar idea. Compare Driesch, "Gifford Lectures," 1907, p. 214.

[3] Quoted by Przibram, "Experimental Zoology," English Trans., Part i., p. 47.

[4] Compare Dr. Archdall Reid's suggestive essay on "Biological Terms" (*Bedrock*, January, 1914).

of an adaptive character and will follow definite lines There is good reason for believing, however, that many minor modifications in structure may arise and persist, incidentally as it were, that have no significance as adaptations.

One of the most remarkable and distinctive features of the lower vertebrates is the presence of gill-slits as accessory organs of respiration. These gill-slits are clearly an adaptation to aquatic life. When the ancestors of the higher vertebrates left the water and took to life on land the gills disappeared and were replaced by lungs, adapted for air-breathing. The change must, of course, have been an extremely gradual one, and we get a very clear indication of how it took place in the surviving dipnoids, which have remained in this respect in an intermediate condition between the fishes and the amphibia, possessing and using both gills and lungs.

We also know that even the most highly specialised air-breathing vertebrates, which never live in water and never require gills or gill-slits at all, nevertheless possess very distinct gill-slits during a certain period of their development. This is one of the most familiar illustrations of the law of recapitulation, and my only excuse for bringing it forward now is that I wish, before going further, to consider a difficulty—perhaps more apparent than real—that arises in connection with such cases.

It might be argued that if gill-slits arose in response to the stimuli of aquatic life, and if these stimuli are no longer operative in the case of air-breathing vertebrates, then gill-slits ought not to be developed at any stage of their existence. This argument is, I think, fully met by the following considerations.

At any given moment of ontogenetic development the condition of any organ is merely the last term of a series of morphogenetic stages, while its environment at the same moment—which, of course, includes its relation to all the other organs of the body—is likewise merely the last term of a series of environmental stages. We have thus two parallel series of events to take into consideration in endeavouring to account for the condition of any part of an organism —or of the organism as a whole—at any period of its existence:—

$E_1\ E_2\ E_3\ \ldots\ \ldots\ \ldots\ E_n$ environmental stages
$M_1\ M_2\ M_3\ \ldots\ \ldots\ \ldots\ M_n$ morphogenetic ,,

Ontogeny is absolutely conditioned by the proper correlation of the stages of these two series at every point, and hence it is that any sudden change of environment is usually attended by disastrous consequences. Thus, after the fish-like ancestors of air-breathing vertebrates had left the water and become amphibians, they doubtless still had to go back to the water to lay their eggs, in order that the eggs might have the proper conditions for their development.

Obviously the environment can only be altered with extreme slowness, and one of the first duties of the parent is to provide for the developing offspring conditions as nearly as possible identical with those under which its own development took place. It is, however, inevitable that, as phylogenetic evolution progresses, the conditions under which the young organism develops should change. In the first place, the mere tendency to acceleration of development, to which we have already referred, must tend to dislocate the correlation between the ontogenetic series and the environmental series. Something of this kind seems to have taken place in the life-cycle of many hydrozoa, resulting in the suppression of the free medusoid generation and the gradual degeneration of the gonophore. But it is probably in most cases change in the environment of the adult that is responsible for such dislocation.

To return to the case of the amphibians. At the present day some amphibians, such as the newts and frogs, still lay their eggs in water, while the closely related salamanders retain them in the oviducts until they have developed into highly organised aquatic larvæ, or even what is practically the adult condition. Kammerer has shown that the period at which the young are born can be varied by changing the environment of the parent. In the absence of water the normally aquatic larvæ of the spotted salamander may be retained in the oviduct until they have lost their gills, and they are then born in the fully-developed condition, while, conversely, the alpine salamander, of which the young are normally born in the fully-developed state, without gills, may be made to deposit them prematurely in water in the larval, gill-bearing condition.

