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563RD ORDINARY GENERAL MEETING.

HELD IN COMMITTEE ROOM B, THE CENTRAL HALL, WESTMINSTER, ON MONDAY, FEBRUARY 1ST, 1915, AT 4.30 P.M.

ARTHUR W. SUTTON, ESQ., J.P., F.L.S., TREASURER, IN THE CHAIR.

The Minutes of the preceding Meeting were read and confirmed.

The CHAIRMAN, in introducing Prof. Ernest W. MacBride, expressed to him the thanks of all present for his coming to read a paper to them on a subject of such great interest and importance.

THE PRESENT POSITION OF THE THEORY OF ORGANIC EVOLUTION. By Prof. ERNEST W. MACBRIDE, M.A., F.R.S.

I HAVE chosen the subject of the theory of evolution as a theme on which to address you for several reasons; first because of all biological subjects this theory awakens the most general interest on account of its far-reaching implications; second, because I regard it as touching one of the two root problems of biological science, viz., the nature of heredity, and hence it possesses for me a supreme interest; and thirdly, because the theory in the form in which Darwin presented it to the world has been challenged by leading biologists at the present time, and this challenge has raised a very lively controversy in scientific circles which is still going on. Since we have all read the *Origin of Species,* one might assume that all my hearers are familiar with Darwin's position, but perhaps, since it is doubtless a considerable time since any of us have read the celebrated *Origin* with care, it may not be out of place to summarize the position taken up in that famous book.

The mere idea that in some way the forms of animals had changed as time progressed, and that different forms of animals had originated from the same ancestral species, was by no means propounded for the first time by Darwin. As he shows in the *Origin of Species,* such an idea had been put forward repeatedly from the time of Aristotle until the present. The merit of

Darwin's work stands or falls with the validity of the proof which he offers that there are processes *now* in operation which must inevitably lead to just such an evolution as many theorists had postulated. To all such theorists the naturalists then in authority had replied that the evidence available compelled them to assume that specific form was invariable--that like begets like after its kind, and that there was no natural process known which could alter it.

Now Darwin begins by pointing out that these same authoritative naturalists recognized the existence of varieties within the same species, and that all of them agreed that these varieties did not owe their origin to separate acts of creation, but had somehow been produced by the transformation of the parent species of which they were varieties. But if this be admitted, we then discover that it is impossible to draw the line between a species and a variety; that in the case of many species of animals, and more especially of plants, leading authorities are hopelessly at variance as to what are species and what are varieties, and it is rather absurd to imagine that a certain amount of difference between two forms is explicable by natural causes, but that to explain a slightly greater amount of difference direct Divine interference must be postulated.

Darwin then points out that the breeds of animals domesticated by man differ in most remarkable ways from the parent species from which they have been derived: that, to take the case of the pigeon for instance, of which he had made a special study, differences in the number of feathers in the tail, in the length and proportion of the bones of the wings and legs, in the shape of the skull, are all exhibited by these breeds. So different indeed are many of them from the wild rock pigeon, *Columba livia,* that many fanciers would give no credence to the suggestion that they had been derived from that species, but supposed that they must have originated from unknown species or had been produced by the crossing of several distinct species. Darwin points out, however, that among all the known wild species of the genus *Columba,* there is none that shows anything like the enormous tail of the fan-tail, with a number of feathers greater than that found in any bird of even the order to which *Columba* belongs.

We may perhaps make the matter clearer by taking another case which Darwin discusses in that wonderful book read by so few, viz., *The Variation of Animals and Plants under Domestication,* and that is the case of the dog. After examining with the greatest care all the evidence which he could collect as to the origin of the dog, he arrives at the conclusion that all the domesticated breeds are derived from several wild species, which fall into two main categories, viz., those allied to *Canis lupus*, the northern wolf, and those allied to the jackal, *Canis aureus.* No doubt the blood of these species has been crossed again and again. But, as visitors to the Zoological Gardens can convince themselves, there is nothing in the anatomy of either wolf or jackal which could possibly account for the peculiarities of the Chinese pug, of the dachshund, and of the greyhound.

If then the wide divergence between domesticated breeds is not to be accounted for by their origin from distinct species or by the crossing of different species, to what then is it to be ascribed? Darwin, after a long acquaintance with practical breeders, answers the question thus:—by the mating together of carefully selected specimens which show in the most marked degree the "points" which the breeder desires to emphasize. Darwin says that to select the proper individuals, a lifetime of experience is needed, since the points in which the selected show their superiority over their neighbours are often only visible to the trained eye.

Since all the "points" or characters of animals differ in intensity of development from individual to individual, and since either over-development or under-development seems to be inheritable, by careful selection practised through a number of years almost any amount of deviation from the original type can be achieved.

Darwin next points out that in every species of animal far more young are produced than can possibly survive; indeed, it is obvious on reflection that where the animal population of a district remains the same from decade to decade, on an average only two of the offspring born of a single pair of parents survive to enjoy adult life and to raise offspring themselves. But let any lover of birds reason out the number of nestlings raised by a pair of sparrows, for instance, during their lifetime, and then calculate what a destruction of nestlings must ensue. Under such circumstances, as Darwin points out, the surviving two will be those best fitted to their surroundings—that is those which are best adapted to gain food, withstand cold, and evade their enemies. Under normal circumstances this elimination of all but the most fit, generation after generation, will keep the average of health and strength in the species at a high level, but if the circumstances change, if the climate becomes colder, wetter or warmer, or if a new class of enemies turns up, then the standard of what is fit will change also, and by the survival of a slightly different type of animal in the course of generations the type will slowly change. If a species spreads over a wide range of country, portions of it will probably experience different conditions, and rather different types will survive in different places, and thus slowly out of one species two new species will be produced. This survival of different types was metaphorically styled by Darwin a selection by Nature and was compared by him to the selection of certain types for mating by the breeder; and hence the term Natural Selection.

The part of the whole theory which creates most difficulties for the theologian is this apparently ruthless waste of young life —the "unfulfilled intention," as Thomas Hardy calls it, so patent in Nature, and yet whether or not Darwin is right in assuming that by natural selection species are really modified or not, nothing is more absolutely certain than that this waste goes on, and it seems to me that this is the real difficulty to be faced and grappled with in endeavouring to reconcile a religious view of life with the laws of Nature as we know them.

It is obvious that, unless there are inheritable differences between members of the same species, natural selection can do nothing, and it by no means follows that differences that we can *see* are differences that can be inherited. A man may be sickly or stunted owing to illness or want of care during his infancy, and yet that man may become the father of a child absolutely free from defect. Now, according to Darwin, inheritable differences are of two kinds, viz., small intensifications or diminutions in the "points" of an animal requiring a trained eye to detect and appreciate them, and great conspicuous differences which are termed by the breeder "sports." A familiar instance of a "sport" is the Irish yew, which has its branches turned up so that they simulate a kind of urn. This yew, which is now to be found scattered all over these islands, is known to have originated from a single tree found growing on a mountain in Ireland. There is no doubt that some domestic breeds, as for instance hornless cattle, have been produced by some breeders by the preservation of such "sports," and the question arises whether something analogous may not take place in Nature. Darwin comes to the conclusion that sports have had little or nothing to do in the building up of natural species, since to modify a population the new type must turn up frequently if it is to constitute a sufficient proportion of the survivors to make its influence felt. Thus, to take an instance quoted by Darwin, suppose that it were advantageous to the crows to increase the length of their beaks, this would be brought about not by the appearance as a sport of a crow with an enormously long beak, whose offspring would found a new race, but by the preservation of a large number of crows with moderately long beaks in each generation.

