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892ND ORDINARY GENERAL MEETING

HELD IN THE CAXTON HALL, WESTMINSTER, S.W.1, ON MONDAY, 17TH APRIL, 1950.

HARVEY M. CAREY, ESQ., M.B., B.S., M.SC., D.G.O., M.R.C.O.G., IN THE CHAIR.

The Minutes of the previous Meeting were read, confirmed and signed.

The following elections were announced:—Rev. Vernon C. Grounds, A.B., B.D., Fellow; Pastor G. A. Williams, Fellow; Rev. Frederick H. Squire, F.R.S.A., Fellow; David Widdison, Esq., M.P.S., Fellow; Rev. W. J. Feely, A.B., Th.B., Fellow; Harvey M. Carey, Esq., M.B., B.S., M.Sc., D.G.O., M.R.C.O.G., Fellow; H. G. H. Lillycrap, Esq., Member; R. H. Shalis, Esq., Member; W. Lloyd Pierce, Esq., B.A., Member; A. R. Braybrooks, Esq., Associate; D. J. Smith, Esq., Associate.

The CHAIRMAN then called on Douglas Dewar, Esq., B.A., F.Z.S., to read his paper entitled "Genetics and Evolution."

GENETICS AND EVOLUTION.

By DOUGLAS DEWAR, B.A., F.Z.S.

SYNOPSIS.

The science of genetics, although less than 50 years old, has added much to our knowledge of heredity, because (*a*) geneticists study organisms of which a number of successive generations can be reared in a year, and (*b*) geneticists have greatly increased the rate at which mutations occur in organisms by exposing the latter to X and other rays and mustard gas and other irritants.

It is submitted that the new facts brought to light by genetics are unfavourable to the evolution theory, because (1) geneticists have been no more successful than practical breeders in effecting transformations in the organisms on which they have operated. (2) Geneticists have been led by their work to believe that acquired characters are not inherited, and so have offended the Soviet Government, which will not allow Mendelian genetics to be taught in Russia. (3) The vast majority of the mutations in organisms bred by geneticists are not beneficial ones, and the fact that mutations of the ordinary kind are not only produced but multiplied by X-ray treatment suggests that the mutations are the result of damage to genes or chromosomes. (4) Most genes, although their main effect is on a particular organ, seem also to affect many if not all other organs. This renders it highly improbable that a mutation can be favourable on balance.

Criticism of Goldschmidt's theory that chromosomes and not gene mutations are the causes of evolution.

Comments on the fact that the chromosomes and mitosis appear to be as complicated in protozoa as in the most complicated metazoa.

It is submitted that geneticists are dealing only with the part in heredity played by the nucleus and are neglecting the almost equally important role of the cytoplasm.

It is submitted that the necessity for the genes, chromosomes and cytoplasm to co-operate with one another renders it difficult to believe that all existing organisms are descended from one-celled ancestors.

UNTIL the beginning of the present century scientific and practical breeders were completely in the dark regarding what may be called the mechanism of inheritance. This was the state of affairs when I had finished my course at Cambridge in 1895, although about 30 years previously the Abbé Mendel at Brünn and Charles Naudin at Paris had independently published the results of their experiments on hybridising plants, in which they disclosed the particulate nature of inheritance. Mendel went so far as to enunciate certain " laws " of inheritence. But the work of these men was for many years ignored by botanists and zoologists. Darwin, although aware of Naudin's work, did not appreciate its significance and made no mention of it in his book on variation in animals and plants.

In 1900, however, three botanists—de Vries in Holland, von Tshermark in Austria and Correns in Germany, realised the value of Mendel's discoveries and verified his results. Shortly after this, Bateson and Hurst, followed by Punnett and Saunders in England, and Morgan, Bridges and Sturtevant in the U.S.A., took up the matter on the zoological side.

Thus was established the science to which Bateson gave the name of Genetics. This new science met with bitter opposition, an account of which was given by Dr. Julian Huxley in *The Sunday Times* of July 10th, 1949. Some of the reasons for this opposition will be noticed later. Nothing daunted, the devotees of the new science continued their experiments, and Morgan and his collaborators hit upon the device of breeding insects of which many successive generations can be reared in a year. The creature which has been the subject of most of the experiments is the little fruit-fly *Drosophila melanogaster*, of which the distribution is world-wide.

As early as 1913 the American geneticists announced their conviction that the heredity outfit of every animal is to be found in that part of the nucleus of the germ cell which takes the form of rod-like chromosomes at the time of cell division. By 1925 they announced their belief that the objects that control heredity are arranged in linear series along the chromosomes, much like the beads on a string. These objects are called genes. The chromosomes are visible under the microscope, but it is doubtful whether the genes are distinguishable even under the photon microscope.

At present we can make only approximate estimates of the size of genes. It is estimated that the diameter of a gene is more than 20 and less than 77 mμ (millimicrons). If we take 50 as an average it means that the diameter of a gene is 1/20,000th of a millimetre.

A gene is generally believed to be composed of one (or possibly a very few) complex protein molecule. Schrödinger, who is a Nobel Prize winner, writes (*What is Life?* (1944)): "the gene is probably a large protein molecule in which every atom, every radicle, every heterocyclic ring plays an individual role more or less different from that displayed by any of the other similar atoms or ring," and "the gene is generally believed to be a very complicated molecule. It is probably an aperiodic solid, *e.g.*, every group of atom plays an individual role not entirely equivalent to that of any other . . . a gene contains certainly no more than about a million or a few million atoms." Later, after quoting some authorities, he changed his figure from "million" to "thousand." This last would seem to be the more accurate figure. Obviously the structure of the gene is very important in connection with mutations.

Hundreds of geneticists are now at work, and the reports of their experiments and those of their predecessors fill many volumes. Thus the question arises: are the results of these experiments favourable or unfavourable to the theory of evolution? In my view they are most unfavourable. As many biologists disagree with me, let me set forth briefly the grounds upon which my opinion is based:

1. The experimental work of geneticists and of practical breeders shows that species are very stable and resistant to attempts to transform them, despite the phenomenon of variation.

Practical breeders have been handling our domestic animals for centuries, but no fundamental change has been made in any kind of animal. In the case of some of these domestic animals, notably horses, cats and dogs, we have pictorial evidence that during the past six millennia they have undergone very little modification.

In the case of horses no less an authority than Lady Wentworth declares:

> "The present species has walked on single hoofs and shown the same structure as far back as history can trace him." (*Horses of Britain* (1944).) "Further, early cave and rock pictures show that in the neolithic period both the heavy-boned northern type of horse and the lighter southern type existed in Europe. The former exhibited one large and two small varieties, while the southern type is depicted in European rock pictures as a speckled pony: the pure Arabian appears only in the rock paintings of Arabia (where it is often depicted as galloping with a rider carrying a spear) and of Egypt (1800 B.C.), where it is shown both ridden and driven." (*The Authentic Arab Horse and His Descendants*' (1945).)

Similarly ancient pictorial representations show that in ancient Egypt, fully 6,000 years ago, several breeds of domestic dog existed, one of which, of greyhound type, was used for hunting deer, another breed had short legs like a dachshund, a third had pendent ears.

We know from the pictures that the oldest domestic animals were asses, oxen, sheep, goats, pigs, dogs, cats, geese and ducks.

The earliest known pictures of domestic animals show that none of them has changed much, each domesticated species was 4,000 years ago as sharply marked off from all other kinds of animals as it is to-day.

It is true that in the case of animals bred for amusement rather than utility many freaks have been produced by man. Darwin made much of this, asserting that if some of the pigeons bred by fanciers had been found in the wild they would have been deemed new species or even genera, and he argued that if man in a few centuries can produce by selection such forms natural selection working during millions of years could have effected vastly greater changes.

Darwin, however, knew nothing of the effect on the body of the secretions of the ductless glands, and shut his eyes to the fact that these freaks are quite incapable of maintaining themselves in nature, monstrosities resulting from gland unbalance; yet, despite their abnormality, they clearly bear the stamp of the wild ancestor. Thus fantails, pouters, jacobins, barbs, tumblers, swallows, trumpeters, etc., all bear the hallmark "pigeon." Moreover, all these breeds, when crossed or when mated with the parent form, yield fertile offspring.

The work of geneticists confirms that of the practical breeders. In the animals on which the former have experimented they have produced many freaks and monstrosities (some of these will be noticed later), but, as in the case of the domestic animal freaks, these are all clearly members of the wild species from which they have been bred. It is true that geneticists have been at work for less than fifty years, but in most cases they have experimented on animals which in the laboratory produce a number of successive broods in a year. Thus in the case of the little fruit fly *Drosophila melanogaster*, on which the majority of geneticists have worked, 25 successive generations can be reared in a year, so that some 1,000 generations have been bred in the laboratory. Assuming that the generation time for man is twenty-five years on an average, or 40 generations in 1,000 years, it would require 25,000 years to perform this experiment on man. Nor is this all, Müller discovered in 1927 that by irradiating this fly with X-rays the rate at which mutations occur is increased about fifteen thousand per cent. Needless to say, these flies and other creatures experimented on have been freely X-rayed during the past 20 years. In consequence the number of mutations which have been produced in the laboratory has been vastly increased. But the mutations so produced are all of the same kind as those which occur in untreated individuals. This is true of the mutations induced by other rays and mutation-inducing chemicals, such as mustard gas.

"Experiments on several types of organisms," writes R. D. Evans (*Science* (1949), vol. 109, p. 304) "have shown that irradiation can produce gene mutations. These induced mutations are not novel types, but appear to be entirely similar to those which occur spontaneously."

Another interesting discovery made by geneticists is that the average rate per generation at which spontaneous gene mutations occur is substantially independent of life span. Thus as many mutations are likely to occur in a fruit-fly in its life-time of 14 days, as in that of a horse which lives for as many years. At the Bicentennial Conference on Genetics, Palaeontology and Evolution held at Princeton University in 1946 Professor Haldane said, "the order of magnitude of the mutation rate per generation in man is about the same as that of *Drosophila melanogaster*, although the mutation rate per day in man is only a five-hundredth that of *Drosophila*."