There can be no doubt that the ancestral amphibians laid their eggs in water in a completely undeveloped condition. The habit of retaining them in the body during their development must have arisen very gradually in the phylogenetic history of the salamanders, the period for which the young were retained growing gradually longer and longer. It is obvious that this change of habit involves a corresponding change in the environmental conditions under which the young develop, and in cases in which the young are not born until they have reached practically the adult condition this change directly affects practically the whole ontogeny. We may say that the series

$E_1\ E_2\ E_3\ \ldots\ \ldots\ \ldots\ E_n$ has become
$E_1'\ E_2'\ E_3'\ \ldots\ \ldots\ \ldots\ E_n'$

and as the change of environment must produce its effect upon the developing organism the series

$M_1\ M_2\ M_3\ \ldots\ \ldots\ \ldots\ M_n$ will have become
$M_1'\ M_2'\ M_3'\ \ldots\ \ldots\ \ldots\ M_n'$

We must remember that throughout the whole course of phylogenetic evolution this series is constantly lengthening, so that what was the adult condition at one time becomes an embryonic stage in future generations, and the series thus represents not only the ontogeny, but also, though in a more or less imperfect manner, the phylogeny of the organism.

The character of each stage in ontogeny must depend upon (1) the morphological and physiological constitution of the preceding stage, and (2) the nature of the environment in which development is taking place. We cannot, however, distinguish sharply between those two sets of factors, for, in a certain sense, the environment gradually becomes incorporated in the organism itself as development proceeds, each part contributing to the environment of all the remainder, and the influence of this internal portion of the environment ever becoming more and more important.

The whole process of evolution depends upon changes of environment taking place so gradually that the necessary self-adjustment of the organism at every stage is possible. In the case of our amphibia the eggs could possibly undergo the first stages of development, the preliminary segmentation, within the oviduct of the parent just as well as in the water, for in both cases they would be enclosed in their envelopes, and the morphological differences between the early stages in the two cases might be expected to be quite insignificant. But it must be the same at each term of the series, for each term is built upon the foundation of the preceding one, and the whole process takes place by slow and imperceptible degrees.

It is true that by the time we reach the formation of the vestigial gill-slits in the embyro of one of the higher vertebrates the environmental conditions are very different from those under which gill-slits were developed in their aquatic ancestors. But what then?

Are not the gill-slits also very different? The changed environment has had its effect. The gills themselves are never developed, and the gill-slits never become functional; moreover, they disappear completely at later stages of development, when the conditions of life become still more different and their presence would be actually detrimental to their possessor. The embyro with the vestigial gill-slits is, as a whole, perfectly well adapted to its environment, though the gill-slits themselves have ceased to be adaptive characters. They still appear because the environmental conditions, and especially the internal conditions, which have now become far more important than the external ones, are still such as to cause them to do so.

I think the chief difficulty in forming a mental picture of the manner in which evolution has taken place, and especially in accounting for the phenomenon of recapitulation in ontogeny, which is merely another aspect of the same problem, arises from attempting to take in too much at once. There is no difficulty in understanding how any particular stage is related to the corresponding stage in the previous generation, and the whole series of stages, whether looked at from the ontogenetic or from the phylogenetic point of view, can be nothing else but the sum of its successive terms.

It will be convenient, before going further, to sum up the results at which we have so far arrived from the point of view of the theory of heredity. We have as yet seen no reason to distinguish between somatogenic and blastogenic characters. All the characters of the adult animal are acquired during ontogeny as the result of the reaction of the organism to environmental stimuli, both internal and external. All that the organism actually inherits is a certain amount of protoplasm—endowed with a certain amount of energy—and a certain sequence of environmental conditions. In so far as these are identical in any two successive generations the final result must be identical also, the child must resemble the parent; in so far as they are different the child will differ from the parent, but the differences in environment cannot be very great without preventing development altogether.

So far, it is clear, there has been no need to think of the germ-cells as the bearers of material factors or determinants that are responsible for the appearance of particular characters in the adult organism; nor yet to suppose that they are, to use the phraseology of the mnemic theory of heredity, charged with the memories of past generations. They have been regarded as simple protoplasmic units, and the entire ontogeny has appeared as the necessary result of the reaction between the organism and its environment at each successive stage of development. This cannot, however, be a complete explanation of ontogeny, for if it were we should expect all eggs, when allowed to develop under the same conditions from start to finish, to give rise to the same adult form, and this we know is not the case. We know also, from observation and experiment, that the egg is in reality by no means a simple thing but an extremely complex one, and that different parts of the egg may be definitely correlated with corresponding parts of the adult body. It has been demonstrated in certain cases that the egg contains special organ-forming substances definitely located in the cytoplasm, and that if these are removed definite parts of the organism into which the egg develops will be missing. We know, also, that the nucleus of the germ-cell of either sex contains—at any rate, at certain periods—a number of perfectly well-defined bodies, the chromosomes, and these also have been definitely correlated in certain cases with special features of the adult organisation.