It is a matter of history that in about 20 years Darwin's theory won its way to wellnigh world-wide acceptance, and it was then obvious that the next step to be taken in the elucidation of biological law was the determination of the causes and course of variation. To this task Darwin applied himself, and in 1868 brought out his master-work, to which I have already alluded, *The Variation of Animals and Plants under Domestication.* The compilation of this book was really the end of all his labours, of which the *Origin of Species* was merely a preliminary account; an account which the pressure of friends induced him to write before he was ready to place his completed evidence before the world. In his second book he takes up the question of the cause of variation, and after a survey of the whole field he arrives at the tentative conclusion that variations are due to the indirect effect of changes in the conditions of life, that is in the environment. The indirect effect, he says advisedly, because on the one hand it is well known that changes in the environment often produce a direct effect on the body: thus cold stimulates the growth of hair, as may be easily seen in the case of children who run about bare-legged on the shore. Since, however, the only bridge between parent and offspring is the tiny germ-cell, it is obvious that nothing can have an hereditary effect except it affects the germ-cell, and as Darwin did not see how the germ-cell could be affected by a change in the body of the parent so as to give rise to a corresponding change in the body of the offspring, he speaks of an "indirect effect," meaning, I suppose, that the germ-cell is affected but not necessarily in a corresponding manner. Yet, in spite of everything, he admits that there is some evidence that the effects of use in strengthening an organ and of disuse in diminishing it are handed on from one generation to another in some cases. To account for this he puts forward his theory of *pangenesis.* According to this theory, every part of the body produces gemmules, and these gemmules circulate in the fluids of the body and accumulate in the genital organs. If an organ is altered, the gemmules which it casts off will be altered—and these altered gemmules accumulating in the genital cells eventually make their influence felt on the course of heredity. Though this theory has been regarded as wild and fantastic by many subsequent writers, and although Darwin himself regarded it merely as a tentative hypothesis, it seems to me that there is probably a considerable element of truth in it. My reasons for this will be given later on.

The next considerable advance upon Darwin's position was made by Darwin's brother-in-law, Sir Francis Galton, who invented a means of measuring at the same time the number of variations and their amount. He applied his method to the measurement of variations in man, but it was applied to other animals by Weldon, who was Professor first in University College and afterwards at Oxford. An example will make this plain. Suppose we are desirous of finding how much the breadth of the carapace of a crab varies, and how many broad and how many narrow crabs there are, it is obviously of no use to measure the absolute values of the breadth of the carapaces of various crabs, because crabs vary in size. If, however, we take the length of the crab as a unit and express the breadth of the carapace as a fraction of it, then the value of this fraction is high for broad crabs and low for narrow crabs. If we now determine the value of this fraction, for say 500 crabs, and sort the values into groups, the members of which differ from one another by less than a certain limit, then we have the means of drawing a curve which will show us at once the range of variation and the number of specimens showing any given extent of variation. If we measure along a horizontal line lengths proportional to the values of the fraction, and erect at the point corresponding to each value a perpendicular proportional in length to the number of specimens showing this value, then we get by joining together the summits of these perpendiculars a curve. If we take a great number of specimens the curve becomes more and more symmetrical, proving that there is a certain mean breadth of carapace which the great majority of crabs show, and that as we recede from this value we find fewer and fewer crabs, but that on the whole there are as many with the fraction at a higher value than the mean as there are with the fraction at a lower value than the mean, and that extreme values either above or below the mean are very rare. Exactly such a curve as this is got if we record hits made by shooting at a target—most will fall at a certain distance from the bull's-eye. There will be a very few bull's-eyes and a few outers. Hence this curve is called the curve of error. There is a school of scientists headed by Prof. Pearson who seem to think that by this method we have penetrated the secrets of variation, that all these variations from the mean are inheritable, and that if natural selection were to favour a greater breadth of carapace than the mean the deviations necessary are present in sufficient numbers, and in any event only very few crabs of any generation survive.

But meanwhile another line of attacking the problem had been developed, and one which led to quite different results. An Augustinian monk named Gregor Mendel had been performing experiments on the crossing of different races of plants even at the time that Darwin was putting the final touches to the *Origin of Species.* He obtained most interesting results, but his work remained unnoticed by his scientific contemporaries; it was not until 1900 that it was rediscovered. Then his experiments were repeated and his results confirmed and extended, and a large school of enthusiastic experimenters into the laws of heredity along the lines mapped out by Mendel has grown up in America and England. A popular idea has arisen that Mendel has in some way refuted Darwin. It is therefore necessary to look closely into what Mendel's results really were, and they can be made quite clear by taking as example the pea plant, on which Mendel's work was chiefly done. Numerous varieties of peas exist; thus, when ripe, dry peas may be yellow or green, round or wrinkled. Now Mendel found that if the pollen from a plant produced from a yellow pea were used to fertilize the ovules of a plant produced from a green pea or *vice versa,* only yellow peas were produced. If these yellow peas were then sown, they produced plants which, when self-fertilized, gave rise to yellow and green peas in the proportion of three yellow to one green.

Now in this cross Mendel termed yellowness the *dominant* character, because it alone appeared in the hybrid or first filial generation; greenness was termed by him the *recessive* character because it disappeared in the first filial generation, but reappeared in the offspring of the hybrid, that is the second filial generation, suffering therefore only a temporary eclipse.

If the green peas which reappear in this generation be sown, they give rise to plants bearing (when self-fertilized) only green peas, and this is true however many generations may be raised from them. Further, some of the yellow peas do the same; but two-thirds of them give rise to plants which, when self-fertilized, produce yellow and green peas in the proportion of three yellow to one green; in other words, they behave like the original hybrid of the first filial generation. Mendel explained his results as follows: When the first hybrid produces ovules and pollen grains, these are of two kinds. One sort of ovule and of pollen grain carries the yellow character, and another sort of ovule and pollen grain carries the green character. These two kinds are produced in equal numbers, and in self-fertilization they may be supposed to be mixed at random. There is therefore one chance in four that a green ovule will meet a green pollen grain, and one chance in four that a yellow ovule will meet a yellow pollen grain, but there is also one chance in four that either a yellow pollen grain will meet a green ovule, or that a green pollen grain will meet a yellow ovule. These two latter unions produce exactly the same result; the resulting pea *looks* yellow, but is a hybrid which in the next generation will give rise, when self-fertilized, to yellow and green peas. Therefore, out of four peas produced by the first hybrid, one is a pure yellow, one is a pure green, and two are yellow hybrids, which look like the pure yellow, so that the proportion, three yellow to one green, is accounted for.

Now suppose that we cross a plant producing round yellow peas with one which produces green angular peas. The resulting peas are round and yellow. Thus roundness dominates over angularity and yellowness over greenness. But when the plants raised from these peas are self-fertilized, three-fourths of the peas produced are yellow and one-fourth green as before, and three-fourths are round and one-fourth angular; but these two sets of qualities are distributed through the peas independently of each other; that is to say, it is exactly as if one had a bag of billiard balls and one were to select at random three-fourths and paint them black, and if one were then to put the balls back into the bag, shake them up, and again select at random three-fourths and mark them with a red dot. As a consequence some balls would have the black paint and the red dot and some would have neither, and some would have the black paint only and some the red dot only. These balls may be taken to represent the ovules and pollen grains of the hybrid. So amongst our peas produced by the self-fertilization of the hybrid we have not only green and angular and yellow and round peas, but two new varieties make their appearance, viz., yellow and angular and green and round. On an average out of sixteen peas nine (*i.e.*, $\frac{3}{4} \times \frac{3}{4}$) will be yellow and round, one will be green and angular (*i.e.*, $\frac{1}{4} \times \frac{1}{4}$), whilst three will be round and green ($\frac{3}{4} \times \frac{1}{4}$), and three angular and yellow ($\frac{3}{4} \times \frac{1}{4}$). Of these two new varieties, however, only one-third, *i.e.*, one in sixteen of the whole progeny of the original hybrid, will breed true. The two-thirds reveal themselves as hybrid with respect to one character (the dominant one).