As Professor Sturtevant pointed out at the same Conference, it is hard to determine natural mutation frequencies because spontaneous mutations are usually extremely rare events. According to Evans (*op. cit.*) the rate is of the order of 10^{-5} to 10^{-6} per gene per generation.

When considering the results of genetical work it is desirable to bear in mind that *Drosophila*, on which so much work has been done, is an unusually variable genus, even for an insect. Of the birds, the biggest known genus is *Zosterops*, of which 67 species have been described. In the case of insects, however, a genus of this size is not unusual. Several hundred species of *Drosophila* have already been described. M. D. T. White writes of it: "It is quite probable that when the Drosophilids of the more remote parts of the world have been properly studied the genus may be found to contain well over a thousand species. We may regard it as a flourishing group which is probably evolving fairly rapidly at the present time." (*Animal Cytology and Evolution* (1945), p. 124.)

2. The experimental work of geneticists seems to show that the effects of use and disuse are not inherited, nor are characters acquired by an individual during its life-time. This is the view of nearly all geneticists to-day, outside Russia.

The prevailing view is thus stated by H. J. Müller, who was awarded the Nobel prize for his work as a geneticist: "Genetics has adduced cogent evidence that, despite the strong influence of environment in modifying the body as a whole, and even the protoplasm of the cells, the genes within the germ cells of the body retain their original structure without specific alterations caused by the modification of the body, so that, when the modified individual reproduces, it transmits to its offspring genes, unaffected by its own acquired characters. The offspring will not tend to repeat the parental modifications unless the same peculiar environment is itself repeated." (Article "Variation," *Ency. Brit.*, vol. 23, p. 988.)

As a little reflection should render it clear that if neither the effects of use and disuse nor acquired characters are inherited, the theory of evolution is impossible, it is surprising that the majority of geneticists in English-speaking countries seem still to accept the evolution theory. The attitude of these is most illogical. The French seem to realise this and in consequence few of their biologists are geneticists. It is significant that the article on genetics in the French *Encyclopaedia* is by E. Guyénot, a professor at the University of Geneva. Dr. A. Labbé, a professor at the School of Medicine at Nantes, and an ardent transformist, writes :

> "Genetics, which is consecrated to the study of heredity, has become a kind of religion, dogmatic, mystical, intolerant, which has its temples, its priests, its believers, its councils, and which aims at converting all the biologists in the world. For it transformism may still exist in theory, but in practice the very fact of transformism is incomprehensible. However, the geneticists still call themselves transformists ; just as in politics where the left and the right parties each claim exclusively the epithet republican. Without being deliberately opposed to these genetical ideas, nevertheless I cannot accept them without many reservations, and, in common with most French biologists, I cannot admit even the foundation of genetics other than as a possible, but unproved entity. Genetics ends inevitably in a more or less complete negation of evolution : at the most it can conceive of fortuitous variations. . . . We do not want this genetics which hampers us. . . . It is only when the laws of the transformation of species will be better known that we can attack the problem of heredity. Let us then set aside genetics which leads us either to the strict fixity of species or a relative variation which is not evolutionary." (*Le Conflit transformiste* (1929), p. 140.)

To the logical biologist, there are only two alternatives, either to reject evolution, or to fly in the face of genetical evidence and believe that acquired characteristics are inherited.

Not many biologists accept the first alternative. One of the few who do is Heribert Nilsson, of Lund University, who is a botanical geneticist. He writes : "It is obvious that the investigations of the last three decades into the problem of the origin of species have not been able to show that a variational material capable of competition in the struggle for existence is formed by mutation. Further, as it has also been impossible to demonstrate a progressive adaptation by means of the transmission of acquired characters (all the numerous experiments made have yielded negative results), we are forced to this conclusion that *the theory of evolution has not been verified by experimental investigations of the origin of species* "(italics his). He continues : " Is then biology without evolution conceivable?" He replies : " Just as affinity in Chemistry or Mineralogy need not be based on the assumption that the elements evolved from one another, from Hydrogen to Uranium, there is no more need of our basing the related series of biology on an evolution from amoeba to *Homo* and so on." (*Hereditas*, vol. XX (1935), p. 236.)

The second alternative was adopted by the late Professor E. A. MacBride in England and, under the orders of the Soviet Government, by all Soviet biologists.

MacBride sought to eat his cake and have it too, by being a geneticist and at the same time asserting that acquired characters are inherited. Indeed he went so far as to the head chapter VIII of his *Evolution* (Benn's Sixpenny Library, No. 109 (1927)): " Inheritability of Habit as the Real Cause of Evolution."

The view of the Soviet Government is thus set forth in an editorial article in *Izvestia* of September 8th, 1948, by Kaftanof, Minister of Higher Education in the U.S.S.R. :

" There are two opposite trends in biological science. One of them is progressive and materialistic, called Michurin's theory . . . the other is the reactionary, idealistic Weismann's or Mendel-Morgan theory. In opposition to the Mendel-Morgan trend Russia developed and, encouraged by the Soviet régime, brought to its full bloom, the great theory of the great modifier of nature, I. V. Michurin.

" Michurin's materialistic theory has been continually enriched by the works of his followers, with the academician T. D. Lysenko at their head. This trend in biology has developed into a mighty current which has taken hold of the masses. It inspires millions of collective farmers with faith in the creative power of their efforts and gives them a firm assurance in the realisation of new successes in the field of abundancy of farming products.

" The Michurinists have proved, not by word but by demonstration, that it is possible to direct the inborn qualities of animals and plants in a desired manner. Michurin's theory has adopted and developed the best sides of Darwinism. Darwin had explained the evolution of animals and plants from the materialistic point of view. Michurin has developed this knowledge and taught methods of directing the process of producing new species of plants and new species of domestic animals, thus transforming Darwinism into a really practical creative doctrine. . . . Thanks to the care of the Bolshevist party and of the Soviet Government, as well as to the personal care of our great leaders, Lenin and Stalin, Michurin's theory has been preserved from oblivion and has become the property of the people. The efforts of Michurin's followers led by the academician T. D. Lysenko have brought it to a new height of achievement. . . . The last session of the U.S.S.R. Lenin Academy of Agricultural Sciences . . . has brought to light the opponents of Michurin's doctrine in biology and has dealt a stunning blow to the reactionary Weismann-Morgan theories." Then he gives a list of the Russian geneticists who were deprived of their posts. Among these are I. I. Schmallhausen who "denies the inheritance of acquired characters and finds that evolution depends upon mutations which originate directly in the germ cells of the organism and have a quite accidental and indeterminate character, not regulated by the conditions of its life. This idealistic, reactionary theory is fundamentally antagonistic to Darwin's teaching. Nevertheless Schmallhausen always hid under the banner of Darwinism. . . . All biological chairs and faculties must be held and supported by qualified Michurinists. . . . We must have textbooks based on the progressive Michurin theory. . . ." (*Science*, Jan. 28th, 1949, pp. 3 *et seq.*)

All this is most discreditable to the Soviet authorities and is injurious to scientific progress, and has elicited justifiable protests from British and American biologists. But some of these protests have been almost hysterical and unnecessarily violent, notably Dr. Julian Huxley's attack in *Nature*, and the broadcast by Dr. C. D. Darlington in December, 1948. Possibly some of this acerbity is because Lysenko and his followers are treating the biologists with whom they disagree very much in the same way as British and American biologists treat those who reject the evolution theory. Anyone who rejects transformism is as unlikely to be given a biological appointment in an English-speaking country as one who asserts that acquired characters are not inherited is to be given a biological post in Russia. Both the Soviet authorities and the British biological authorities are trying to stamp out opponents of evolution, and the Soviet authorities regard geneticists as the enemies of evolutionism. *Hinc illae lachrymae* !

3. The vast majority of mutations are the reverse of beneficial; indeed a large percentage are lethal, *i.e.*, they lead to the early death of the animal in which they occur.

Let me quote a few authorities in support of this statement. Mr. E. B. Ford writes: "It may be said that all genetic factors which have arisen by mutation in the laboratory have certain peculiarities in common. It seems that they are nearly always associated with some lowering of vitality as compared with the wild-type form, and the more marked their effect the more deleterious seems to be their action. They appear to be concerned with the production of small superficial differences or with obviously pathological departures from normality which could not in any event survive in a state of nature. Further, nearly all are recessives. . . . It may, in short, be stated that no mutation has ever occurred in the progress of genetic work which is fully viable and behaves as a dominant to the wild type condition. That any have given rise to changes which would be of survival value in nature appears highly doubtful." (*Mendelism and Evolution* (1936), p. 43.)

Mutants of the shrimp *Gammarus* "would have but little chance, in normal conditions of nature, of survival through the early critical period. Each new mutation has shown greatly lowered vitality during its earlier generations, accompanied by marked abnormalities in breeding." (Sexton, Clark and Spooner, *Jour. Marine Biol. Assn.* (1930), p. 189.)

Gene-mutations are "generally injurious" (*Genetics* (1931), p. 14.)

Robson and Richards (*The Variations of Animals in Nature* (1936), p. 222) write of *Drosophila*: "We have taken the list of 389 mutations given by Morgan, Bridges and Sturtevant in *The Genetics of Drosophila* (1925), and analysed them as far as possible with the following result:

Lethal	90
Defective	120
Viability poor	16
? Defective	9
Uncertain or normal	114
Eye colour	40

Speaking generally, it may be said that nearly 60 per cent. of the mutants are certainly defective, and a certain small percentage is normal."

Notice that not one of these mutations is described as beneficial or good. Nevertheless, in my opinion, Robson and Richards have under-estimated the number of bad mutations. As the result of a perusal of *The Mutants of Drosophila melanogaster*, by Bridges and Brehme, which was published in 1944, I wrote: "These mutations are almost all what may be called *loss mutations*, all are defective in some way, thus over 100 mutations of wings have been recorded, in all of which the wings are defective or reduced to stumps or absent." (*Is Evolution Proved?*" (1947), p. 187.)

Professor J. B. S. Haldane challenged this assertion (*Is Evolution a Myth?* (1949)) but when I invited him to name some good mutations, he was able to cite only some black mutants of *Drosophila melanogaster*, which are more resistant to drought and insecticides than is the wild type. But many of these stocks show low viability—a serious defect, so the best that can be said of them is that, like the curate's egg, they are good in parts!

