RECENT PROGRESS IN THE STUDY OF VARIATION HEREDITY AND EVOLUTION

R. H. LOCK

VARIATION, HEREDITY, AND EVOLUTION

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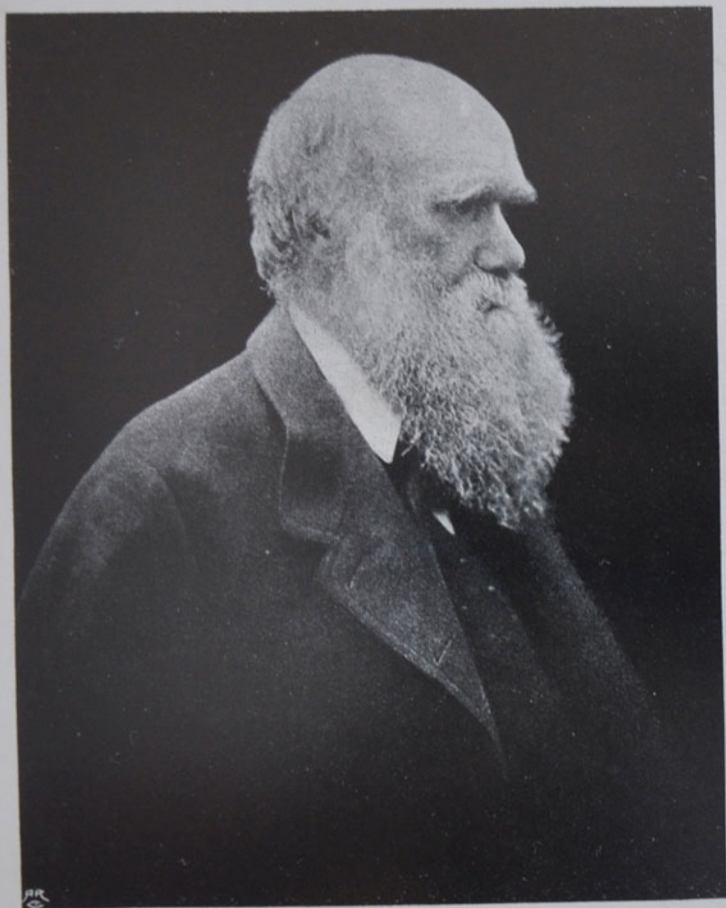

Frontispiece.]

Ch. Darwin

RECENT PROGRESS IN THE STUDY OF VARIATION, HEREDITY, AND EVOLUTION

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PREFACE

The idea of writing this little book occurred to me whilst reading Mr. W. C. D. Whetham's volume on 'The Recent Development of Physical Science.' I found the story of the modern progress of physics so interesting as to encourage the belief that a similar account of the subjects with which I was myself more particularly familiar might prove of a like interest to other people. I did not, indeed, suppose for a moment that I could vie with Mr. Whetham in the power of literary expression which renders his book so eminently readable. I rather hoped that the peculiar interest and importance of the theme might outweigh the present author's deficiencies in this respect.

For the group of subjects of which I intended to give a brief account Mr. W. Bateson has recently proposed the term 'genetics,' an expression which sufficiently indicates their scope to the initiated. Since, however, the meaning of the word 'genetics' is not yet clearly understood by everybody, it seemed better to adopt in the present instance a somewhat more descriptive title.

The rediscovery of Mendel's law some seven years ago led to a complete change in our attitude towards the problems of variation, heredity, and evolution; and the new method of study thus introduced has rendered possible a renewal of the attack upon these problems with increased vigour and with remarkable results. At the present time this activity may be said to have its centre in the school of genetic research founded at Cambridge by the independent energy of Mr. Bateson. So far-reaching are the results already arrived at by Mr. Bateson and others, both in their scientific interest and in their probable influence upon human affairs, that it seemed desirable to give an immediate account of these and of kindred lines of recent study, even though the rapid progress which is a characteristic manifestation of this department of science must render any such attempt a more or less transitory one.