These examples illustrate the laws of heredity discovered by Mendel, and little new in principle has been added since. It has, however, been shown that the difference between two races consists in most cases in the presence in one of some definite character which is wanting in the other. The race characterized by the presence is the dominant one. The best instance of this is the ordinary white mouse. This when crossed with the grey wild mouse yields grey offspring. Now the whiteness or albinism is due to a lack of something in the constitution necessary for the production of colour. Hence whiteness is recessive. Such characters in the offspring are said to be due to "factors" in the germ. The enthusiastic supporters of these views go so far as to deny that any "variations" except those of the marked character due to the presence or absence of a "factor" are inheritable at all. The variations in degree, such as were measured by Galton and Weldon, are termed by them "fluctuations," and are declared to be non-inheritable. Fluctuations are ascribed to differences in the nutrition of various germs, not to differences in their inherent hereditary potentiality. A difference in the hereditary potentiality such as would give rise to a new race is termed a "mutation," and most Mendelians are prepared to admit that such mutations occasionally take place, though how or why they are unable to say. A celebrated Dutch botanist, De Vries, believed that he had discovered a plant (*Oenothera lamarckiana*), the evening primrose, in the act of giving off mutations; but as this plant is of hybrid and American origin, many biologists suspect that perhaps the apparent origin of mutations may be only the segregation out of the characters of the two parent species and the recombination of these in different groupings, just as we have seen that round green peas may be produced by the combination of round yellow and green wrinkled peas.

If, however, it is to be admitted, as few reasonable Mendelians would deny, that our domestic breeds have been derived from wild species by the appearance of inheritable mutations, then it is of great interest to know more about these mutations. It appears that they are nearly all due to the absence of a factor which was present in the original wild species. To give a familiar example: domesticated black, yellow, chocolate, and white mice are known; the wild mouse is of a grey colour technically called *agouti*. Now this agouti when closely examined is found to contain as factors, black, yellow, and chocolate, and of course the wild mouse has in its constitution *chromogen*, the factor which permits the development of colour, which the white mouse entirely lacks. So that the only evolution of which the more extreme Mendelians will admit the evidence is evolution backwards. It has been even hinted that the primordial germ from which all life was derived may have contained the factors for all the qualities of which a Shakespeare might boast himself, and that these qualities were prevented from exhibiting themselves merely by the presence of inhibiting factors which were gradually dropped as time went on.

There is an extreme left wing of the Mendelians, however, who go further than this, and deny altogether the occurrence of mutations.

De Vries had maintained the view that every natural species consists of several, sometimes of many, "elementary species," *i.e.*, of forms producing germs of different hereditary potentialities, which in nature are continually intercrossing, and so producing much of the variation which is observed in natural species. The effect of mating selected pairs is, according to De Vries, merely to purify gradually the selected stock and finally to arrive at a race consisting of only one elementary species. When this goal has been reached, according to most Mendelians, no further selection will have any effect in changing the character of the stock. The difference between what we may call a natural species and an elementary species, is that, speaking broadly, two natural species either refuse to cross with one another at all, or if they do cross will produce sterile offspring, whilst two elementary species cross freely and produce fertile offspring.

We may now briefly review the situation at which we have arrived, if the position taken up by Mendelians is a sound one. We find then that there are practically an infinite number of elementary species of animals and plants in the world, each with its distinct definite and unalterable hereditary potentiality. Groups of these are capable of crossing with each other and constitute those populations known as species to the naturalists. In this way continually new combinations of characters are produced, from which, however, the characters of the original elementary species are always tending to segregate out. The process resembles exactly the dealing out of hands of cards from a pack, which is being continually reshuffled. Some Mendelians maintain that an infinite number of distinct hereditary potentialities have existed from the beginning of life, and that new forms can only arise and have only arisen by new combinations of these potentialities (Lotsy). Others are willing to admit that mutations, *i.e.*, changes in the hereditary potentiality, may have taken place; but these changes have always consisted in the dropping of a factor, and in thus producing a form which, compared to the original form, may be regarded as a cripple.

In attempting to give a history of life on the earth on these lines we are thus led into a complete cul-de-sac. The continual shuffling of potentialities brought about by the sexual union of two germs we can understand, but how all these separate races arose is left in insoluble mystery.

It is interesting to recount the solution of the difficulty which was offered by Weismann. Weismann had arrived at somewhat similar conclusions to the Mendelians on totally different grounds. His reasoning was as follows: In the case of animals the two germs brought together in sexual union are of different kinds; one of these, the male germ, is very minute, and the other, the female germ or egg, is much larger. Yet the hereditary qualities of the progeny resulting from the union of two varieties is just the same whichever variety supplies the egg. Therefore the two germs so different in appearance must be entirely alike in their hereditary potentialities. Now the portion of the male germ which penetrates the female germ consists entirely of the nucleus, and the conclusion is obvious that the nucleus must be the bearer of the hereditary qualities. But the nucleus is a complex structure; it consists of a firm wall enclosing a clear sap traversed by transparent cords called linin, on which are strung a certain number of granules termed *chromatin*, from their power of absorbing and holding staining materials. When the nucleus divides, this chromatin arranges itself in the form of a number of short, thick rods, called *chromosomes*, and each chromosome becomes split longitudinally and the two halves go to the two daughter nuclei. The number of chromosomes produced in each dividing nucleus is the same, and is characteristic of the species of animal to which the nucleus belongs. Since then we find here a substance which Nature takes the greatest pains to divide into precisely equal halves at each division, and since the hereditary substance is somewhere in the nucleus, Weismann jumps to the conclusion that the chromatin is the hereditary substance of which he is in search. Before the germ-cells are ready to unite each germ-cell has only half the normal number of chromosomes. Weismann assumes that the halving can take place in a random manner, and thus he comes to the conclusion that at each sexual union there is a reshuffling of chromosomes, and in this way he accounts for the origin of inheritable variations. Then, of course, he stumbles against the difficulty of accounting for the different inheritable qualities embodied in the different chromosomes. Weismann assumes that these differences began in the simple ancestors of the higher animals and plants, which, according to the view generally held, consisted of single cells. In such organisms, according to Weismann, it was possible for acquired qualities, "fluctuations," to be inherited, and the action of different environments caused differences in hereditary potentiality, out of which by varied combination the qualities of the higher animals were built up. In resorting to this explanation Weismann virtually gives up his case. There is no ground whatever for the supposition that the simple animals are constitutionally unlike more complex animals, and moreover all the direct evidence which has been brought forward to support the view that fluctuations are non-inheritable, applies with just as much force to unicellular as to multicellular animals; in fact some of the best evidence has been supplied by the study of unicellular animals, and to this evidence we must now apply ourselves.

Whilst in the case of the majority of the higher animals the production of young is impossible without the previous coalescence in sexual union of two germs, which are carried by distinct individuals, yet this is by no means universally the case. There are many cases in animals where both kinds of germs are borne by the same individuals, which are then termed *hermaphrodites*, and in this case the production of young by self-fertilization is possible, and then we need not fear the introduction of extraneous factors. Self-fertilization is possible in the case of the great majority of the higher plants.