J. H. Müller asserted: "Most mutations are bad, in fact good ones are so rare that we may consider them all as bad." (*Time*, November 11th, 1946, p. 46.)

The best proof that mutations are almost invariably bad is the fact that X-ray treatment causes abundant mutations of the kind that occur normally, and the evidence indicates that these rays act by displacing or knocking out atoms in the molecules of the genes on which they impinge, in other words these rays cause damage to the genes.

This is what Schrödinger, a Nobel Prize winner, has to say in this matter (*What is Life?* (1944)): "The mutations are actually due to quantum jumps in the gene molecule" (p. 34), and: "We shall assume the structure of a gene to be that of a large molecule, capable of only discontinuous change, which consists in a re-arrangement of the atoms and leads to an isomeric molecule. The re-arrangement affects only a small region of the gene, and a vast number of different re-arrangements may be possible. The energy thresholds separating the actual configuration from any possible isomeric ones have to be high enough (compared with the average energy of an atom) to make a change-over a rare event. These rare events we shall identify with spontaneous mutations. . . . We may safely assert that there is no alternative to the molecular explanation of the hereditary substance. The physical aspect leaves no other possibility to account for its permanence. . . . It is conceivable that an isomeric change of configuration in some part of our molecule, produced by a chance fluctuation of the vibrational energy can be a sufficiently rare event to be interpreted as a spontaneous mutation. Thus we account, by the very principles of quantum mechanics, for the most amazing fact about mutations, the fact by which they first attracted de Vries's attention, that they are 'jumping' variations, no intermediate forms occurring . . . X-rays, so to speak, cause explosions. That in many cases the effect of the explosion will not be an orderly isomeric transition, but a lesion of the chromosomes, a lesion that becomes lethal when by injurious crossings the uninjured partner (the corresponding chromosome of the second set) is removed or displaced by a partner whose corresponding gene is known to be itself morbid—all that is absolutely to be expected and is exactly what is observed" (p. 66).

It is easy to understand how X-rays can break a thread-like chromosome, or eject an atom, or disturb atoms in the gene molecule. But treatment with mustard gas seems to be as effective as X-rays in producing mutations, and I find it difficult to see how the impact of a mustard gas molecule on a gene molecule can produce the same effects as bombardment by X-rays.

Experiments. however, show that the effects in the two cases are not exactly the same. Auerbach, Robson and Carr give an account of some of these differences. They write ("The Chemical Production of Mutations," *Science*, 1947, pp. 243–7): "After X-ray treatment of males (of *Drosophila melanogaster*) most of the mutated offspring show the induced abnormality (such as the yellow body colour instead of the normal grey) over the whole surface of the body. Only a small proportion (less than 15 per cent.) of the mutated individuals are mosaics (*i.e.*, show the abnormality in a part of the body, the remainder being normal). In the progeny of the mustard-gas treated males, on the other hand, the mosaics form a high proportion (usually between 30 and 50 per cent.) of all mutated individuals."

It is also found that bombarding by X-rays is more likely than treatment with mustard gas, to break the chromosome thread.

As X-rays and mustard gas are both destructive agents and as the mutations they produce are identical with those which occur in animals not subject to special treatment, I submit that the belief that the accumulation of successive mutations in natural conditions can in course of time gradually transform one type of animal or plant into a higher or more complex type, is on a par with the belief that the aerial bombing of a town composed mainly of huts and small cottages can in time transform it into a town composed of large houses, churches and warehouses.

4. Another fact, which in my view is most unfavourable to the evolution theory, and which writers on genetics are apt to slur over, is the large number of genes which co-operate to produce quite trivial features. For example, as Stern admits (*Genetics, Palaeontology and Evolution* (1946)): "No less than 30 genes co-operate in forming the actual colour of the eye of the adult *Drosophila*." There is nothing peculiar in this, "each character has been found by geneticists to depend on many genes for its realisation." Now Müller estimates that there are only 1,800 genes in *Drosophila*. From this it follows that if each gene operates in connection with only one character, the number of genes possessed by *Drosophila* is quite inadequate for the realisation of all its characters. Therefore geneticists have to believe that most, if not all, genes affect a number of characters. As Stern puts it: "The conclusion follows, therefore, that in general there is no simple one-to-one relation of gene to character, or of character to gene. Development of organisation, character and organism must accordingly be envisaged as consequences or products derived from multidimensional networks of genic interactions."

Müller goes even further. "There is reason to infer," he writes (Article "Gene," *Encyc. Brit.*, vol. 15, p. 1000), "that every gene contributes to every part of the body, affecting some parts more than others, and it is these that are picked out for convenience in studying heredity."

As a mutation seems to involve the dislocation or disturbance of at least one of the atoms in one of the molecules of the gene affected, the resulting mutation is likely to affect all the organs or features on which that gene acts, and the odds must be enormous against this effect being favourable on all or most of these organs. So that the odds are enormous against the mutation being a good one. Stern certainly does not overstate this when he writes: "Because there is such a complex interplay among genes, mutations or hereditable changes in genic structure and action will generally be disadvantageous to the organism already in possession of a well-adjusted genotype (or collection of genes). So also a deterioration or pronounced change of environment may put the organism with a formerly well-adapted genotype at a disadvantage, because genes interact not only with each other but also indirectly or directly with the environment. For these reasons a change in the action of the genes without environmental changes, or change in the environment without genic change, or change in both genes and environment may be expected to make for an unbalance even though the system was formerly a well-adjusted one." Being a good evolutionist, Stern then proceeds to make the best of a bad job. He writes: "Nevertheless, gene mutation is a *sine qua non* of evolution, and environmental changes inevitably occur and make new demands upon the organism, so there must be situations in which genic or environmental changes are tolerable to the organism during those periods in which new genotypes are being subject to selection or new environments explored."

The last part of the above passage is typical of the transformist's outlook. He starts off with the assumption that evolution has occurred, and so has to assert that highly improbable events *must* have happened!

In another attempt to overcome this difficulty Professor H. S. Jennings of the Johns Hopkins University, U.S.A., writes (*The Biological Basis of Human Nature* (1930), p. 322):

> "When we see gene mutations in experimental breeding, have we before our eyes the process that has resulted in progressive evolution?
>
> If all such mutations are destructive or disadvantageous, they cannot be the material of progressive evolution. Some investigators have therefore expressed the opinion that in gene mutations we are witnessing merely the disintegration of the genetic system, the breaking down of organisms, not their upbuilding; we are observing the 'wrecking of the train,' not its construction. The method of progressive evolution would then be completely hidden from us.
>
> To this it is answered that it is not known that all gene mutations are disadvantageous. For many of the mutations producing slight changes, there is no indication of harmful effects. There are even certain conspicuous alterations which, it is practically certain, are not disadvantageous. Different colours in rabbits and rats arise by mutation: there appears to be no evidence that they result in decreased vigour. The diverse eye colours in man must originally have arisen by mutations: presumably blue eyes (since they are recessive)

from darker eyes. Yet there is no indication that differences in vigour go with diverse eye colours.

It was to be anticipated that most changes in the materials of the genetic system, so drastic as to cause a sudden large alteration in the structure or physiology of the organism would be harmful. But the case is different with respect to the much more numerous mutations causing very slight effects. Many of these too may be harmful, but some of them may not. Some of them may well make the individual more efficient under the conditions in which it lives. Even if but a small proportion of them are thus advantageous, this is sufficient. Individuals with these rare beneficial mutations will multiply, gradually supplanting those without the mutations. After a time a large proportion of the stock will consist of the individuals bearing the advantageously modified genes."

The above is clearly wishful thinking on the part of Jennings. It may be soothing syrup to some. How much more soothing to the evolutionist would be an example of a mutation which is clearly advantageous!

Dr. Richard Goldschmidt, Professor of Zoology at the University of California, occupies an isolated position among geneticists because he asserts that the gene mutations (on which they set such store, and which he calls micro mutations) can lead only to evolution within the species, *i.e.*, can produce only varieties, races and sub-species. He sets forth his views thus (*The Material Basis of Evolution* (1940), p. 6): "I cannot agree with the viewpoint of the textbooks that the problem of evolution has been solved as far as the genetic basis is concerned.

"This viewpoint considers it as granted that the process of mutation of the unit of heredity, the gene, is the starting point for evolution, and that the accumulation of gene mutations, the isolation and selection of the new variants which afterwards continue to repeat the same process over again, account for all evolutionary diversifications. This viewpoint, to which we shall allude henceforth as the neo-Darwinian thesis, must take it for granted that somehow new genes are formed, as it is hardly to be assumed that man and amoeba may be connected by mutations of the same genes, though the chromosomes of some Protozoa look uncomfortably like those of the highest animals. It must further be taken for granted that all possible differences, including the most complicated adaptations, have been slowly built up by the accumulation of such mutations. We shall try to show that this viewpoint does not explain the facts, and we shall look for explanations which might evade these and other difficulties and simultaneously account for such facts as have to be pushed in the background to make the popular assumptions plausible. At this point in our discussion I may challenge the adherents of the strictly Darwinian view, which we are discussing here, to try to explain the evolution of the following features by the accumulation and selection of small mutants: hair in mammals, feathers in birds, segmentation of arthropods and vertebrates, the transformation of the gill-arches in phylogeny including the aortic arches, muscles, nerves, etc.: further, teeth, shells of molluscs, ectoskeletons, compound eyes, blood circulation, alternation of generations, statocysts, ambulacral system of echinoderms, pedicellaria of the same, cnidocysts, poison apparatus of snakes, whalebone, and, finally primary chemical differences like haemoglobin vs haemocyanin, etc. No one has accepted this challenge! Corresponding examples from plants could be given."

Goldschmidt devotes the first 185 pages of the book named above demonstrating that gene mutations cannot account for the origin of new species, much less of higher categories. He concludes this part of his book thus (italics his) (p. 183): "Micro-evolution by accumulation of micromutations—we may also say neo-Darwinian evolution—is a process which leads to diversification strictly within the species. . . . *Sub-species are actually, therefore, neither incipient species nor models for the origin of species.* They are more or less diversified blind alleys within the species. The decisive step in evolution, the first step towards macroevolution, the step from one species to another, requires another evolutionary method than that of sheer accumulation of micromutations."