Before we can hope to complete our mental picture of the manner in which organic evolution has taken place, if only in outline, it is evident that we must be able to account for the great complexity of structure which the germ-cells themselves have managed to acquire, and also to form some idea of the effect of this complication upon the development of both the individual and the race.

We must consider the origin of cytoplasmic and nuclear complications of the egg separately, for they appear to be due fundamentally to two totally distinct sets of factors. In the first place we have to remember that during oogenesis the egg-cell grows to a relatively large size by absorbing nutrient material from the body in which it is enclosed. It is this nutrient material that is used for building up the deutoplasm or food-yolk. There is good reason for believing that the character of this nutrient material will change, during the course of evolution, *pari passu* with the changing character of the organism by which it is supplied. Doubtless the change is of a chemical nature, for we know from precipitin experiments that the body fluids of closely allied species, or even of the two sexes of the same species, do exhibit distinctly recognisable differences in chemical composition. It also appears highly probable, if not certain, from such experiments as those of Agar upon Simocephalus, that substances taken in with the food, which bring about conspicuous modifications of bodily structure, may at the same time be absorbed and stored up by the egg-cells so as to bring about corresponding changes in the adults into which the eggs develop.

There seems therefore to be no great difficulty in comprehending, at any rate in a general way, how the egg may become the repository of definite chemical substances, organ-forming substances if we like to call them so, possibly to be classed with the hormones and enzymes, which will influence the development in a particular manner as soon as the appropriate conditions arise.

Unfortunately, time will not allow of our following up this line of thought on the present occasion, but we may notice, before passing on, that with the accumulation of organ-forming substances in the egg we have introduced the possibility of changes in bodily structure, to whatever cause they may be due, being represented by correlated modifications in the germ-cells, and this is doubtless one of the reasons why the germ-cells of different animals are not all alike with regard to their potentialities of development.[5]

We now come to the question of how the nucleus of the germ-cell acquired its great complexity of structure. We are not concerned here with the origin of the differentiation into nucleus and cytoplasm and the respective parts played by the two in the life of the cell. The problem which we have to consider is the complication introduced by the sexual process, by the periodically recurring union of the germ-cells in pairs, or, as Weismann has termed it, amphimixis. This is well known to be essentially a nuclear phenomenon, in which the so-called chromatin substance is especially concerned, and it is a phenomenon which must have made its appearance at a very early stage of evolution, for it is exhibited in essentially the same manner alike in the higher plants and animals and in unicellular organisms.

Let us suppose, for the sake of argument, that when amphimixis first took place the chromatin of each germ-cell was homogeneous, but that it differed sightly in different germ cells of the same species as a result of exposure to slightly different conditions during its past history. What would be likely to happen when two different samples of chromatin came

5 Compare Cunningham's "Hormone Theory" of Heredity ("Archiv für Entwicklungsmechanik der Organismen," Bd. xxvi., Heft 3).

together in the zygote? The result would surely depend upon the interaction of the complex colloidal multimolecules of which the chromatin is composed. Various possibilities would arise. (1) The two samples might differ in such a way as to act as poisons to one another, disturbing each other's molecular equilibrium to such an extent that neither could survive. This is possibly what happens when an ovum is fertilised by a spermatozoon of a distinct species, though there are, of course, exceptions. (2) They might be so alike as to be able to amalgamate more or less completely, so that there would simply be an increase of chromatin of possibly more or less modified constitution. (3) They might continue to exist side by side, each maintaining its own individual character.

In the third case the union of the two different samples would give rise to a mass of chromatin of twofold nature, and repetition of the process from generation to generation would, as Weismann has shown, result in ever-increasing heterogeneity, until the chromatin came to consist of a great number of different concrete particles, each of which might conceivably differ from all the others. But when two heterogeneous masses of chromatin meet in the zygote there may be all sorts of mutual attractions and repulsions between the different colloidal multimolecules, for all three of our supposed cases may arise simultaneously, and thus the results may become extremely complicated.