In other cases the egg is capable of developing without fertilization, a phenomenon which is known as *parthenogenesis*; in this case also nothing but the hereditary potentiality of one kind of parent need be considered. Lastly, in the lower animals there is no distinction between body and germ-cell, but the mother gives rise to two daughters by dividing in two, and for long periods this kind of reproduction can go on without the intervention of anything that could be called sexual union.

In all three cases we have the opportunity of raising what has been called a "*pure line*" of progeny. For the case of self-fertilization such a line has been investigated by Johannsen in the case of the bean. Johannsen observed that if beans of a certain type were taken, and individual beans sorted according to their weight, a typical curve of error could be obtained, and if the larger beans were selected the average size of their progeny was larger than that of the smaller beans, though not so much larger as it ought to have been in proportion to the size of the parents. This want of proportionate increase was detected by Sir Francis Galton and called by him "regression towards mediocrity."

If, however, we raise a progeny from a single bean produced by the self-fertilization of a single bean plant, then again we find that the progeny sorted by size will give rise to a curve of error. But if we now select the larger beans from this progeny and raise offspring from them, we find that they vary about a mean, which is not the size of their immediate parent, but is a fixed mean, which is the same as that for the progeny of the smaller beans. The variations in size seem therefore to be fluctuations and in no way indicative of a change in hereditary potentiality, and a change in type of such a line by the continual selection of the larger individuals for propagation would seem to be impossible.

The same result has been arrived at by Agar, working on the eggs of the water-flea *Daphnia,* which develop parthenogenetically, and by Jennings, who studied the unicellular animal *Paramecium,* which propagates itself by division.

It would seem, therefore, that this work leads to the conclusion that a very essential part of Darwin's reasoning is unsound, for it would appear that by a continual selection of individuals showing a certain character in greater or less degree—and this is what Darwin postulated—no change in the type can be effected.

Before, however, we resign ourselves to this conclusion, there are several matters which call for grave consideration. In the first place, no one doubts that when two races differing from one another in a sharply marked character are crossed, the progeny will inherit the qualities of the parents according to the laws worked out by Mendel. In broad outline this was known to Darwin, who knew nothing of Mendel or of his work. But it is to be remembered that Mendel expressly excluded from his purview "all qualities of a more or less description," and he never hinted that the laws which he discovered would apply to them. Yet it is precisely these qualities of "more or less" which are important to the comparative anatomist. Allied species and genera differ from one another not so far as can be seen in the presence or absence of a factor, but usually in the greater or less development of homologous organs. These greater or lesser developments stand, in many cases, in obvious relationship to the possessor's functions and habits, and it is this adaptation which Mendel utterly fails to explain. Again, while it is true that cultivated white sweet-peas differ from the wild stock in the absence of a factor which would allow, if present, of the production of the original purple colour, the difference in size of the pod and pea in the cultivated and wild varieties cannot thus be accounted for. It is easy to point to the fact that the white rabbit, if crossed with the wild rabbit, will give rise to progeny which will behave in a Mendelian manner as regards colour, but the difference in size and weight between the domesticated and wild varieties is not thus got over. Of course, we may, if we will, extend the Mendelian rules to cover differences of more or less, and this has actually been done by some Mendelians. We may say either that a mutation may cause only a slight increase or decrease in some organ, but if we do this we are only repeating in pompous phrase Darwin's statement that differences in size are *sometimes* inherited—or we may suppose that different elementary species are distinguishable from one another by the presence of factors which cause slight differences in the proportions of certain organs, so that by their crossing all intermediate grades can be accounted for. The difficulty about meeting such a facile presupposition as this is to devise means to bring it to a crucial test. If we select bigger individuals from a species and by mating them raise bigger offspring, and claim that this proves an inheritability of differences of "more or less," the Mendelian answers that in this case the difference in degree was due to a mutation because it bred true, and thus we find ourselves reasoning in a circle.

If, then, the blind acceptance of the idea that the principles of Mendel are the final word in the science of heredity leads to the conclusion that the qualities or factors of the germ cells are as unalterable as the chemical elements, let us put this theory to the test of asking whether it explains the known facts of life. In his *Origin of Species* Darwin emphasized the fact that the record of past life on the earth is exceedingly defective, and that all we have of it are bits and scraps. Broadly speaking, that statement still holds good, but since Darwin's time a few excessively lucky finds have been made. We seem to have chanced several times upon the actual locality where a type of animal was evolved. In the Western States of North America there once existed great inland lakes. These lakes, in due course, became filled up with beds of mud and sand, brought down by the rivers which flowed into them. As the lake became shallower these deposits formed swampy meadows at its edges, and when the animals that lived in the neighbourhood came down to drink they were often bogged in these swamps and drowned. Since these lakes existed for millions of years, we have embedded in them a fair sample of the quadrupeds which inhabited the neighbourhood, and in going from the earlier to the later of these beds we notice changes in these animals, for instance we behold the evolution of the horse out of a mammal having four toes on the fore limb and three on the hinder limb, like the modern tapir. Now in this series we see no evidence of the sudden acquisition or the sudden dropping of a "factor"; rather the change seems to have been due to the increase of certain parts by use and the diminution of other parts by disuse. Again, in the Karoo desert of South Africa there is a series of beds representing an even greater lapse of time, and in these we have the record of the evolution of a mammal out of a reptile. Here again no evidence of mutations in the original sense is seen, but in such important matters as the arrangement of the jaw-bones and ear-bones, wherein the difference between a mammal and a reptile is most marked, evidence of gradual change in size coincident with change in function is seen.

If Mendelism fails to suit the facts of palæontology, still more is it in disaccord with the facts of embryology. It is often tacitly assumed by Mendelians, who work chiefly with mammals and with the higher plants, that the young form is produced with all the characters of the adult. But, of course, in a large number of animals this is not so: the young one begins life as a larva which, in form and habits, is unlike the adult, and which only gradually acquires the form of the adult as it assumes the habits of the adult. Now, it has been found, if we take the case of an aberrant member of a group in which the normal type of adult structure is fairly constant, that the aberrant member when young exhibits a type of structure much more like the normal type than it does when it is adult. This phenomenon is interpreted in this way: the aberrant member of the group is supposed to have taken up a new mode of life and to have had its structure changed in consequence as a reaction to the new mode of life—just as continued exercise makes the leg muscles of the athlete increase in size. This reaction, in course of long generations, is believed to have been fixed in the constitution of the germ, so that eventually it comes about before the new environment has had time to act. A beautiful example of this is given by the life-history of the hermit crab. This crustacean, when adult, protects its abdomen by thrusting it into the empty shell of a whelk or sea-snail, and the abdomen becomes curved in conformity with the curvature of the shell. But when the hermit crab is young, its abdomen is quite straight, like that of other crabs and lobsters. If, now, a young hermit crab be reared to maturity, but be prevented from finding a shell, its abdomen will become curved, although not so much curved as if it had found a shell. We might well ask

Mendelians: If the hermit crab acquired its peculiar abdomen by the loss of the factor for straightness, how is it that the young hermit crab has a straight abdomen?

Such reasoning as this raises at once two objections; first, it may be asked, is there any evidence from experiments that such reactions to environment, in a word, such acquired characteristics, can be inherited? and, secondly, if they can, by what mechanism can this be accomplished? To answer the first objection we may add that such evidence is difficult to obtain, because to produce it demands experiments carried on over a much longer series of years than any Mendelian has as yet attempted. Nevertheless, in a few cases there is some indication of this inheritance of reaction. In the spotted salamander, for instance, it has been found that if the beast be reared on a dark background the spots of yellow diminish in size, and when this has gone on for several generations the young born, even if reared in normal surroundings, have smaller spots than young born of salamanders which have always lived in normal surroundings.