Goldschmidt devotes the last 200 pages of his book to macro-evolution. This part of his book is an anticlimax, in that the only cause of evolution that he can suggest is change in the way in which the genes are arranged in the chromosomes: these changes he calls systematic mutations to distinguish them from changes in the genes themselves. He asserts (p. 203) that the facts have led him to believe that "a pattern change in the chromosomes, completely independent of gene mutations, nay, even of the concept of the gene, will furnish this new method of evolution."

This is a startling announcement because the ways in which chromosomes can be repatterned appear to be few : (1) A section of a chromosome may get broken off or detached and then re-attach itself to the same chromosome at some other point, or it may attach itself to another chromosome, and in either case it may attach itself with its original front end in front or at the back, so that the linear order of the genes is reversed (Inversion). (2) The detached section may not re-attach itself to another chromosome, and so add to the number of chromosomes, although the total length of all the chromosomes will not be increased.

In these two instances there is no increase or diminution of the number of genes, or in the structure of any of these. The only alteration is that many of the genes change their neighbours. (3) The detached section of the chromosome may get lost and cease to form part of the gene complex. This entails a loss of genes, otherwise no change. (4) A chromosome, or all the chromosomes may not split up longitudinally at cell division, so that the number of chromosomes becomes doubled and each gene becomes duplicated. This is the condition known as polyploidy, which is uncommon in animals but often occurs in plants ; indeed many of the flowers produced by horticulturists are polyploids. This tends to increase the size of the plant affected, and may result in the formation of new species, but these are all of the same type as the normal parent. The loss of genes that occurs in (3) above, at the best may mean an unhealthy plant ; more often it has a lethal effect.

As the repatterning of chromosomes is effected by X-rays and mustard gas, it, as in the case of gene changes, appears to be of a pathological nature, and it is difficult to believe that a succession of pathological changes can convert an amoeba into a starfish or any other class of viable animal.

Apart from this, so far the experimental work of geneticists seems to negative this hypothesis. Numerous experiments show that the repeated inversions and duplications which seem to have occurred in chromosomes have had very little effect on the body form of the species in which they are exhibited. Thus there are two races, known as A. and B. of the fly *Drosophila pseudobscura*, very alike in appearance, despite the fact that their chromosomes exhibit a number of differences, indeed, greater differences than those between the species *D. melanogaster* and *D. simulans*. Nor is this all ; within each of these two races the chromosomes exhibit considerable diversity.

"Tan and Koller," writes M. J. D. White (*Cytology and Evolution* (1945), p. 100), "have shown that the two races differ in at least four inversions in each limb of the X, one in the 2nd and one in the 3rd chromosome. Within each race, however, the gene-sequences are not constant, since a number of different inversions are present. The 3rd chromosome of *pseudobscura* seems to be especially variable: a total of 21 different inversions are now known in this chromosome. Seven of these are found only in race B., 13 only in race A., while one (known as 'standard') occurs in both. As far as the other chromosomes are concerned, five sequences are known in the 2nd chromosome, two in the 4th, while in the X three are known in the 'right' limb and two in the 'left' one. The unusual variability of the 3rd chromosome is quite unexplained, but Helfer has shown that all the chromosomes are equally fragmented by X-rays in proportion to their length." White adds (p. 101): "The morphological differences between the A. and B. races are so slight that they cannot be detected except by careful measurements and statistical analysis. The sharpest difference recorded is in the wing-beat frequency. The mating between the different chromosomal types appears to be at random."

Again *Drosophila miranda* and *Drosophila pseudobscura* are very alike in external appearance, yet their chromosomes are quite different, and their hybrids when produced are completely sterile. Dobzhansky and Tan have estimated that if they be derived from a common ancestor there must have been about 100 breaks in the past in their chromosomes. These are not peculiar cases. Goldschmidt himself writes: "From the work on intraspecific chromosome changes we know that inversions and re-arrangements may occur without having any noticeable effect, even when they are accumulated."

Moreover even losses of parts of chromosomes or additions to or duplication of chromosomes may have very little effect on external appearances. J. B. S. Haldane (article "Heredity" in *Encyc. Brit.*) mentions that "individuals of *Drosophila melanogaster* which have lost one of the pair of small chromosomes are viable but small." Further, the presence of a third small chromosome has little apparent effect on the creature. In animals the augmentation of the number of chromosomes is very uncommon, but it occurs frequently in plants, when the number may be doubled or further augmented. The result of such multiplication is usually an increase in size of the plant in question, but no fundamental change seems to be effected. No amount of multiplication of the chromosomes will turn a rose into something which is not a rose or a hemp-nettle into something which is not a hemp-nettle.

Against all this evidence Goldschmidt has not adduced a single instance where it can be shown that chromosome re-arrangement has resulted in the production of a new type of organism. All that he can do is to assert that this must have happened in the past because of the great differences between the various classes and other large groups!

Goldschmidt attempts to get over the fact that chromosome changes in all the cases genetically investigated do not result in considerable change in the body thus (p. 206): "This new pattern seems to emerge slowly in a series of consecutive steps. . . . These steps may be without any visible effect until the re-patterning of the chromosome (repatterning without any change of the material constituents) leads to a new stable pattern, that is, a new chemical system. This may have attained a threshold of action beyond which the physiological reaction system of development, controlled by the new genetic pattern, is so basically changed that a new phenotype emerges, the new species, separated from the old one by a bridgeless gap and an incompatible intrachromosomal pattern. 'Emergent evolution' but without mysticism! I emphasise again this viewpoint, cogent as it is and, in my opinion, necessary to an understanding of evolution, is to be understood only after the fetters of the atomistic gene theory have been thrown off, a step which is unavoidable but which requires a certain elasticity of mind."

The above passage shows the effect of the belief in evolution on the human mind. Goldschmidt realises that the gene mutation theory cannot account for evolution, so he discards this theory and replaces it by a far less tenable one. It never occurs to him that evolution may not have taken place.

Moreover, as chromosome mutations are induced by X-rays and other irritants, just as gene mutations are, a great many of the former must have occurred in the laboratory while the geneticists have been at work. Therefore, if Goldschmidt's theory were true, many viable new genera should by now have been bred in the laboratory. The fact that this has not happened is fatal to Goldschmidt's theory.

So far nothing approaching an adequate cause of evolution has come to light.

5. Of the facts brought to light by the geneticists and cytologists one of the most unfavourable to evolutionism is that the chromosomes of the simplest organisms appear to be as complicated as those of the highest animals. "The chromosomes of some Protozoa," writes R. Goldschmidt (*The Material Basis of Evolution* (1940), p. 6), "look uncomfortably like those of the highest animals."

The process called mitosis, whereby a cell divides into two is so complicated that, in my view, it cannot have been developed by the blind forces of nature. This process is described in all elementary books on cytology and genetics. An excellent easily-accessible account is given by V. H. Mottram in the chapter "The Chromosome Ballet," of his *The Physical Basis of Personality* (Pelican book, A.139).

Karl Belar writes (article, "Protozoa," *Encyc. Brit.*, vol. 18, p. 626): "In all groups of the Protozoa we recognise to-day the occurrence of true mitosis, as complicated in every way and indeed often much more complicated than in multicellular animals. . . . In no case can we say that the method of nuclear division in the Protozoa is simpler or more primitive than in the higher animals and plants: the chromosomes of the Protozoa are no fewer than and show in most cases the same peculiarities as those of multicellular organisms."

This does not mean that the chromosomes of all animals are very alike in appearance. In fact they exhibit great variety in number, form and size; but there seems to be little, if any, connection between these features and the kind of animal in which the chromosomes occur. As regards number in the generative cells, the thread-worm *Ascaris* has 1 chromosome, and at the other end of the scale the moth *Phigalia* has 112: *Drosophila melanogaster* has 4, the rabbit 22, man 24. Twenty-four is quite a common number, it occurs in perhaps the majority of placental mammals, in several birds, some snails and amphibia, but it has not been found in any marsupial mammal or lizard or fly.

As to how the genes produce their effects I can only hazard a guess, *viz.*, that each gene manufactures a chemical compound or enzyme which stimulates the surrounding cytoplasm to develop in a special direction. The cytoplasm, in turn, influences the nature of the enzymes produced by the genes which it surrounds. Thus does the undifferentiated cytoplasm in each cell become differentiated into the form it exhibits in the cell of the adult.

But every cell in the developing embryo contains all the genes of the species. The cells in the legs contain the same genes as the cells in the eyes. Why then do the eye-inducing genes not produce eyes in the leg, or the leg-inducing genes produce legs in the eye region ? The reason seems to be that *each gene can only fulfil its organ-inducing function when it is surrounded by the right kind of cytoplasm.* To produce an eye two factors are essential : the genes which secrete the necessary enzymes and the cytoplasm which has become differentiated in the eye-direction, *i.e.*, acquired the power of developing into part of an eye under the proper stimulus.

The foregoing remarks make it clear that, in my opinion, geneticists are dealing only with one aspect of the problem of heredity, *viz.*, the part played by the chromosomes and the genes ; they pay little attention to the role of the cytoplasm, which constitutes by far the greater part of the ovum and of every other living cell. The reason for this procedure on the part of the geneticists is, I think, that the behaviour of the chromosomes is easy to watch through the microscope, while that of the constituent parts of the cytoplasm is difficult, if possible, to make out. Dr. C. H. Waddington in a broadcast talk in August, 1949, rather naïvely said that the " few thousand particles known as genes are the most important things which are passed on from the parent to the offspring." He considers that the genes are more important than the cytoplasm " because, if the genes in an animal are abnormal, then the adult which develops will be abnormal, whereas we find very few abnormalities or peculiarities that can be traced back to changes in the rest of the egg." (*Listener*, August 25th, 1949.)

It does not seem to have occurred to Dr. Waddington that if anything goes wrong with the cytoplasm, the ovum will fail to develop, or that the cytoplasm may be much more stable than the genes and chromosomes.