The chromatin of the germ-cells in all existing organisms is undoubtedly heterogeneous, and this heterogeneity may be to some extent visibly expressed in its arrangement in more or less multiform chromosomes during mitosis. We may provisionally accept Weismann's view that these chromosomes are themselves heterogeneous, being composed of chromomeres or ids, which in their turn are composed of determinants.

All this complexity of structure may be attributed to the effects of oft-repeated amphimixis, a view which is supported in the most striking manner by the fact that the nucleus in all ordinary somatic cells (in animals and in the diploid generation of plants) has a double set of chromosomes, one derived from the male and the other from the female parent, and by the well-known phenomenon of chromatin reduction which always precedes amphimixis.

When we approach the problem of heredity from the experimental side we get very strong evidence of the existence in the germ-plasm of definite material substances associated with the inheritance of special characters. Mendelian workers generally speak of these substances as factors, but the conception of factors is evidently closely akin to that of Weismann's hypothetical determinants. The cytological evidence fits in very well with the view that the factors in question may be definite material particles, and it is quite possible that such particles may have a specific chemical constitution to which their effects upon the developing organism are due.

From our point of view the interesting thing is the possibility that arises through the sexual process of the permutation and combination of different factors derived from different lines of descent. A germ-cell may receive additions to its collection of factors or be subject to subtractions therefrom, and in either case the resulting organism may be more or less conspicuously modified.

By applying the method of experimental hybridisation a most fruitful and apparently inexhaustible field of research has been opened up in this direction, in the development of which no one has taken a more active part than the present President of the British Association. There cannot be the slightest doubt that a vast number of characters are inherited in what is called the Mendelian manner, and, as they are capable of being separately inherited and interchanged with others by hybridisation, we are justified in believing that they are separately represented in the germ-cells by special factors. Important as this result is, I believe that at the present time there exists a distinct danger of exaggerating its significance. The fact that many new and apparently permanent combinations of characters may arise through hybridisation, and that the organisms thus produced have all the attributes of what we call distinct species, does not justify us in accepting the grotesque view—as it appears to me—that all species have arisen by crossing, or even the view that the organism is entirely built up of separately transmissible "unit characters."

Bateson tells us that "Baur has for example crossed species so unlike as *Antirrhinum majus* and *molle*, forms differing from each other in almost every feature of organisation." Surely the latter part of this statement cannot be correct, for after all *Antirrhinum majus* and *molle* are both snapdragons, and exhibit all the essential characters of snapdragons.

I think it is a most significant fact that the only characters which appear to be inherited in Mendelian fashion are comparatively trivial features of the organism which must have arisen during the last stages of phylogeny. This is necessarily the case, for any two organisms sufficiently nearly related to be capable of crossing are identical as regards the vast majority of their characters. It is only those few points in which they differ that remain to be experimented on. Moreover, the characters in question appear to be all non-adaptive, having no obvious relation to the environment and no particular value in the struggle for existence. They are clearly what Weismann calls blastogenic characters, originating in the germ-plasm, and are probably identical with the mutations of de Vries. These latter are apparently chromatin-determined characters, for, as Dr. Gates has recently shown in the case of Œnothera, mutation may result from abnormal distribution of the chromosomes in the reduction division.[6]

We have next to inquire whether or not the Mendelian results are really in any way inconsistent with the general theory of evolution outlined in the earlier part of this address. Here we are obviously face to face with the old dispute between epigenesis and preformation. The theory of ontogeny which I first put forward is clearly epigenetic in character, while the theory of unit characters, represented in the germ-cells by separate "factors," is scarcely less clearly a theory of preformation, and of course the conception of definite organ-forming substances in the cytoplasm falls under the same category. The point which I now wish to emphasise is that the ideas of epigenesis and preformation are not inconsistent with one another, and that, as a matter of fact, ontogenetic development is of a dual nature, an epigenesis modified by what is essentially preformation.

We have already dealt briefly with the question of organ-forming substances in the cytoplasm, and it must, I think, be clear that the existence of these is in no way incompatible with a fundamental epigenesis. We shall find directly that the same is true of Mendelian "factors" or Weismannian "determinants."