A good many cases of the same kind have been recorded from among plants; and it seems clear that when a plant or animal reacts to new conditions by a change of structure, if the influence of the new conditions continues long enough the change of structure becomes in time hereditary.

As to how the heredity can become affected, we do not, of course, know, but we can make a guess. We are beginning to know a little of the manner in which the complex body of the higher animal is built up out of the germ. We find at first a few *organ-forming substances* dispersed in the protoplasm of the germ. By the action of these the first simple tissues are built up. Then these tissues act on each other by emitting chemicals termed *hormones*. To give an example: if the stalked eye of a shrimp be pulled off, it grows a new one. But if the optic ganglion beneath the eye be removed, then, instead of a new eye, an antenna is produced. The only way to account for this is to assume that under normal circumstances some chemical is emitted by the ganglion which causes the skin above it to mould itself into an eye.

Now, if by a reaction to new conditions the tissues of an animal change, they will emit a new type of hormone into the blood, and these hormones will after a time be built up in the genital cells. When these cells develop the modified hormone will be set free, and will cause the modification of the tissues, even before the new environment has time to act.

If this view as to the manner in which heredity can be altered is correct, not only does the past history of life as exhibited by fossils become clear to us, but an explanation is afforded of the recapitulation of ancestral history given by embryonic and larval development. We can see that species of animals have become modified in the majority of cases through their entry into a new environment. This entry has usually taken place when the animal has reached the adolescent stage of development, and its structure is then modified as a reaction to the new environment. This modification enables it to exist in the new environment. Its life under the old conditions up to the period of migration constitutes the larval stage of its life-history. As time goes on the reaction to the new environment comes quicker and quicker and finally appears before the migration, and the larval stage is correspondingly shortened.

Our final conclusion, therefore, is that the laws discovered by Mendel throw no light whatever on the origin of variations, *i.e.*, changes in hereditary potentiality; they merely show us what will happen if two races already diverse from one another are crossed. But the real problem of biology is the origin of this diversity.

If the line of reasoning outlined above be sound, it will be gathered that the main position of Darwinism is entirely unaffected by recent discoveries. It is probable that Darwin laid too much stress on the parallelism of the differences between parent wild species and domesticated breed, and those between wild species and wild species. We now know that many of the differences in colour, etc., which distinguish breeds from parent species are pathological differences due to the elimination of a Mendelian factor, and are quite distinct from differences in general proportions due to functional reaction which divide wild species.

Still, when we recollect that in domestication a species is protected from danger and relieved from the necessity of violent exertion, one cannot help feeling that increase in bulk which so often characterizes it is due to a functional reaction, especially as it has been a matter of slow acquisition, and has not been acquired at a single bound, as we should expect in the case of a quality due to the presence or absence of a Mendelian factor.

Darwin was most probably mistaken in assuming that the differences in proportion of limbs, etc., which occur between members of the same brood are inheritable. The work of Johannsen and Agar on pure lines seems to show that they are not. But all experimenters on the subject of pure lines have been at pains to keep the environmental conditions as stable as they can. If differences occur in consequence of a changed environment, and if the changed environment persists long enough, then we get a changed heredity. Natural selection would then weed out those individuals which did not react—in a word, the unadaptable. If the further question be raised as to why some are more adaptable than others, we must frankly confess our ignorance. Explanations of living phenomena consist in comparing one living being with another, and in deducing general rules and characteristics. Attempts to compare the phenomena exhibited by living beings with those exhibited by the non-living have hitherto been unsuccessful. There is a superficial resemblance, of course, but when the comparison is pressed into detail it breaks down. The attempt to explain the activities of the simplest organisms, such as *Amœba*, on purely physical and chemical grounds, which at one time seemed to be on the verge of succeeding, has proved fruitless. *Amœba* reacts to its environment in a simpler way, but on the same general principles as we do ourselves. The teaching of biology seems to be that the condition of progress is expressed in the text "To him that hath shall be given."

Discussion.

The Chairman, in introducing the Lecturer, said: It is not my province at the present moment to express any opinion on the subject of the paper, but I hope I may have an opportunity of offering a few thoughts at the end of the discussion.

I should like to remind those present that the Professor has not come to argue in favour of any theory of Evolution, but to put before us the opinion held by the scientific world to-day of Darwin's theory of Organic Evolution.

Some may wonder why our Institute has chosen this subject for consideration and discussion, and may consider it a sign of our decadence and falling away to ask for such a paper, but our object is to learn all we can from every possible source, and try to bring all the knowledge we acquire to bear upon Revelation. We should lose much that is helpful if we only followed out our own lines of thought upon any question. As humble believers in the Christ revealed in the Bible, we naturally live in a very small groove compared with the world around us, and our outlook would become more and more contracted if we did not exchange thoughts with those who approach the problems of Nature from a less defined or restricted point of view. We should also lose opportunities of influencing some who differ from ourselves.

These are, perhaps, the principal reasons why we have asked the Professor to-day, so that our outlook may be widened, and that we may know what the world is thinking. One reason we feel so strongly about the subject of Organic Evolution is, that looking backwards for thirty or forty years, or more, we know that what may briefly be called "Darwinism" has modified the outlook of professing Christians to a very great degree. I do not say it *ought* to have done so, but most certainly it *has*! If we asked 100 thoughtful men to-day, clergymen or laymen, whether they believed that God created man in His Own image, we should not find that they would all express their belief in the same terms that were generally used some years ago.

Our learned Secretary, Mr. Maunder, in his intensely interesting paper on "The First Chapter of Genesis," asked the question, "When God beheld that which He had made, and saw that it was good, does it follow that, could a man have been there to look on, there was anything present that would have been apparent to his sight: anything, that is to say, that he could have recognized as an accomplishment of the command?" Mr. Maunder would suggest that, though God created man in His Own image, it did not follow that if we had been present on the sixth day of Creation, we should have recognized man as existing in the form we know him to-day. I mention this as an instance of the influence which Darwinism has had upon Christian men. Whether that theory of organic evolution which we are accustomed to speak of as "Darwinism" is itself founded upon sufficiently strong evidence as to warrant such a changed attitude is a matter of extreme interest to us all.

At the end of the lecture, the CHAIRMAN rose to propose a very hearty vote of thanks to Professor MacBride for his most able and interesting paper, and pointed out that the Lecturer had repeatedly stated in his paper that Darwinism stood or fell on the answer to this one question, "Is it possible for acquired characters to be passed on from one generation to another?" *i.e.*, "are variations acquired in the life of any animal or plant capable of transmission to a succeeding generation?"

Sir ROBERT ANDERSON, K.C.B.: We must all recognize the great interest attaching to the subject of Professor MacBride's paper; but practical people will recognize also that its interest is purely academic. For Darwinian Evolution is a mere theory, and a theory, moreover, which is not only unproved, but obviously incapable of proof. At a University College meeting a dozen years ago (1st May, 1903), Lord Kelvin uttered a memorable dictum on this subject. The occasion was one of a series of addresses on "Christian Apologetics." The first was delivered by Dean Wace, when I had the honour of presiding. At this second, an eminent botanist dealt with the Evolution theory in relation to his own sphere of study; and he demonstrated that while Darwinism was true in the garden, it was not true in the field. In other words, under the pressure of culture life tends to advance, but, in the absence of culture, deterioration is the rule. Lord Kelvin, who followed, touched upon the crucial question of the *origin* of life, and he summed up his argument by declaring that "science positively affirms creative power."