To speak of the genes being more important than the cytoplasm is on a par with saying that the walls of a house are more important than the foundations on which they are built. The cytoplasm is the foundation of the edifice which we call an organism. It is the medium in which the genes exist and from which they derive their sustenance. In a cell which has just been produced by division the chromosomes swell by taking in material from the cytoplasm; later some, at any rate, of this material is returned to the cytoplasm, after it has been changed chemically in some way.

I am not alone in believing that the cytoplasm plays a far more important role in heredity than most geneticists will allow. Conklin, Loeb, Jenkinson, Russell and Sonneborn have all stressed the great importance of the cytoplasm.

Without accepting all E. S. Russell's conclusions I may say that I consider his *The Interpretation of Development and Heredity* (1930) a most valuable book.

Dr. H. J. Jennings, although he does not seem to go so far as Russell is, I submit, almost certainly correct when he writes:

> "The cytoplasm is the medium in which the genes live and operate. It is modified, transformed by the action of the genes, so that at the later stages of development the cytoplasm differs greatly from that which was present in the earlier stages. This changed protoplasm reacts anew with the genes, causing these now to change their action, resulting again in new cytoplasmic products. This continues until ultimately the diverse tissues and organs of the adult body have been produced as a result of changes in the cytoplasm. . . . The cytoplasm is the material out of which the parts of the diversified body are manufactured, through interaction with the genes. But in the development the cytoplasm is not passive; it reacts upon the genes, and what the genes do, what they produce is largely determined by the nature of the cytoplasm in which at various stages of development they find themselves." (*The Biological Basis of Human Nature* (1930), p. 78.)

Clearly then the genes in the ovum of a crustacean are surrounded by cytoplasm of a very different nature to that of the cytoplasm in which the genes of a mollusc, or of a vertebrate are placed.

This, I contend, is the reason why the genes of a Protozoan, in conjunction with the surrounding cytoplasm, produce an animal having neither skull, limbs, vertebrae, pelvis, eyes, ears, snout, teeth, mouth, brain, nerves, heart, blood, blood vessels, intestine, liver, spleen, etc., while those in the cytoplasm of the ovum of a vertebrate, in conjunction with the cytoplasm, produce an adult having all the above things.

If the cytoplasm of vertebrates be derived from that of Protozoa by a process of evolution, it is surprising that no one has made a plausible suggestion as to what has effected the difference between the end-products of the two kinds of cytoplasm.

The genes and the chromosomes work in co-operation with the cytoplasm; without such co-operation the development of the fertilised ovum would either go awry or fail to take place.

If, then, the great groups of many-celled animals and plants evolved from one-celled ancestors as the result of successive mutations, all these mutations must have changed both the genes and the cytoplasm in such a manner that, despite these changes, the genes not only continued to act in unison with the cytoplasm, but acted more successfully and so produced more and more complicated organisms.

The idea that mutations of this description not only took place, but were caused by unidentified natural forces, is, I submit, fantastic. Some nineteen hundred years ago St. Paul said, "All flesh is not the same flesh: but there is one kind of flesh of men, another flesh of beasts, another of fishes, and another of birds." To-day I think we can go farther and say that all cytoplasm is not the same cytoplasm: but the cytoplasm of each class of animal differs from that of all the other classes.

Discussion.

Dr. Harvey M. Carey (Chairman) said: The old chemists watching a fire propounded the "Phlogiston Theory of Combustion," which postulates that when combustible material burns phlogiston is driven off, leaving behind the calx or ash. Their observations were correct, but their deductions based on these observations were in error because of their ignorance of the underlying mechanism.

Biologists have made accurate observations, but their deductions should be accepted with caution until the underlying mechanisms are understood. This applies particularly to evolutionary concepts. It is to the subject of genetics that one must look for a satisfactory explanation of the basis of evolutionary change if indeed this has occurred or is occurring. The old Lamarckian concept of the inheritance of acquired characters appears to be largely discarded in most circles in view of the failure to marshal any real experimental evidence in its favour. Mendelian variation has been shown to be limited in its scope, so that the evolutionary theory has fallen back on to the concept of mutation in an attempt to find a satisfactory "modus operandi."

The fact that the great majority of mutational changes appear to be abstractions rather than additions of features can be readily understood from a consideration of the underlying biochemical changes. Mutations occur spontaneously, but their rate of appearance can be accelerated by gamma radiation, mustard gas, etc., which possess in common the capacity to alter the chemical character of the conjugated protein molecule which is the basis of the gene. It is an elementary chemical principle that small changes in the chemical composition of a molecule, such as the oxidation of a reactive group or radicle, will rob a substance of its specific properties. Evidence has not been forthcoming of the production of a molecule with the capacity of inducing the development of new morphological features which differ qualitatively from already existing structures and which are not merely degenerations or quantitative modifications of these features.

Even if this problem is solved, it must still be demonstrated how a number of new characters appear simultaneously or in a compatible sequence, and how at every stage of the process the physiological integrity and survival value of the individual is maintained. This herculean task has not been satisfactorily discharged by the most competent brains in evolutionary circles.

Mr. G. E. Barnes said : I should like to thank Mr. Dewar for his very able summing-up of the present position with regard to the relation between genetics and evolution and, at the same time, to add a few comments on his paper.

With regard to section 1, I should like to ask Mr. Dewar what he means by the "hallmark" or "stamp" of a pigeon. If he is judging from morphology, then Darwin's claim is perfectly correct that many of the varieties of domestic pigeon are sufficiently different from the wild Columba livia to warrant their being put in separate species or genera, had their ancestry been unknown. The fact that fertile offspring are produced when two of these varities are crossed is no longer regarded by taxonomists as a criterion of their belonging to the same species.* The fact that the immediate cause of the

* *See* Calman, *The Classification of Animals* (1949), ch. 3.

varying characters is in some cases an endocrine one does not imply that there is not some more fundamental genetic cause which appears in successive generations. And surely, the fact that in certain environments these varieties are incapable of maintaining themselves does not preclude the possibility that in other environments (*e.g.*, geographical isolation) they would be capable of so doing. It seems to me therefore that Darwin's argument from human selection to natural selection is still relevant. If taxonomists had no knowledge of the ancestry of the varieties of pigeons, they would, I believe, regard them as separate species or genera.

Mr. Dewar states in section 2 that "if neither the effects of use and disuse nor acquired characters are inherited, the theory of evolution is impossible." Surely this is not true. Almost all biologists outside Russia would agree that acquired characters do not affect the genetic constitution of the individual, and therefore do not influence its offspring (and this is all that Müller is saying in the passage that he quotes), but that does not mean to say that the environment will not have a direct modifying effect on the individual's genes. Such effects are well-known, and have been shown repeatedly to influence the offspring. Thus environmental effects may be represented as follows :—

ENVIRONMENT.

food, training, etc. ↙ ↘ X-rays, drugs, random heat effects, etc.

Somatic changes. ——×——→ *Genetic changes.*
(Acquired characters.)

Affecting individual and not inheritable. | Inheritable and therefore modifying the race.

To deny, as most would, the inheritability of acquired characters is merely to erase arrow X in the above scheme, but this does not prevent evolution.

At the end of section 4, Mr. Dewar concludes that Goldschmidt's theory is untenable, because chromosome mutations have never, so far, produced viable new species in the laboratory. This does not necessarily follow. The laboratory is normally a very stable environment, and if new species have not developed in the laboratory, that is no reason why they should not develop in a changing environment such as occurs in nature. No genetic pattern can manifest its effects in somatic characters unless it is given a suitable environment, and an environment which is suitable for one genetic pattern may be very different from one which is suitable for another. New patterns may be arising continually in the laboratory, but the chances are that the laboratory environment which is suitable for the original type may not be suitable for the new patterns, and they will therefore not be observed as viable new species. Had those same changes occurred, however, in a constantly changing environment, it is much more likely that new types would have arisen.

There are some small points relating to section 5. It is perfectly true that " each gene " can only fulfil its organ-producing function when it is surrounded by the right kind of cytoplasm. But what determines that the right kind of cytoplasm should be present at the right time ? The answer, in the case of most developing embryos (regulation eggs) and possibly in all, is earlier gene activity. So we are brought back again to the importance of the genes. I do not disagree at all with Mr. Dewar's timely warning against forgetting possible cytoplasmic factors, but I believe that the weight of evidence is in favour of the overwhelming importance of the genes rather than of the cytoplasm. Geneticists are, of course, aware of cytoplasmic factors when they talk of " plasmagenes." A slight inaccuracy, which, however, does not affect the argument, is the statement that " the cytoplasm constitutes by far the greater part of the ovum and of every other living cell." Among living cells which possess more nuclear material than cytoplasmic material are mammalian monocytes and many spermatozoa.

Towards the end of his paper, Mr. Dewar writes, " If the cytoplasm of vertebrates be derived from that of Protozoa by a process of evolution, it is surprising that no one has made a plausible suggestion as to what has effected the difference between the end-products of the two kinds of cytoplasm." I do not think it is surprising at all. The neo-Darwinist (and most biologists are neo-Darwinists) would consider the explanation, genetic mutations, so obvious that it need hardly be stated.

I would make one general remark about the whole problem. It may be very dangerous in discussing the problem of the relationship between genetics and evolution (as it has proved in other biological problems) to argue from very carefully controlled laboratory conditions to events in nature. Nearly all genetical work has taken place in the laboratory, whereas nearly all evolution, if it has in fact occurred, has occurred in nature.

Dr. R. J. C. HARRIS said : Mr. Dewar has tended to support his argument, in some places, with inaccurate data. These inaccuracies do not affect the argument decisively but must not be allowed to pass unnoticed.

It is biochemically naïve to talk of a " gene molecule." The gene itself seems to be nothing more precise than a locus on a chromosome and the addition of " molecule " to its properties has, except for the physicist, nothing to support it. The danger lies, not in the assumption that the gene is *composed* of atoms, but in the hypothesis that the gene *is* a " molecule." This *may be* merely another way of stating the hypothesis that the " structure " of each gene is unique but the point is that once the gene begins to be endowed with the properties of a molecule, then it can be postulated to react chemically with simple molecules, such as the mustard gas mutagen, or to mutate as a result of " quantum jumps in the gene molecule " (Schrödinger). No analytical data have ever been put forward for a gene. The chromosomes, however, appear to consist of protein and nucleic acid.