We have seen that it is possible to conceive of even a complex organism as inheriting nothing from its parent but a minute speck of protoplasm, endowed with potential energy, and a sequence of suitable environments, the interaction between the two bringing about a similar result in each succeeding genera-

6 *Quarterly Journal of Microscopical Science*, vol. lix., p. 557

tion, with a slow progressive evolution due to the operation of the law of accumulation of surplus energy. If any of the conditions of development are changed the result, as manifested in the organisation of the adult, must undergo a corresponding modification. Suppose that the chromatin substance of the zygote is partially modified in molecular constitution, perhaps by the direct action of the environment, as appears to happen in the case of Tower's experiments on mutation in the potato beetle, or by the introduction of a different sample of chromatin from another individual by hybridisation. What is the germ-plasm now going to do? When and how may the changes that have taken place in its constitution be expected to manifest themselves in the developing organism?

Let us consider what would be likely to happen in the first stages of ontogeny. If the germ-plasm had remained unaltered the zygote would have divided into blastomeres under the stimuli of the same conditions, both internal and external, as those under which the corresponding divisions took place in preceding generations. Is the presence of a number of new colloidal multimolecules in the germ-plasm going to prevent this? The answer to this question probably depends partly upon the proportion that the new multimolecules bear to the whole mass, and partly upon the nature of the modification that has taken place. If the existence of the new multimolecules is incompatible with the proper functional activity of the germ-plasm as a whole there is an end of the matter. The organism does not develop. If it is not incompatible we must suppose that the zygote begins its development as before, but that sooner or later the modification of the germ-plasm will manifest itself in the developing organism, in the first instance as a mutation. In cases of hybridisation we may get a mixture in varying degrees of the distinguishing characters of the two parent forms, or we may get complete dominance of one form over the other in the hybrid generation, or we may even get some new form, the result depending on the mutual reactions of the different constituents of the germ-plasm.

The organism into which any zygote develops must be a composite body deriving its blastogenic characters from different sources; but this cannot affect its fundamental structure, for the two parents must have been alike in all essential respects or they could not have interbred, and any important differences in the germ-plasm must be confined to the "factors" for the differentiating characters. The fundamental structure still develops epigenetically on the basis of an essentially similar germ-plasm and under essentially similar conditions as in the case of each of the two parents, and there is no reason to suppose that special "factors" have anything to do with it.

We thus see how new unit characters may be added by mutation and interchanged by hybridisation while the fundamental constitution of the organism remains the same and the epigenetic course of development is not seriously affected. All characters that arise in this way must be regarded, from the point of view of the organism, as chance characters due to chance modifications of the germ-plasm, and they appear to have comparatively little influence upon the course of evolution.

One of the most remarkable features of organic evolution is that it results in the adaptation of the organism to its environment, and for this adaptation mutation and hybridisation utterly fail to account. Of course the argument of natural selection is called in to get over this difficulty. Those organisms which happen to exhibit favourable mutations will survive and hand on their advantages to the next generation, and so on. It has frequently been pointed out that this is not sufficient. Mutations occur in all directions, and the chances of a favourable one arising are extremely remote. Something more is wanted, and this something, it appears to me, is to be found in the direct response of the organism to environmental stimuli at all stages of development, whereby individual adaptation is secured, and this individual adaptation must arise again and again in each succeeding generation. Moreover, the adaptation must, as I pointed out before, tend to be progressive, for each successive generation builds upon a foundation of accumulated experience and has a better start than its predecessors.

Of course natural selection plays its part, as it must in all cases, even in the organic world, and I believe that in many cases—as, for example, in protective resemblance and mimicry—that part has been an extremely important one. But much more important than natural selection appears to me what Baldwin [7] has termed "Functional Selection," selection by the organism itself, out of a number of possible reactions, of just those that are required to meet any emergency. As Baldwin puts it, "It is the organism which secures from all its over-produced movements those which are adaptive and beneficial." Natural selection is here replaced by intelligent selection, for I think we must agree with Jennings [8] that we cannot make a distinction between the higher and the lower organisms in this respect, and that all purposive reactions, or adjustments, are essentially intelligent.