But scientists of a certain type use the hypothesis of Evolution simply as a cloak for their atheism. In marked contrast, both Kelvin and Charles Darwin accepted as a fundamental doctrine that all life must come from life; both refused to accept the doctrine that the phenomena of life are the results of blind chance. It is indeed more incredible than any miracle yet recorded, that the material, intellectual and spiritual life of man should be derived from the chance collisions of dead particles of dead matter. And the fact that man is a religious being shows that he is God's creature in a sense different from that implied by any theory of material evolution. As A. R. Wallace aptly said, "to call the spiritual nature of man a 'by-product,' is a jest too big for this little world."

Mr. WOODS SMYTH: I should like to congratulate the Victoria Institute on the lecture to which we have just listened. There is a distinction between organisms undergoing the process of evolution and the finished terminal forms of life. In the earlier ages, it may be argued, there were synthetic types of living organisms, that is organisms embracing potentially the forms of animals now widely separated. Thus one creature united the forms of the deer, the camel and the hog, but to-day these three animals are widely differentiated, and no amount of selection, natural or artificial, can make them other than what they are. Do what you will, the hog will still remain a pig. This suggests that experiments made now with living forms to illustrate the "Doctrine of Evolution" may not be satisfactory; at least, we cannot reverse the processes in their entirety; and since the conditions of life on the earth are not the same now as in ancient eras, even the very lowest forms of life may not be the same now as those in primeval ages. I would suggest that, in the living forms as known to us to-day, all potential factors for anything higher may have gone out of them. Yet Haeckel mentions that a certain species of Triton which breathed by gills only and had never developed lung tissue, did so develop this tissue when the water in the basin in which it was kept began to decline. Thus we have an instance of a gill-breathing water dweller being changed into a lung-breathing land animal, through change of its environment.

The Rev. A. IRVING, D.Sc.: Sir Robert Anderson has referred to a lecture delivered by Professor G. Henslow at University College. I had a son at the College at the time, and took the opportunity of hearing the lecture. Professor Henslow gave us the word "directivity,"—which a few years ago was not to be found in any dictionary. It expresses what Bergson has since taught us, and represents a something behind all vital processes, directing those purposeful activities. It is most important that we should have a clear idea of that "something" behind all phenomena. Bergson has recognized it, and does not hesitate to admit that we have in that something an influence which can only be ascribed to transcendent God. This is expressed by the term "Creative Evolution," and by Lord Kelvin's favourite phrase, "Creative and Directive Power." It is to my mind an expression of the Divine Immanence—the Divine Immanence *in* the universe—making use of the properties of matter to mould them to higher purposes, though the "Directivity of Life." (*See* Henslow, Trans. V., I., Vol. xliv.)

The last thought that I would suggest is this: when people go so far as to say,—as the Modernists do,—that what we include in the terms "mental" and "spiritual" are mere by-products of the mechanical action of the molecules of the human brain, as in the speculations of Haeckel, and the empirical charlatanism of Loisy, it brings us to the position which has found its *reductio ad absurdum* in the nonsense which has misled the German people, and brought about the present *débâcle*; nonsense against which their own great teacher, Treitschke, warned them some nine years ago. (*See* Professor J. H.

Morgan's Introduction, pp. 47, 48, *The German War-Book.* London: John Murray, 1915.)

The Rev. M. DAVIDSON asked whether Professor MacBride thought that fortuitous variations were partly due to bisexual reproduction. Weismann himself discovered that two varieties of *Cypris reptans* possessing marked colorations occur in the ponds near Freiburg. Individuals of the dark green variety appeared suddenly in an aquarium which contained the yellow-ochre coloured variety in the year 1887. As these variations occurred in the absence of sexual reproduction, this cannot be the sole cause of variations.

Would it be possible to ascribe fortuitous variations, if not due to bisexual reproduction, to the tendency of the cell to divide unequally, since the probability of the cell dividing equally would be very small, so that cells would tend to become heterogeneous?

Further, do regressive variations play any part in the evolution of species or varieties? He believed that Reid in his works had strongly emphasized that, without regressive variation, all species would rush to destruction. By regressive variation was meant failure to recapitulate ancestral development.

The Rev. J. J. B. COLES felt sure that they were all much obliged to Professor MacBride for having come and put before the Institute a statement, quite up-to-date, of the position of a theory of this importance. They were equally indebted to him for the way in which he presented the statement, and for the scientific honesty and caution which allowed that, on several very difficult points, no true solution had yet been attained.

The real problem of biology was the origin of diversity. That problem was yet unsolved. Might he suggest that in organic beings an element was found that was not automatic. We were not automata ourselves, but had a certain freedom of choice, and the same was the case with animals. To carry the argument further, was not an independent means of organic action found in the lower forms of life? If so, the question was one which called for a fuller investigation than it had yet received, as it ran on the true lines of comparative science. He thought, therefore, that in all organic matter, where we had life and all its mystery, it was better to begin at the very commencement, and to believe that in the primordial germ there was the beginning of that which might lead to some form of variation.

Was it possible to hold the evolutionary theory on scientific grounds and at the same time to hold to that religious faith which was to us so immensely precious? He believed that it was. We needed to be very patient with those who were investigating in this matter, for there was much yet to be learned. He believed that there had not only been progress in the arts and sciences, but there had also been in the Dark Ages a loss of knowledge, the full recovery of which would be very valuable in discussing questions of philosophy, science and religion.

Mr. T. B. BISHOP: I feel that we have to thank Professor MacBride for bringing us up to date on the question of evolution, because the war has nearly banished all scientific subjects from our monthly reviews, and I have scarcely seen any criticisms on Professor Bateson's Presidential Address at the British Association's meeting at Melbourne.

I am not sure whether we may not look upon the paper before us as an answer to Professor Bateson's school.

As a layman who is very desirous of more information on the subject of evolution generally, I wonder whether I may ask Professor MacBride a few questions?

On p. 107 it is stated that in some beds in the Karoo Desert of South Africa we have the record of the evolution of a mammal out of a reptile. May I inquire in what book the particulars of this discovery may be found?

(*a*) On p. 108 the case is mentioned of the spotted salamander, and of some plants. I should like to know whether, in Professor MacBride's opinion, these instances do not entirely upset the chief argument of Weismann's book, that against the inheritance of acquired characters?

(*b*) On p. 94 the paper refers to processes *now* in operation, which must inevitably, according to Darwin's view, lead to such an evolution as he postulated.

But is there any proof at all that evolution is *now* in progress?

(*c*) In *The Popular Science Monthly* for June, 1911, there was a paper by Dr. J. Arthur Harris, of the Station for Experimental Evolution at Cold Spring Harbour, New York, describing attempts which had been made by biometric methods, such as those alluded to in the paper on p. 98, to ascertain the intensity of the selective elimination which may occur in nature, and the results were very uncertain. His conclusion was that upon the application of those methods many supposedly valid biological theories have shrunk to nothing, and he says: "Possibly this may be the fate of the natural-selection theory." I do not know what later evidence there may be.

The last paragraph of the paper refers to the Amœba. May I inquire whether any recent researches have shown that the Amœba, which is to be found in all parts of the world, ever evolves into an organism of a higher character?

The article "Protozoa," in the *Encyclopædia Britannica*, concludes by saying that the origin of life is veiled in a mist which biological knowledge in its present state is unable to dispel. But if the Amœba in past ages evolved into higher organisms, what reason can be given why it should not be doing so now?

On p. 102 mention is made of natural species and elementary species, and I gather that by elementary species is intended what by some authors are called sub-species, or even varieties. But Mr. Erich Wasmann, in his book, *Modern Biology and the Theory of Evolution*, propounds the theory of a distinction between what he calls "systematic species" and "natural species," and he looks upon the natural species as having been originally created, and the systematic species as having, often in many thousands, sprung from them; and in this way he thinks that the theories of creation and descent can easily be reconciled with one another.