Mr. Dewar finds it easy to understand *how* X-rays can break a thread-like molecule, but it is precisely this question of the mechanism of action of X-rays and chemical mutagens that is, at present, engaging the attention of geneticists, physicists and biochemists ! The physicists, like Schrödinger, tend to support the " quantum jump to a gene isomer " hypothesis whereas the biochemist tends to look for evidence of chemical interference with gene function. *If* the genes exert any specific autosynthetic, or heterosynthetic, function (and the very interesting work of Tatum and others with *Neurospora*, suggests that they have, in this organism, a direct relationship with the cytoplasmic enzymes), then the biochemists's explanation becomes reasonable. It is a misrepresentation to suggest that it is " the impact of a mustard gas molecule on a gene molecule " which has been put forward by

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geneticists to explain the action of chemical mutagens. Geneticists are quite willing to accept chemical transformation of the gene as a cause of gene mutation. Many geneticists, in fact, are still quite content to consider genes as "beads" on a chromosome "string." The recent progress in elucidation of gene action has largely been the result of the fertilization of genetics by biochemistry.

The chemical mutagens are usually reactive substances and, in view of the chemical complexity of the cell, there seems to be no reason why such molecules should pass unchanged through the cytoplasm and act selectively on the genes. Equally, since the cell is a unity, there is no reason why an interference with cytoplasmic enzyme systems should not produce, as an end-result, interference with gene function.

As Mr. Dewar so cogently points out, the cytoplasm has enjoyed a greater importance among geneticists in recent years. This is almost certainly because it has been shown (by biochemists !) to possess a structure which is in every way as interesting as the structure of the nucleus and equally as important [*see*, *e.g.*, Brachet, *Growth* 1947, **11**, No. 4, pp. 309–324].

T. H. Morgan [*Embryology and Genetics*, New York, 1934, p. 10], postulated a mechanism for differentiation in embryonic development.

Initial differences in the chemical composition of the cytoplasm affect the genetic activity of nuclei which are primitively identical. These modified nuclei, in turn, affect the cytoplasm and induce its differentiation. This hypothesis is now, as indeed in 1934, largely unproved but experimental evidence in support of it is accumulating.

[Brachet, *L'hypothése des plasmagènes dans le développement et la différenciation.* Unités Biologiques douées de Continuité Génétique Centre National de la Recherche Scientifique, 1949.]

WRITTEN COMMUNICATIONS.

The BARONESS WENTWORTH wrote : I have a long unpublished article on the subject beside what has been published in *Thoroughbred Racing Stock.* The more I consider it the more fantastic I think the conclusions as to *horse* evolution.

Major Keyloch's theory that "no horse can inherit any characteristic from either of its parents but only from its grandparents" proves how misleading a study of genes and chromosomes may be in practical breeding where theory is sometimes flatly contradicted.

Accepted facts :—

(1) No grey horse can be produced unless one at least of its parents is grey (grey includes all stages preceding white).

(2) Two greys can sometimes produce a bay.

(3) Grey cannot be inherited from a previous generation having been once lost, or occur sporadically.

(4) Two bays can produce a chestnut, but two chestnuts can only produce chestnuts.

It will be noted in the following breeding example all four grandparents may be grey, but have no influence on their grandchildren's colour :—

Bay	Bay Horse	Grey Horse	The bay is inherited directly from the parents and the colour of the grandparents never re-appears.
		Grey Mare	
	Bay mare	Grey Horse	
		Grey Mare	

Dr. O. R. BARCLAY wrote : I do not wish to comment on Mr. Dewar's main thesis, but I do feel that I must comment on his statement on pp. 156 f. that "if neither the effects of use and disuse nor acquired characters are inherited, the theory of evolution is impossible." To quote authorities in 1927 and 1929 to prove this seems hardly relevant because, as he admits, genetics was in its infancy then and, one may add, was often misunderstood. Surely Mr. Dewar must admit that most of the ablest evolutionists and geneticists to-day find no impossibility here. Mr. Dewar does not discuss theories of the "evolution" of dominance or of beneficial effects in mutations which on first appearance are recessive and harmful. These theories may seem to be highly improbable but they are surely possible.

In any case Mr. Dewar himself seems to believe that natural mutation may by natural or artificial selection produce limited but permanent changes within a species ; this surely is evolution on a very small scale. It may be that such a mechanism seems incapable of accounting for large-scale changes or constructive changes, but surely he cannot go further than to say that it seems so improbable that we ought to dismiss the suggestion. A slight overstatement here could spoil the force of his argument altogether, and ignores much ingenious speculation on the part of learned men.

Mr. R. T. Lovelock wrote: This excellent and interesting paper has proved doubly valuable firstly as being a simple summary of an involved subject presented by an expert in language which a " layman " may understand, and secondly as being a presentation of the case against evolution from one who knows as much of the biological detail as do the protagonists of that theory. I found the development of the idea that in the cytoplasm we have an agent equally potent with the genes in the mechanism of heredity particularly provocative of thought, since it has long been evident that if the Bible be true, the activities of Mendel did not lay bare the whole of the story.

The Bible contains throughout its length, as an underlying presumption, the idea that " sin " is a transmissible taint, while in some places (*e.g.*, Matt. 23 : 29–33) the transmission would seem to be genetical rather than environmental. It is obvious that if only Mendelian laws are concerned with inheritance, in no sense can the sin of Adam effect the personal content of his descendants, and if we are to accept the Bible principle on this point, we must believe that genes as entities constitute only a part of inheritance. While science is still in ignorance of the detailed construction of genes and their mechanism, I have always felt at liberty to ignore the argument from ignorance and believe that a mechanism as yet unknown was in operation which ensured the transmission of some factors dependent on this life. Mr. Dewar has indicated that in the cytoplasm we have an agent which might well cause similar genes to result in differing structures, and until the full nature of these elements is known the argument of " Mendel " can never be advanced against the Bible teaching. It is, of course, recognised that no experimental evidence for such an idea has yet been found, and the Russian teaching is a piece of political expediency.

One point arising from this second factor in inheritance might well have been more forcefully indicated. When the laws of Mendel received their first grudging recognition from science in this country, Bible protagonists hastened to accept them, jubilantly pointing to all the difficulties which had been assembled for the followers of Darwin ; it would have been more in keeping for them to " do as they would be done by " and realise that the whole picture was not then known, and that argument from ignorance is no more sound in biology than it has been proved to be in archæology. Mr. Dewar might well have stressed a little more the implications of his point about cytoplasm—that this additional factor should prevent any honest protagonist from using the limitations of mutation as a point against evolution; there is lack of evidence for, but that can never become evidence against, until we know more of the mechanism. Even so acute a mind as that of Dr. Clark has made this error in a recent work (*The Universe: Plan or Accident?* pp. 97–99), when he argues that since natural selection will never accomplish a gradual transition from one useful form to another, and we know that all such transition must be such by gene mutation, therefore natural selection can never accomplish evolution. We do not know what cytoplasm can do, nor whether it is liable to discontinuous "jumps" akin to mutation; if, however, it were, and Mr. Dewar was right in supposing that a change in it could completely alter the form originated by a given gene, then such transition would become a possibility.

As science has progressed it has served to reveal in increasing detail how God works, and natural law is but the name for those parts of divine intervention which we understand more clearly than the rest. We know that in the past God has given rise to various forms of life; increased accuracy in time measurements has already caused the term "explosive evolution" to come into use. We might expect that sooner or later increased biological knowledge will begin to illustrate the nature of difference between types and suggest how they arose under God's direction. For the sake of the Bible's reputation it is well to see that we are not found supporting error when that time comes.

Dr. E. S. Russell wrote: I have read Mr. Dewar's paper with great interest and appreciation. His critical account of the gene theory is most valuable; it is up-to-date and brings out very clearly the weaknesses and limitations of the gene hypothesis. His criticism of Goldschmidt's theory of the repatterning of chromosomes is devastating.

I entirely agree with him that evolution cannot be explained in terms of the gene-natural-selection hypothesis. This may account for the origin of intra-specific races, and possibly in some cases of species, but it is quite incompetent to explain how the larger steps in evolution came about—the formation of new classes, orders or families. My own view is that without some form of directive and creative variation which is cumulative from generation to generation there could have been no large-scale evolution.

As to the transmission of acquired characters, I believe that this has played an important part in certain forms of evolution, those namely that lead to adaptive radiation, especially in Vertebrates. Some very cogent evidence has recently been adduced by Wood Jones (*Habit and Heritage*, 1943) in favour of this transmission. It does not, however, seem to be the sole key to evolutionary differentiation, much of which is not of the nature of adaptive specialisation.

I agree with Mr. Dewar that the importance of the cytoplasm has been underestimated, but I would go further and suggest we should not consider nucleus and cytoplasm separately, but as mutually interdependent constituents of a real unity, the cell as a living whole.

That evolution has taken place seems incontestable, but we know extraordinarily little as to the way in which it has come about or as to its "causes" or "factors." It may be that a metaphysical rather than a purely scientific theory is required.

Professor T. Dobzhansky wrote : Free expression and discussion of opposing theories is doubtless important and beneficial for the progress of science. However, discussion is profitable chiefly when the area of agreement between the opponents is much greater than that of disagreement. Unfortunately, I must take exception to almost every opinion on biological matters expressed in Mr. Dewar's highly provocative paper. I shall restrict myself to a single point which, however, occupies a rather central position in Mr. Dewar's argument, namely, that since no mutations definitely beneficial to their carriers are known, the modern theory of evolution falls to the ground. I submit that the opposite is true ; the genetic theory of evolution would be embarrassed if anyone were to observe the origin of a mutant superior to the ancestral type in the environment in which the latter normally lives. All types of mutation occur with finite frequencies and, accordingly, the probability that we can observe a mutation arising for the first time in the history of the species is negligible. Mutants which are useful in the normal genetic and secular environment have, by and large, already become incorporated into the normal genotype of the species or race by natural selection. But we have ample evidence that a mutation which is deleterious in a certain environment and on a certain genotypic background, may be useful in another environment or in conjunction with other genes. To give but one example, Spassky and the writer exposed to a stringent selection some strains of Drosophila flies whose viability had been reduced below normal by certain mutations. During fifty generations of selection, the viability of most of the strains rose to normal again owing to selection of mutations favourable to the altered genotypic background. Classification of mutants as " useful " or " deleterious " is quite meaningless unless the nature of the genetics and secular environments is stated. If we had no other evidence that evolution has taken place, observation on the behaviour of mutants would lead us to construct a theory of evolution.