Surely that much-abused philosopher, Lamarck, was not far from the truth when he said, "The production of a new organ in an animal body results from a new requirement which continues to make itself felt, and from a new movement which this requirement begets and maintains." [9] Is not this merely another way of saying that the individual makes adaptive responses to environmental stimuli? Where so many people fall foul of Lamarck is with regard to his belief in the inheritance of acquired characters. But in speaking of acquired characters Lamarck did not refer to such modifications as mutilations; he was obviously talking of the gradual self-adjustment of the organism to its environment.

We are told, of course, that such adjustments will only be preserved so long as the environmental stimuli by which they were originally called for continue to exercise their influence. Those who raise this objection are apt to forget that this is exactly what happens in evolution, and that the *sine qua non* of development is the proper maintenance of the appropriate environment, both internal and external. Natural selection sees to it that the proper conditions are maintained within very narrow limits.

A great deal of the confusion that has arisen with regard to the question of the inheritance of acquired characters is undoubtedly due to the quite unjustifiable limitation of the idea of "inheritance" to which we have accustomed ourselves. The inheritance of the environment is, as I have already said, just as important as the inheritance of the material foundation of the body, and whether or not a newly acquired character will be inherited must depend, usually at any rate, upon whether or not the conditions under which it arose are inherited. It is the fashion nowadays to attach very little importance to somatogenic characters in discussing the problem of evolution. The whole fundamental structure of the body must, however, according to the epigenetic view, be due to the gradual accumulation of characters that arise as the result of the reactions of the organism to its

7 "Development and Evolution" (New York, 1902), p. 87.
8 "Behaviour of the Lower Organisms" (New York, 1906), pp. 334, 335
9 "Histoire naturelle des Animaux sans Vertèbres," tom. i., 1815, p. 185.

environment, and are therefore somatogenic, at any rate in the first instance, though there is reason to believe that some of them may find expression in the germ-cells in the formation of organ-forming substances, and possibly in other ways. Blastogenic characters which actually originate in the germ-cells appear to be of quite secondary importance.

We still have to consider the question, How is it that organic evolution has led to the formation of those more or less well-marked groups of organisms which we call species? We have to note in the first place that there is no unanimity of opinion amongst biologists as to what a species is. Lamarck insisted that nature recognises no such things as species, and a great many people at the present day are, I think, still of the same opinion. In practice, however, every naturalist knows that there are natural groups to which the vast majority of individuals can be assigned without any serious difficulty. Charles Darwin maintained that such groups arose, under the influence of natural selection, through gradual divergent evolution and the extinction of intermediate forms. To-day we are told by de Vries that species originate as mutations which propagate themselves without alteration for a longer or shorter period, and by Lotsy that species originate by crossing of more or less distinct forms, though this latter theory leaves quite unsolved the problem of where the original forms that crossed with one another came from.

I think a little reflection will convince us that the origin of species is a different problem from that of the cause of progressive evolution. We can scarcely doubt, however, that Darwin was right in attributing prime importance to divergent evolution and the disappearance of connecting links. It is obvious that this process must give rise to more or less sharply separated groups of individuals to which the term species may be applied, and that the differences between these species must be attributed ultimately to differences in the response of the organism to differing conditions of the environment. It may be urged that inasmuch as different species are often found living side by side under identical conditions the differences between them cannot have arisen in this way, but we may be quite certain that if we knew enough of their past history we should find that their ancestors had not always lived under identical conditions.

The case of flightless birds on oceanic islands is particularly instructive in this connection. The only satisfactory way of explaining the existence of such birds is by supposing that their ancestors had well-developed wings, by the aid of which they made their way to the islands from some continental area. The conditions of the new environment led to the gradual disuse and consequent degeneration of the wings until they either became useless for flight or, in the case of the moas, completely disappeared. It would be absurd to maintain that any of the existing flightless birds are specifically identical with the ancestral flying forms from which they are descended, and it would, it appears to me, be equally absurd to suppose that the flightless species arose by mutation or by crossing, the same result being produced over and over again on different islands and in different groups of birds. This is clearly a case where the environment has determined the direction of evolution.