In connection with this idea I may call attention to a passage in Professor Bateson's address at Melbourne, in which he said:—

> "We should be greatly helped by some indication whether the origin of life has been single or multiple. Modern opinion is perhaps inclining to the multiple theory, but we have no real evidence."

Oskar Hertwig expressed a similar opinion in an address in 1900 on "Biology in the Nineteenth Century" (p. 44):—

> "If we would form an hypothesis as to the descent of the present world of living organisms from simple original cells in the earliest times, the polyphyletic hypothesis has certainly much more probability than the monophyletic."

Dr. J. Reinke, of the University of Kiel, says (*Principles of Biology*, 1909, p. 170 (Heilbronn)):—

(*d*) "Our first question of the evolution theory is whether, after the cooling of the earth's surface, one, several, or very numerous original cells have appeared on it. . . . We must consider it very improbable that only at one single point in the earth one single cell has appeared; the prospect that it would keep alive and multiply would be of the slightest. But if there were several, say even a dozen, original cells, we could not speak of the blood-relationship of all plants and animals; and if several original cells, why not millions?"

Professor Otto Hamann, of Berlin, in a pamphlet on "The Descent of Man," quotes Oskar Hertwig's opinion in support of his own view (if I understand him aright) that there were as many original atoms (as he calls them) as there are species of animals, but this cannot be what Professor Bateson means when he speaks of a multiple origin of life.

I do not know whether I may mention another paragraph in Professor Bateson's address. He says:—

"Modern research lends not the smallest encouragement or sanction to the view that gradual evolution occurs by the transformation of masses of individuals, though that fancy has fixed itself on popular imagination."

Now in Dr. Alfred Russel Wallace's *World of Life*, in replying to an objection of Herbert Spencer's that any variation, to be of any use to a species, would require a number of concurrent variations, he says:—

(*e*) "The argument is entirely fallacious, because it is founded on the tacit assumption that the number of varying individuals is very small. . . . But all these assumptions are the very reverse of the known facts. The numbers of varying individuals in any dominant species (and it is only these which become modified into new species) is to be counted by millions."

May I ask—are we to conclude that modern research has upset this argument of Dr. Wallace?

As regards Mendelism, the origin of variations has always been shrouded in mystery, but the discoveries of Mendel show that, in certain cases at all events, variation is governed by definite natural laws. May we not think it very probable that future research may prove that this is so in all cases? Dr. Wallace, in *The World of Life*, does not appear to attach much importance to Mendelism, but the whole argument of that book tends to show how the progress of the organic world throughout the ages has been governed by natural laws, while still these laws have clearly been controlled by purpose and design.

Mr. M. L. ROUSE said: My brother and I, when we were boys, started with a stock of pigeons of six or seven varieties, including Jacobins and Fantails, in a cote where no enemy could reach them, and we saw with disappointment in eight years all the pigeons assume the plain form and slate-blue colour of the wild sorts, with here and there a white or bronzy quill.

A MEMBER: On p. 94 the Lecturer asks why we claim for species a special creation and not for variations, but surely he answers us on p. 102: "Two natural species either refuse to cross, etc."

Again, it has been proved by Mendelian experiments upon beans, that the curve of error is the same for the beans which have started with a single large bean and have gone on to fertilization as the beans raised from a small progenitor. This shows there is a general tendency to a mean size.

On the other hand, it is found very difficult to keep up a special stock of cattle, for instance, or of seed corn, unless you occasionally introduce other races or varieties. I know that was done years ago with short-horned cattle, and of course it is relations which have these common signs or characters.

As regards the problem of the multiplication of animals, and how it is that such a number are born into the world and so few survive, I may say that in Canada and the States sparrows have spread to the town and have multiplied to a far greater extent; but usually they did not increase to that degree, but surely the Creator intended that the larger animals should feed upon them. The fact that one animal preys upon another is a far better way of disposing of them than if the ground was covered with their dead bodies.

The Rev. M. ANSTEY said the meaning of the word "variation" had never been explained. He doubted whether it had any meaning at all. It was a word used by us to cover up the fact of our ignorance. Like the word "chance," it meant nothing. We all knew that there was no such thing as chance. There was no room for it in a realm of order, in which cause and effect were linked together in indissoluble correllation. Similarly "variation" was a word used to suggest an effect which somehow or other had come into existence without any adequate cause. But such an effect could not be. Consequently there was no such thing as "variation." Hitting the bull's-eye was one thing in respect of which "variation," or failing to hit the bull's-eye, was a word which, if it had any meaning at all, simply meant hitting the target in any one of any number of contradictory directions.

Professor H. LANGHORNE ORCHARD: We are indebted to the author for a most interesting presentation of the present position of the theory of Organic Evolution. But there is an aspect of the case that I should like to urge: we must distinguish between what is proven and what is only hypothesis. If appeal is made to a vivid imagination, a sketch, more or less ingenious, may be drawn of a conceivable evolutionary process. Yet the utmost achievement of that sort of advocacy is to show that, if there be no fact contrary, the thing *might* conceivably have so taken place. But science should not regard such a doubtful possibility as an actuality.

The CHAIRMAN said: I should like to say that, although the existence of a God and of a Creator may be compatible with the acceptance of "Darwinism," I am absolutely convinced in my own mind that the acceptance of what Darwin teaches as to progressive evolution would absolutely compel us to have the Bible written over again. (Several members dissented.) This is my opinion, and I have spent forty years or more in close observation of plant life, and if the progressive development of higher forms from lower forms could be demonstrated as the method or means whereby organic forms of nature have attained their present condition it would, I believe, sooner or later bring everyone of us here to the realization of the fact that this is not what the Bible was intended to say, or does say.

May I mention that some of the speakers, especially the first speaker, seemed to imply that scientific seekers after truth are consciously opposing God's Revelation, and almost seeking to undermine it. I have the greatest sympathy with the man who endeavours to follow the teachings of Pure Science: he is only seeking after Truth, and true science cannot be opposed to God's Truth. Such men as Professor Bateson, the "apostle of Mendelism," Dr. Keeble and others, are absolutely as sincere and honest as ourselves, in their search for Truth. They are seeking step by step to acquire knowledge from a study of Nature itself, and knowledge so acquired must throw an immense amount of light on Revelation. This is a very different method of study to the acceptance of a "theory" and the endeavour to make Nature fit in with that theory. I particularly wish to point out that the Lecturer has repeatedly told us in his paper, when speaking of "Darwinism," that the theory of Organic Evolution stands or falls on the truth, or untruth, of the assertion that characters acquired through the struggle for existence, or by the change of environment, can be, or are being, passed on. If these acquired characters cannot be transmitted there is no possibility of a progressive development, nor of any evolution of the complex from the simple, or of higher organisms from lower. Unless indeed the original form of living matter, assumed by many to have been so simple and structureless, were endowed with all the potentiality of a wonderful variation no change could have taken place; but it must clearly be borne in mind that such a form of Evolution as this was not Darwin's view, and is not what we know as "Darwinism."

With all due deference to the Professor, I claim that no one is able to produce any evidence to-day in the plant world of characters acquired from without being passed on to succeeding generations. The Professor says that a good many cases have been recorded, but we want the evidence, and it is impossible to find this. Professor Bateson, the President of the British Association for this year, some of whose research work I have been privileged to watch, says in his Presidential Address:

"Every theory of Evolution must be such as to accord with the facts of physics and chemistry, a primary necessity to which our predecessors paid small heed. *For them the unknown was a rich mine of possibilities on which they could freely draw.*" (The italics are mine.) "For us it is rather an impenetrable mountain out of which the truth can be chipped in rare and isolated fragments."