Dr. JOHN HOWITT wrote : This paper by Mr. Dewar is, like all his writings, full of interest to the student of evolution. Genetics is the laboratory of evolution as geology is the history, and it is amazing to discover the actual results of laboratory experiments in this field. " Natural selection " and the " survival of the fittest " were catch phrases that captivated the imagination of an earlier generation. But in the laboratory of genetics these concepts have yielded only negative results, as Mr. Dewar has pointed out. In a recent article Dobzhansky (*Scientific American*, January, 1950, p. 35) refers to certain experiments conducted by himself and B. A. Spassky in which they intentionally disturbed the harmony between an artificial environment and the fruit flies living on it. He states that at first the change in environment killed most of the flies, but during fifty consecutive generations most strains showed a gradual improvement of viability. He concludes as follows : " Most mutants that arise in any species are, in effect, degenerative changes, but some, perhaps a small minority, may be beneficial in some environments." This is a far cry from the survival of the fittest and illustrates the almost unbelievable extent to which geneticists are forced to retire in order to support a theory which is obviously false.

Lt.-Col. L. MERSON DAVIES wrote : This is a most timely paper, which should open the eyes of people impressed by the claims of evolutionary geneticists. I am particularly interested to see that Mr. Dewar stresses the *rôle* of the apparently structureless cytoplasm; for I insisted on this same matter when I reviewed Dr. Joseph Needham's work on *Biochemistry ond Morphogenesis* (1942) at the request of the editor of *The Nineteenth Century and After.* In my remarks (*ibid.*, Vol. CXXXIV, for August, 1943, pp. 77, ff.) I pointed out (pp. 82–84) that Dr. Needham could only claim to find "a number of stimuli (*alias* enzymes, catalysts, hormones, genes, organisers, evocators, etc.), which either activate structures or cause them to appear"; and I insisted that this was not enough, since the stimulus was "the least significant part of each problem" for, as "every student of mechanisms, especially living ones, must realise, the explanation of most reactions lies far more in that which reacts than in that which causes the reaction. To depress a switch or turn a knob may 'evoke' any kind of result, according to the mechanism concerned. It is not the switch, but the attached mechanism, which decides whether the result will be to produce light, heat, wireless sounds, start an engine or fire a gun. In living structures the distinction between stimulus and stimulated is still more marked; and the effects, say, of adrenalin, are far less explicable by its own relatively static and simple composition than by the far more complex living organism which both produces it and draws on it in moments of emotional crises."

I then showed how, as regards developing structures, the "subordinate nature of the *rôle* played by the stimulus" was "indicated by one of Needham's own diagrams" (his Fig. 42) "and remarks regarding it"; for there the essential guiding structure which Needham had to postulate as deciding the course of events by "determination of parts to pursue fixed fates" was wholly unknown and unidentified. Since, in the initial zygote (fertilised cell), the chromosomes and their genes are only supposed to be stimuli, I asked: "Where, then, is the real mechanism? Is it in the seemingly structureless cytoplasm?" This question was never answered, either by Needham himself or by his colleagues at Cambridge, to whom separates of the paper were sent.

The more we try to solve the mystery of organic structures the more vast and impenetrable does that mystery seem to be. As I pointed out, the multiple facts which Dr. Needham emphasised regarding the growth of the individual only increased the difficulty of explaining them, since "all the colossal programme of its vastly intricate development, and the whole life cycle—together with arrangements for the unlimited continuation of the type—are packed away in a minute cell, and must apparently be located just where there is no sign of any structure at all," *i.e.*, in the cytoplasm.

The fact that evolutionists will not face such considerations, shows how they fight shy of *basic problems*, and magnify secondary discoveries as if they solved matters instead of actually increasing their mystery.

Dr. A. MORLEY DAVIES wrote: My knowledge of genetics is very superficial. As with Mr. Dewar himself, my own biological training ended at a time when genetics were in a very rudimentary state, and I have only followed their development in casual reading. I am therefore unable to contribute any comments of value. I recognize that Mr. Dewar makes some very reasonable criticisms.

Professor HERIBERT NILSSON (Lund, Sweden) wrote: Thank you for the copy of Mr. Dewar's paper. However, I am now busy finishing off the work I began in 1940 on Speciation, and so have no opportunity of making the comments I should otherwise have been only too pleased to submit. But I can say this, that my attitude to the Evolution question agrees entirely with that of Mr. Dewar.

AUTHOR'S REPLY.

In reply to Mr. Barnes, in my view every variety of pigeon shows the hallmark pigeon in its comportment, carriage, gait, flight, and its behaviour in the presence of another of its species. I have no doubt that Mr. Barnes would recognise as a pigeon any new breed shown to him, just as he would recognise as a dog any mongrel dog, even if of a type he has never seen.

The fact that taxonomists unacquainted with the origin of, say, the fantail pigeon might class it a species or even genus different from that of the blue-rock simply shows that form is not an infallible criterion of a species. This test is on the whole satisfactory when applied to wild animals, but is apt to fail when dealing with freaks, incapable of holding their own in the wild, bred by breeders.

As regards my assertion that the theory of evolution is impossible if neither the effects of use or disuse or acquired characters are inherited, Mr. Barnes rightly says that, if these factors are eliminated, there remain various other agents which cause gene and chromosome mutations. But I contend that the apparently random mutations caused by these agents are incapable of converting into a Vertebrate a Protozoan, which lacks eyes, ears, nose, legs, heart, liver, spleen, pancreas, bones, muscles, nerves, bloodvessels, no matter how much time is allowed. I see no reason to-day to modify the following assertion I made nearly twenty years ago : " There appears to exist no mechanism whereby a new type of organism can arise from an existing one" (*Man : A Special Creation*, p. 55). Shuffle *ad infinitum* all the constituent atoms in the molecules, the molecules in the genes, and the genes in the chromosomes of a protozoan and the result will still be a protozoan. I may add, that, in my view, even if the effects of use and disuse can be inherited the theory of evolution is impossible ; if they are not inherited the impossibility is palpable.

As to the laboratory being normally a very stable environment: this is not so in the case of that of a geneticist, whose object is to induce as many mutations as he is able. Much time and energy have been spent on trying to discover mutagens. Apart from radiations of several kinds, chemical means of inducing mutations have been adopted. Some account of this work is given by Auerbach, Robson and Carr in their paper " The Chemical Production of Mutations." They tell us that the search for chemical mutagens has been going on for well over 20 years. . . . Iodine, ammonia, metal compounds, and carcinogens are only some out of the great number tested. These geneticists have been working with four kinds of mustard gas. I submit that Drosophila flies have been subjected to a far greater variety of environments since 1916 than they experienced during the whole period of their existence before that year.

As to the relative importance of the nucleus and the cytoplasm, I think that geneticists are beginning to realise that the latter has more say in the matter of heritance than has hitherto been believed. With all respect to Mr. Barnes I do not consider that the suggestion that the difference between an amœba and an elephant is accounted for by the piling up of gene mutations is plausible. I agree that the fact that geneticists have not produced a new kind of animal does not necessarily mean that there has not been evolution in nature. But I do think that the results of their work indicate that it is highly improbable that all existing animals and plants are descended from one-celled ancestors.

I am greatly indebted to Dr. Harris for showing us how the biochemist's idea of the gene differs from that of the physicist. As most of us, including myself, are not *au fait* with the latest work of biochemists and physicists I hope that the Council of the V.I. will induce Dr. Harris to give us a paper on Biochemistry and Evolution and ask a physicist to favour us with a paper on Physics and Evolution.

As no one has ever seen a gene, I am glad that Dr. Harris emphasised that in the case of this as in that of the atom and of the molecule we can at present only theorise as to its nature. Dr. Harris goes so far as to say that the gene seems to be a locus on a chromosome ; would it not be preferable to say " an entity occupying a definite locus on a chromosome " ?

To my way of thinking a locus is simply an area of space which may or may not be occupied by some object, so that it has no property save emptiness, and its only function could be to serve as a resting place for some physical object. Thus, as the gene appears to have a potent effect on the formation of organs, unless it be non-physical, it must be composed, like all matter, of atoms held together, and consist of at least one molecule, and apparently a very complicated one.

I am sorry to have made it appear that geneticists believe that mutations may be caused by the impact of a mustard gas molecules on gene molecules. The idea is mine. As treatment with mustard gas induces mutations similar to those induced by X-rays I try to visualise how it is that the effects are similar.

Dr. Harris shrewdly remarks "there seems to be no reason why such molecules (*e.g.*, mustard gas molecules) should pass unchanged through the cytoplasm and act selectively on the genes." Does not the same problem arise in the case of X-ray and other irradiations ?

In conclusion I hope that Dr. Harris will, in the paper I suggest, tell us about the recent discoveries regarding the structure of the cytoplasm. I was led to stress the importance of this on considerations other than its structure.

Lady Wentworth's remarks are a valuable antidote to the writings of enthusiasts, such as L. J. Langdon-Davies, who greatly overestimate the practical value of the work of geneticists. Langdon-Davies writes (*Russia Puts the Clock Back*, p. 50): "Poultry farming, too, has benefited from genetics. . . . Thus to begin to lay eggs early a chicken must possess two special genes in its chromosomes." If, by looking at a chicken one could know that it had or had not these valuable genes, this knowledge would be of great practical value. In fact its value is only academical. Lady Wentworth, as an experienced horse-breeder, cannot but feel that geneticists have afforded little help to horse breeders. It is the latter who set problems for the geneticists. W. E. Castle writes ("The ABC of Color Inheritance in Horses," *Genetics*, vol. 33, 1948): "The horse genes have been given special names, as they have been discovered, and it is not easy to correlate these with the better-known names and symbols used by experimental breeders." Occasionally a black horse has a bay foal. As this fact is not easy to account for on the assumption that only four genes, A, B, C and D, control the colour of the coat, some geneticists postulate an extra gene E. to account for this.