Whilst I was still engaged upon my task, the first volume of Dr. Lotsy's admirable 'Vorlesungen über Descendenztheorien' made its appearance. But for the fact that most of the following pages had then already been written, I might have hesitated to pursue my project, since a book not altogether unlike the present might be produced by the comparatively simple process of making a series of judicious extracts from Dr. Lotsy's work. The latter is, however, in the German language, and on a considerable scale, so that there seemed still to be room for an introduction to

the science of genetics of the more modest dimensions which I had contemplated. I should wish, however, particularly to recommend Dr. Lotsy's lectures to any reader who wishes to go further into these matters.

I am indebted to several friends for assistance during the course of my work. Mr. R. P. Gregory kindly read through the proof of the chapter on cytology; and I wish here to record my thanks to Mr. J. Stanley Gardiner, to Mr. C. T. Regan, to Mr. W. S. Perrin, and to Mr. R. C. Punnett for information on special points. To the last-named I owe the photograph which appears as Fig. 15. I am particularly grateful to Mr. R. H. Biffen and to Mr. G. Udny Yule for access to work which has not hitherto appeared in print.

Adequately to acknowledge Mr. Bateson's influence upon these pages is a more difficult matter, and not the less so because I have deliberately refrained as far as possible from consulting him whilst the book was in course of preparation, in order that it might retain if possible some traces of individuality. It is therefore clear that he is in no way responsible for its deficiencies. But, apart from the fact that I am conscious of having quoted his ideas at more points than could possibly be acknowledged *seriatim*, I owe to Mr. Bateson both my first introduction to the science of genetics, and a continual fund of encouragement in the prosecution of studies connected with it.

I have to thank Mr. Francis Darwin for kind permission to reproduce a portrait of his father; Professor

de Vries for the present of an excellent portrait; and Mr. Francis Galton for the loan of a photograph well known from earlier reproductions. The portrait of Mendel is reproduced from the frontispiece to Mr. Bateson's 'Defence,' by the permission of the Syndics of the Cambridge University Press. Messrs. Macmillan and Co. have kindly allowed the reproduction of the diagram which occupies p. 80, and of the table and figure on pp. 82 and 83. The figures facing pp. 136 and 143 are from de Vries' 'Mutationstheorie,' published by Messrs. Veit.

The attempt has been made to render the following pages intelligible to the general reader, as well as to the more scientific public, to which they are primarily addressed. A short glossary has been added, which may be found useful by those who have no previous acquaintance with biological terms.

CAMBRIDGE,
October 23, 1906.

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RECENT PROGRESS IN THE STUDY OF VARIATION, HEREDITY, AND EVOLUTION

CHAPTER I

INTRODUCTION

THE present volume deals with variation and inheritance in plants and animals, especially in so far as those subjects bear upon the problem of the origin of species. By inheritance we mean those methods and processes by which the constitution and characteristics of an animal or plant are handed on to its offspring, this transmission of characters being, of course, associated with the fact that the offspring is developed by the processes of growth out of a small fragment detached from the parent organism. The term 'variation,' on the other hand, includes a number of different phenomena which will be described at greater length as the work proceeds; but, broadly speaking, we may say that the study of variation is concerned with the circumstance that members of the same species are not all alike, and more particularly with the fact that

differences are to be found between different members of the same family. Some of these differences arise comparatively late in life, and may be the result of circumstances or of education. It is the first duty of the student of variation to distinguish as far as may be possible between differences of this kind on the one hand, and those differences on the other which depend upon the fact that the different detached fragments, as we have termed them, of the parent organism—its germ-cells, in fact—show greater or smaller differences among themselves.

The facts of variation have this very special importance, that the whole theory of organic evolution is based upon them. The fact that members of the same species are not all alike, depending upon the further fact that offspring may differ from their parents, makes it possible in the course of generations for progressive changes to take place, so that from the offspring of different members of the same species different new species may arise. But for this fact of variation it would have been quite impossible for Darwin to have overthrown the former crude belief in a special creation of each separate species, since there would have been no material for his great factor—natural selection—to work upon. It is with variation, then, and with the manner in which characters appear in the successive generations of living things, that we are here concerned.