In such cases there is not the slightest ground for believing that crossing has had anything whatever to do with the origin of the different groups to which the term species is applied; indeed, the study of island faunas in general indicates very clearly that the *prevention* of crossing, by isolation, has been one of the chief factors in the divergence of lines of descent and the consequent multiplication of species, and Romanes clearly showed that even within the same geographical area an identical result may be produced by mutual sterility, which is the cause, rather than the result, of specific distinction.

Species, then, may clearly arise by divergent evolution under changing conditions of the environment, and may become separated from one another by the extinction of intermediate forms. The environmental stimuli (including, of course, the body as part of its own environment) may, however, act in two different ways: (1) Upon the body itself, at any stage of its development, tending to cause adaptation by individual selection of the most appropriate response; and (2) upon the germ-plasm, causing mutations or sudden changes, sports, in fact, which appear to have no direct relation whatever to the well-being of the organism in which they appear, but to be purely accidental. Such mutations are, of course, inherited, and, inasmuch as the great majority of specific characters appear to have no adaptive significance, it seems likely that mutation has had a great deal to do with the origin of species, though it may have had very little to do with progressive evolution.

Similarly with regard to hybridisation, we know that vast numbers of distinct forms, that breed true, may be produced in this way, but they are simply due to recombinations of mutational characters in the process of amphimixis, and have very little bearing upon the problem of evolution. If we like to call the new groups of individuals that originate thus "species," well and good, but it only means that we give that name, as a matter of convenience, to any group of closely related individuals which are distinguished by recognisable characters from the individuals of all other groups, and which hand on those characters to their descendants so long as the conditions remain the same. This, perhaps, is what we should do, and just as we have learnt to regard individuals as the temporary offspring of a continuous stream of germ-plasm, so we must regard species as the somewhat more permanent but nevertheless temporary offshoots of a continuous line of progressive evolution. Individuals are to species what the germ-plasm is to individuals. One species does not arise from another species, but from certain individuals in that species, and when all the individuals become so specialised as to lose their power of adaptation, then changes in the environment may result in the extinction of that line of descent.

It is scarcely necessary to point out that no explanation that we are able to give regarding the causes of either phylogenetic or ontogenetic evolution can be complete and exhaustive. Science can never hope to get to the bottom of things in any department of knowledge; there is always something remaining beyond our reach. If we are asked why an organism chooses the most appropriate response to any particular stimulus, we may suggest that this is the response that relieves it from further stimulation, but we cannot say how it learns to choose that response at once in preference to all others. If we are asked to account for some particular mutation, we may say that it is due to some modification in the constitution or distribution of the chromosomes in the germ-cells, but even if we knew exactly what that modification was, and could express it in chemical terms, we could not really say why it produces its particular result and no other, any more than the chemist can say why the combination of two gases that he calls oxygen and hydrogen gives rise to a liquid that he calls water.

There is one group of ontogenetic phenomena in particular that seems to defy all attempts at mechanistic interpretation. I refer to the phenomena of restitution, the power which an organism possesses of restoring the normal condition of the body after it has been violently disturbed by some external agent. The fact that a newt is able to regenerate its limbs over and over again after they have been removed, or that an echinoderm blastula may be cut in half and each half give rise to a perfect larva, is one of the most surprising things in the domain of biological science. We cannot, at present, at any rate, give any satisfactory mechanistic explanation of these facts, and to attribute them to the action of some hypothetical entelechy, after the manner of Prof. Hans Driesch, is simply an admission of our inability to do so. We can only say that in the course of its evolution each organism acquires an individuality or wholeness of its own, and that one of the fundamental properties of living organisms is to maintain that individuality. They are able to do this in a variety of ways, and can sometimes even replace a lost organ out of material quite different from that from which the organ in question is normally developed, as in the case of the regeneration of the lens of the eye from the iris in the newt. That there must be some mechanism involved in such cases is, of course, self-evident, and we know that that mechanism may sometimes go wrong and produce monstrous and unworkable results; but it is, I think, equally evident that the organism must possess some power of directing the course of events, so as generally to secure the appropriate result; and it is just this power of directing chemical and physical processes, and thus employing them in its own interests, that distinguishes a living organism from an inanimate object.