Doubtless some horse-breeders have found Mendel's laws helpful and they seem to afford a plausible explanation of some of the facts revealed by breeding operations.

In reply to Dr. Barclay, when I speak of the theory of evolution being impossible I refer to the theory that all living organisms are descended from a single-celled ancestor or a few such ancestors.

I think it confusing to apply the term evolution (or even micro-evolution as Goldschmidt does) to changes within the species of which no biologist since the time of Linnæus denies the possibility.

I would limit the term evolution to changes which have resulted in the formation of new families and all the larger groups of organisms ; I would call changes that take place within the species, genus and family differentiation.

I would describe as Creationists those who believe that all the changes in organisms that have taken place in the past are those that come within the category differentiation.

I quote MacBride, Labbé and Heribert Nilsson, not as proof tha the theory of evolution is impossible, if the effects of use and disuse and acquired characters are not inherited, but as biologists who do realise this. The contribution of the last-named to this discussion bears out my contention in his case.

I know not whether or not most of the ablest evolutionists and geneticists "find no impossibility of evolution," even if the effects of use and disuse are not inherited. They certainly write as if they believed in evolution. Possibly in some cases this belief is the result of what they were taught as students, and they have not considered the matter since. Others may be merely following the fashion, as seems to be the case with some French zoologists, for Paul Lemoine (when summarising, in his capacity as editor, the contents of the volume of the *Encyclopédie Française* dealing with living organisms) wrote : " The result of this exposition is that the theory of evolution is impossible. In reality, despite appearances, no one any longer believes in it, and one speaks, without attaching any importance to it, of evolution to denote succession, or more evolved in the sense of more perfected, because it is the conventional language, admitted and almost obligatory in the scientific world. Evolution is a kind of dogma in which the priests no longer believe, but which they keep up for their people." So far as I am aware no protests against this statement appeared in French periodicals.

For my part I find it difficult to believe that Dr. Barclay, or any other biologist who has studied the question, believes that in the course of time an amœba-like protozoan can have evolved into a vertebrate solely as a result of (1) losses of atoms in the genes, (2) successive rearrangements of the atoms of the molecules of which the genes are composed, (3) losses of or rearrangement of these molecules, (4) loss of chromosomes or parts of them, (5) division or union of chromosomes, (6) repeated rearrangements of the genes in the chromosomes.

In order to realise what belief in such evolution means, let us consider a living amœba and a living man. *Ex hypothesi* each of these is descended from an amœba-like ancestor endowed with certain genes. Let us assume that this common ancestor lived 1,500 million years ago and that one line of its descendants has terminated in the amœbas now living in England and a second line has terminated in the men living in England. The genes in the living amœba and men are direct descendants of the genes of an amœba which lived 1,500 million years ago. In the case of the amœba all the random mutations of the genes in its lineage from the ancestral form have, so to speak, cancelled each other out, so that the amœba is morphologically indistinguishable from its 1,500 million years-old ancestor, but the mutations in the line of ancestors of man first changed the amœba into a metazoan, say an echinoderm, next they converted this into a fish, later they changed the fish into an amphibian, and then transformed this last into a reptile, and later turned this into a mammal, and finally into a human being. And this human lineage while undergoing all its amazing transformations contrived to hold its own in the struggle for existence.

Mr. Lovelock rightly points out that if the cytoplasm plays an important part in heredity, it is open to evolutionists to say: "The fact that gene and chromosome mutations are very limited would not show that evolution is impossible; we do not know what the cytoplasm can do, or whether it is liable to discontinuous 'jumps' akin to mutation." But this contention affords the evolutionists little help, because the cytoplasm is as much exposed to the action of the external forces which cause mutations to occur, as the genes and chromosomes are, and the meagre results of the work of breeders and geneticists show that such "jumps" occur very rarely, and the few that are known to have occurred, such as that which resulted in the ancon sheep, seem to have been of a pathological nature.

In my view Mr. Lovelock's criticism of Dr. Clark is not well founded, and the latter is right in citing as a most serious objection to the theory of undirected evolution the fact that the synthesis of arginine in *Neurospora* involves seven stages, some of which involve the production of substances apparently quite useless to *Neurospora*, and each of these substances is the product of a different enzyme which is itself dependent on a particular gene.

I am greatly beholden to Dr. E. S. Russell for his kind remarks and for having, by his *The Interpretation of Development and Heredity*, led me to suspect that geneticists are concentrating too much on the cell nucleus. I subscribe to his view that the sound course is to consider nucleus and cytoplasm as a real unit and the cell as a living whole.

However, I do not agree that Prof. Wood Jones has adduced cogent evidence of the transmission of acquired characters. A perusal of *Habit and Heritage* gave me the impression that the case for such transmission is very weak. The author was able to cite only four instances of what he believes to be such transmission :—

(1) The facets on the leg and the ankle bones of Asiatics.
(2) The cervical curve in the backbone of Weddel's Seal.
(3) The reversal of the ordinary direction of body hairs in some marsupials.
(4) The single uterus of Primates.

In the case of (1) and (3), I suggest that Wood Jones puts the cart before the horse. Seeing how uncomfortable it is for a European to squat on his haunches for any length of time, I think that the Asiatic habitually assumes this posture because his leg and ankle bones are provided with these facets. It may well be that kangaroos and Koalas use their combs in the way they do because of the direction of the hairs to which these are applied.

As regards (2), Wood Jones assumes that the neck curve of the seal is a consequence of its land ancestors having taken to water and so having to hold up the head when swimming as a dog does. To my mind the notion that any quadruped ever gradually got its legs fettered as they are in seals and sea lions is fantastic. (*See Is Evolution a Myth*? p. 49.)

As to (4), I cannot believe that the single uterus of the Primates is the consequence of the rupture at every birth for untold generations of the partition separating the *culs-de-sac* of the bifid uterus of the ancestors of the Primates.

Professor Dobzhansky's and Dr. Howitt's communications reached me by the same post. As Dr. Howitt's is in effect a reply to Professor Dobzhansky, it enables me to shorten my remarks.

Prof. Dobzhansky's assertion that "the genetic theory of evolution would be embarrassed if any one were to observe the origin of a mutant superior to the ancestral type in the environment in which the latter normally lives" is exhilarating, coming as it does after Professor J. B. S. Haldane's gallant attempt to prove that good mutations are not uncommon. I take it that Professor

Dobzhansky contends that every race or species now existing has become so well adapted to its environment, as the result of natural selection, that all the mutations now showing themselves are more or less harmful, and the most harmful are weeded out by natural selection, which thus keeps the race in a static or unchanged state, and this will continue until changes in the environment occur which will render useful some mutations hitherto harmful, and natural selection will then lose no time in incorporating these mutations into the genotype of the species. After this change the species will remain static until a further change in the environment converts harmful mutations into useful ones.

On this view evolution would seem to be an extremely slow process—a series of minute steps with a long rest between each step. But Professor Dobzhansky makes it clear that in his view the environment is almost as changeable as the weather in England. He writes "the environment is never constant; it varies not only from place to place but from time to time" (*Scientific American*, January, 1950, p. 36).

While disagreeing with Professor Dobzhansky regarding the stability of the environment, I agree that a mutation harmful in one kind of environment might be useful in another kind and so be preserved in the latter as, for example, an insect on a windswept island in which a wingless mutation occurs. But this does not alter the fact that the mutation is what may be called a loss mutation, and it is difficult to believe that evolution can be the result of loss mutations or of mutations that were originally harmful and only became beneficial owing to environmental change. Such mutations would seem to result in devolution rather than evolution.

The very interesting experiment of Dobzhansky and Spassky may mean, as the experimenters believe, that the mutation which lowered viability in the first environment slightly improved it in the new. But I submit that an alternative explanation is possible, *viz*, that in the highly selected fly population exposed to the new environment a new mutant arose which increased viability or nullified the effects of the gene or genes responsible for the low viability of the original population.

I may remark that some geneticists do not share Professor Dobzhansky's optimism. Dr. Carl C. Landegren of Southern

Illinois University writes, (*Scientific American*, March, 1950, p. 2): "I am of the opinion that 'progressive' evolution has never been observed in the laboratory. The minor fluctuations demonstrable in the laboratory which Dobzhansky calls 'evolution' are, in my opinion, merely forward and backward changes comparable to the balancing movements which an acrobat on a tightrope has to perform to maintain his balance but which have nothing to do with his forward progress, except that if he failed in them he would fail completely."

The comments of Lt.-Col. L. Merson Davies are very welcome, as they supply an inadvertent omission in my paper. Some seven years ago I read with great appreciation and profit his review of Needham's *Biochemistry and Metamorphosis*. In my view Davies is right in emphasising that the genes are stimuli rather than the real mechanism of heredity. As in the case of most organs the genes have probably more than one function, and it seems to me that they, with the chromosomes, are a beautiful device for ensuring that no two individuals of a species are identical, a fact which has been brought home to the people by the use made by Scotland Yard of human finger prints as an infallible means of identification.

The number of possible combinations of the genes is enormous. This is what Professor Dobzhansky has to say on this matter (*The Scientific American*, January, 1950, p. 36): "Although the number of genes in a single organism is not known with precision, it is certainly in thousands, at least in the higher organisms. For *Drosophila* 5,000 to 12,000 seems a reasonable estimate, and for man the figure is, if anything, higher. To be conservative, let us assume the human species has only 1,000 genes and that each gene has only two variants. Even on this conservative basis, Mendelian segregation and recombination would be capable of producing 2^{1000} different gene combinations in human beings. The number 2^{1000} is easy to write but it is utterly beyond comprehension. Compared with it, the total number of electrons and protons estimated by the physicists to exist in the universe is negligibly small! It means that, except in the case of identical twins, no two persons now living, dead, or to live in the future, are at all likely to carry the same complement of genes. Dogs, mice and flies are as individual and unrepeatable as men are."

To my mind one of the most impressive phenomena of the living world is this prodigious variety, coupled with the stability of the type.