Ever since the publication of Darwin's 'Origin of Species' in 1859, these subjects, and especially the theoretical aspects of them, have been received even

by the general public with all the signs of a genuine enthusiasm; and none, moreover, can be more fascinating to the professional naturalist. But since the time of Darwin the more popular accounts have dealt almost exclusively with theoretical considerations and with matters of opinion. Highly abstruse controversies have raged freely between Neo-Lamarckians and Neo-Darwinians, and these have found a place in the pages of works ostensibly intended for the instruction of all and sundry; whilst only a bare residuum of actual matters of fact has seen the light of popular publication. If the truth must be told, the experimental method was given up for a long time by the majority of specialists themselves in favour of the controversial, and, indeed, this tendency has by no means yet died out from among the habits of some professed evolutionists. On the other hand, during the last fifteen to twenty years, a few scattered workers have diligently applied themselves to a study of the facts of variation and inheritance, with results which already more than justify the anticipation in which their work was begun—namely, that by such methods alone can any real progress in our knowledge of the processes of evolution be brought about.

The science of organic evolution is by no means the simple and isolated study it might be supposed to be from a perusal of some of the more popular accounts. Its footing rests immediately upon the widest foundations which zoology, botany, and physiology can afford; and these in their turn are ultimately based upon the results of chemical and physical science. But some

of the most fundamental parts of physical science, as I think we may fairly call the branches of electricity and molecular physics, seem at present to be undergoing modifications which bid fair to bring about a complete revolution in current ideas upon these subjects. It is highly probable that these results will ultimately lead to a considerable modification in prevailing notions about living things; but the new developments have yet to reach biology through the channels of organic chemistry, physiology, cytology, and the like, and at present we do not know what the result of this influx is likely to be. These considerations need not, however, detain us, for the new knowledge of variation and inheritance, of which it is proposed to give some account, is largely concerned with the grosser characters of organisms, so that ultraminute structures may be left alone for the present until the stream of physical knowledge stirs them into greater prominence. So much is this the case with the study of variation and inheritance by experimental methods at the present day, that this science is treated by some with a fine contempt, because its tools are those of the breeder and gardener, and because the assistance of the compound microscope may often be laid aside for days together. Yet this applies only to one aspect of the subject, and the microscopic study of the embryonic rudiments of organisms, going hand in hand with the experimental observation of adult structures, is rapidly leading to a clearer understanding of the processes of heredity.

The problem which those who are engaged in this

kind of work have set themselves for solution is that of the nature and method of origin of the existing differences between certain groups of organic beings —namely, species. Basing their studies on the doctrine that the present species have arisen through the modification of pre-existing species, they endeavour to observe how modifications of existing species do actually arise in Nature, as well as under domestication; and they watch the hereditary transmission of the modified forms when like is bred with like, and when different types are crossed together. For the theory of uniformity, now universally accepted, teaches us that the organisms with which we are now familiar owe their present characteristics to the accumulation of a series of changes similar to those which are still in progress. It has, therefore, appeared likely to a few that a further understanding of the processes of evolution might best be obtained by a closer study, firstly, of variation, or the ways in which offspring differ from their parents; and, secondly, of inheritance, or the ways in which the resemblances between parents and their offspring are perpetuated from one generation to another.

It may be well to point out at once that the further study of the method of origin of new species, admitting, as it does, that this process is not yet by any means fully understood, does not for this reason imply that the theory of organic evolution itself is open to criticism. The evidence that new species arise by the modification of pre-existing species is quite independent of the evidence that this process invariably occurs

by the action of natural selection upon minute differences, in the manner which Darwin described, and which has been claimed by others as the sole means by which the origin of new forms takes place. The evidences of evolution are much more numerous and more weighty than the evidences of the survival of the fittest. The theory of evolution, as opposed to the creation hypothesis, is supported by innumerable facts of classification, of morphology, and of embryology, by the geographical distribution of animals and plants, and by their succession in the geological strata, as well as by direct observation of the actual occurrence of changes in the case of domestic productions as well as under Nature, and many of these facts have no direct bearing upon the theory of natural selection.