In conclusion I ought, perhaps, to apologise for the somewhat dogmatic tone of my remarks. I must ask you to believe, however, that this does not arise from any desire on my part to dogmatise, but merely from the necessity of compressing what I wished to say into a totally inadequate space. Many years of patient work are still needed before we can hope to solve, even approximately, the problem of organic evolution, but it seemed to me permissible, on the present occasion, to indulge in a general survey of the situation, and see how far it might be possible to reconcile conflicting views and bring together a number of ideas derived from many sources in one consistent theory.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

LONDON.—A course of twelve lectures on the theory and practice of radio-telegraphy will be delivered by Prof. J. A. Fleming at University College, on Wednesdays at 5 p.m., beginning October 28. The course will be in two parts, six lectures before Christmas and six between Christmas and Easter. It is open to both members and non-members of the University. It is intended for telegraphic engineers and electrical students who have already some elementary knowledge of the subject, and it will presume an elementary acquaintance with the differential calculus, and with the properties of vector quantities. The object of the course is to impart a more advanced knowledge of the theory and practice of wireless telegraphy in its modern form. University College is provided with an antenna and P.M.G. licence for its use.

THE High Commissioner for New Zealand announces that Dr. W. P. Gowland, of the University of Liverpool, has been appointed to the chair of anatomy at the University of Otago, Dunedin, New Zealand.

WE learn from the *Times* that the Senate of the National University of Ireland has passed the following resolution with reference to the destruction of the town of Louvain :—"The Senate of the National University of Ireland desires to offer to the illustrious University of Louvain its deep sympathy on the calamity which has befallen it—a calamity without parallel in history since the destruction of the Library of Alexandria. If this example prevail in warfare, then we may expect to find the records and achievements of civilisation extinguished by ignorance in arms. Therefore we appeal to the universities of all nations to unite in a protest against an act so disastrous to the progress of mankind."

THE Board of Agriculture and Fisheries has awarded research scholarships in agricultural and veterinary science of the annual value of 150*l.*, tenable for three years, to the following candidates, viz. :—*Agricultural Science,* J. Ll. Evans (Wales), S. M. Wadham (Cantab.), J. W. Munro (Edinburgh). *Veterinary Science,* R. Daubney, A. H. Adams. The Board has also awarded Mr. E. W. Jeffreys (Wales) an agricultural scholarship tenable for two years to fill a vacancy caused by the resignation of a scholar selected last year. The scholarships have been established in connection with the scheme for the promotion of scientific research in agriculture, for the purposes of which the Treasury have sanctioned grants to the Board from the Development Fund, and they are designed to provide for the training of promising students under suitable supervision with a view to enable them to contribute to the development of agricultural and veterinary science.

THE annual report of the Education Branch of the Board of Agriculture on the disposal of grants for agricultural education and research for the year 1913–14 shows that the Board is making satisfactory progress with its comprehensive scheme of organising agricultural work in the country. It has arranged for most of the universities to undertake special work in connection with the various counties which they serve, and, in addition, it supports a number of research institutes put up for the express purpose of investigating particular subjects. The whole scheme has been carefully planned to avoid overlapping; the report furnishes most interesting reading, and is a sufficient reply to the assertion sometimes made that British Government Departments can do nothing for scientific research. It is not claimed that the scheme is yet perfect; indeed, it is not yet in full working order, but it seems clear from the details furnished that things are going satisfactorily, and that the fully developed scheme will serve the purpose for which it was intended. Provision is made for higher agricultural education, the provision of technical advice for farmers, the investigation of local problems, and for carrying out agricultural research at institutions the function of which it is to develop subjects rather than to study set problems. The total amount of money granted during the year was 67,939*l.*, against 32,434*l.* last year.

THE calendar for 1914–15 of the Edinburgh and East of Scotland College of Agriculture has now been issued, and copies may be obtained from the secretary to the college, 13 George Square, Edinburgh. The college was founded in 1901 with the object of providing for agricultural education and research in the central and south-eastern counties of Scotland. The classes of the college are arranged in conjunction with certain classes in the science faculty of Edinburgh University, so as to provide a full course of teaching theoretical and practical, in agriculture and the allied sciences. This cooperation with the University has the further advantage that the courses for the diploma