Now Mendelism is not based upon an hypothesis (such as the transmission of acquired characters) as Darwinism is. Professor Bateson knows perfectly well that if the evidence he acquires "chipped in rare and isolated fragments" from the unknown, is substantiated, then Darwinism must go, although it has so long "held the field" in the realm of thought. We can only go step by step in the acquiring of knowledge, if we are determined to be satisfied with nothing for which Nature does not afford evidence.

On the question of acquired characters, may I read what Professor Bateson says in his book, *Problems of Genetics*, published in 1912?

"Professor G. Klebs, as is well known to students of evolutionary phenomena, has for several years been engaged in investigations relating to the inheritance of acquired characters. In his many publications on the subject the issue has always been represented as more or less uncertain.

"Desiring to know how the matter now stands according to Professor Klebs' present judgment, I wrote to him asking him to favour me with a brief general statement. This he most kindly sent in a letter dated 8th July, 1912.

"As such a statement will be read with the greatest interest by all who are watching the progress of these studies, I obtained permission to publish it as follows:—

(the letter was in German—the translation I have supplied)

'*8th July*, 1912.

'I will willingly answer your amiable question although I cannot answer it as I desired. Your scepticism in the question of the transference of acquired characteristics to descendants is *only too justified.*

'My experiments with Veronica are *not* conclusive (beweisend), since I have not hitherto succeeded in producing a variety to a certain extent constant, with inflorescence having foliage. In regard to my Sempervivum, I am of course to-day still of the opinion that the strong artificial alteration of the bloom has had an influence on individual descendants. I have hitherto published nothing on this subject, *the majority of the abnormal double flowers were unfortunately sterile.* I obtained some seedlings from a less altered example, but they have not yet flowered. In this case it may only be a question of the subsequent effects (Nachwirkung) in the first generation, comparable to those cases in which seeds of trees from the high Alps show certain subsequent effects in the plain. *But up to the present there is no certain case known in which the character artificially brought about has been transmitted through several generations under the usual "normal" conditions.*

'On the other hand, these negative results are not decisive. For how little serious investigation has really been done in this direction, and doubtless the matter is not so simple.

'I am experimenting with other plants because I am of the opinion that it might be possible to obtain at least such new varieties corresponding to the garden varieties.

'*But up to now the experiments have unfortunately not succeeded either with myself or anyone else.*'" (The italics are mine. A. W. S.)

One word more. As Professor MacBride so clearly states, Mendelism attributes varieties, *not* to inheritance of additional acquired characters, but to *the loss* of some character or characters originally possessed by the plant. Professor Bateson says (speaking as one who had formerly been favourably disposed towards Darwinism): "*We have to reverse our habitual modes of thought.* At first it may seem rank absurdity to suppose that the primordial form or forms of protoplasm could have contained complexity enough to produce the diverse types of life. But is it easier to imagine that these powers could have been conveyed by extrinsic additions?"

Now what does Mendelism in the mind of a Christian student point to, or indicate? Surely that there is the strongest reason possible, from present-day science, for us to maintain that the Bible is correct in teaching that when created forms of life came from the Creator's hand they did so in their present highly perfected forms and not in the shapeless condition which "Darwinism" implies. I do not say that Mendelians assert this, but that we may find in Mendelism a very strong support for what the ordinary man has always believed to be the teaching of the Bible.

The Mendelian, as such, and the "Darwinian," as such, starts with the assumption that the complex and highly developed forms of life around us could not, or did not, commence existence as we see them. Nevertheless "Mendelism" may be taken as indirectly confirmative of the Bible record, and not as destructive thereof, because the evidences which it collects from the contemporary processes of Nature all point to the fact that plants possessing organic life have been able to add nothing to that with which they were originally endowed.

Professor MacBride: It is quite impossible for me to reply to all the interesting criticisms made on my paper.

One set of criticisms are of a type which I may call theological, and another, which interests me still more, are genuine scientific criticisms of the points put forward. May I remind the Society that it was strictly prescribed by Council that I should rigidly put before you the views held by experts and exclude all reference to theology.

I have had a great many questions asked as to whether Weismann's theory that variations could arise by sexual variation could be maintained. This has been entirely exploded.

I have also been asked whether in the process of division of cells an unequal division would give rise to variation. The cell is a secondary thing and of no importance. Take, for example, the egg of the Sea Urchin. It divides into two, and then into four. By artificial means it is possible to separate one quarter and that will develop into a larva of diminished size agreeing in all respects except size with the normal larva of the Sea Urchin—quite perfect, and so one cell can do the work of four.

I have been asked where the account of the evolution of the mammal from a reptile is to be found. Accounts of the wonderful series of intermediate forms between these two groups are being published from time to time in the *Proceedings of the Zoological Society* by two workers, Dr. Watson, of University College, and Dr. Broom.

With regard to Amœba, I was asked, if my views are correct, why it does not evolve now. All naturalists, including Professor Bateson himself, are agreed that there has been evolution. If evolution has taken place the most probable supposition is that it occurred by the spreading of the species into new environment, and in the beginning of things there was plenty of new environment available for simple Protozoa to spread into, but the field is now occupied by the higher forms.

As regards the passing on of acquired characters, Mr. Sutton has handled the variations of plants for many years, and his views are deserving of the highest respect, and I should not have made the statement if several instances had not been brought forward. There is a very interesting article in the *Twentieth Century* by Prince Kropotkin, on the inheritance of acquired variation in plants.

In order to demonstrate this experimentally, a long series of years would be necessary, but all the changes which can be made in our limited time would be very slight.

Lastly, if I had shared the atheistical point of view attributed by the Chairman to some of my colleagues, I should not have taken the trouble to address you. What I have felt for many years very strongly, is that if the good of Christianity is to be experienced over a wider circle, it will be necessary sooner or later that its terms be restated.

I have been pained by the implication of many of my critics that Christianity seems to be wedded to out-of-date statements. God and science were put into opposition. What does God mean to me? It means all this great driving power behind the phenomena which we here call Nature. If God created man He created everything. One way in which God manifests Himself is this regularity of law.

I do earnestly hope that this Society will try and re-think the questions of religion and express them in modern terms, and they will gain a much wider circle of hearers.

The Meeting adjourned at 6.25 p.m.

Further Reply by the Lecturer.

(*a*) In reply to Mr. T. B. Bishop, I would say that if the observations which I have mentioned on p. 108 can be repeated and established, they certainly do upset the chief argument of Weismann's book.

(*b*) It is a fundamental postulate of science that the laws governing Nature are constant and eternal. If evolution occurred long ago, owing to these causes, it must be proceeding now, though slowly.

(*c*) There is a good deal of evidence that superficial differences between members of the same brood, family or litter, due to accidents of nutrition are not inheritable, as was assumed by many Darwinians, but not by Darwin himself. Darwin said that variations existed that were *sometimes* inheritable. Natural selection does not create differences, it only eliminates the unfit.

(*d*) As to the quotation from Dr. J. Reinke, I may say that all this is true. But the fundamental similarity in protoplasm, wherever found, suggests unity of origin. Even if different living cells appeared at once, if they arose in consequence of the same processes operating on the same material there would be a similarity of constitution amounting to blood relationship.

(*e*) In Professor Bateson's opinion, modern research has upset the argument quoted from Dr. A. R. Wallace's *World of Life*, but not, I think, in the mind of the majority of naturalists. We most of us think that the kind of variations with which Dr. Bateson has experimented are not the kind which have played a part in the evolution of natural species.