Before discussing the problem of the origin of species, it is necessary to arrive at some idea as to what the term 'species' means. And this is not altogether an easy matter, since a precise definition has not been, and cannot be, agreed upon. The idea of species is, indeed, of great antiquity and very gradual growth. Primitive men doubtless recognised certain plants or animals as being like one another, and different from others, and they gradually came to distinguish such forms by giving a different name to each. The names first used must have applied as a rule to genera rather than to species. Thus, such common names of plants as rose, bramble, vetch, nettle, dock, crowfoot, are names of genera—groups of greater extent than species, and often more easily defined than the latter. Later

on civilized men paid closer attention to the different kinds of plants, and the old herbalists discovered and described a number of different sorts of roses, of buttercups, and of other plants, and distinguished each by a descriptive sentence.

As more and more species came to be described, this method of designation became very cumbersome, until Linnæus, about the middle of the eighteenth century, adopted the idea of a binomial nomenclature (originally suggested by Bachmann), in which every species of each known genus received a separate name of its own to distinguish it, so that the different kinds of buttercups were now known as *Ranunculus acris*, *R. bulbosus*, *R. sceleratus*, and so on.

Linnæus himself appears to have had a very definite idea of what constituted a species, and in accordance with the view then current, he defined a species as being a group of organisms which owed its origin to a separate act of creation. From the nature of the case this definition could be of little use in practice. Practically, then, species were defined as groups of animals or plants, the members of which resembled one another in definite morphological characteristics—that is to say, in constant features of form and structure. This definition has survived the downfall of the dogma of the constancy of species, and at the present day species as defined by Linnæus are found to be groups of much merit both for naturalness and for convenience—at any rate in the case of plants. The fact that intermediate forms and minor groups do sometimes and to some extent bridge over the gap which separates a

pair of species thus defined seems to have caused disquiet in the mind of Linnæus himself, and he recommended his disciples to have no dealings with these inferior varieties, as being beneath the dignity of a botanist to notice. Of late years these minor species have excited much attention, and it is to a study of this kind of species in particular that the mutation theory of de Vries owes its origin, as will be told in a later chapter.

Such minor groups, occurring within the limits of a single Linnæan species, and subdividing it into smaller collections of individuals, were made the object of special study in the case of plants by the French botanist, Jordan; and for this reason they are sometimes referred to as Jordan's species. Jordan, for example—though the example is indeed an extreme one—described more than two hundred different types, all of which would formerly have been included in the single Linnæan species, *Draba verna*. To take a more familiar instance. We find in the 'British Flora' of Bentham and Hooker the primrose, the cowslip, and the true oxlip, all described as varieties of one and the same species; yet these three kinds of plants are now almost universally recognised to be as good species as any in nature.* In a similar way, on closer investigation, it has been found necessary to split up a considerable number of Linnæan species, and to subdivide each into several species of smaller range.

* A contrary opinion is, however, expressed in the *Journal of Botany* for July, 1906.

It has already been pointed out that Linnæus himself distinctly deprecated this process of splitting. 'Varietates levissimas non curat botanicus,' said Linnæus. Jordan, however, applied the method of experiment to many of the species of his own definition, and having transplanted them from a variety of localities to the uniform soil of a garden, found that they preserved their distinctive characters and came perfectly true to seed.

It appears then that Jordan's species are just such true and constant groups as those of Linnæus. They are separated from one another by definite features of form and structure, only these differences are not so wide as those which separate Linnæan species. The latter are, indeed, to be looked upon as more or less artificial groups or aggregates of these physiological species, as Jordan's species have also been called. The problem of the origin of the smaller groups is clearly to be placed before that of the origin of the larger species.

It is true that in the case of certain groups of animals and plants there would appear to be no possibility of drawing hard and fast lines between the species, which thus seem to shade gradually one into the other. There is, however, a great difference between the admission that certain nearly related species are difficult or impossible to separate definitely, and the statement that there is no true distinction between them, and the latter statement is one which few are bold enough to make. The case stands thus. We know that great numbers of large groups (classes

and families) of animals and plants exist, in which the most nearly related species are quite definitely distinct from one another. In other classes systematists have so far found great difficulty in framing definitions of specific groups. We shall see later on, though at first sight it may appear almost paradoxical, that it is quite possible for groups to be perfectly distinct, although individual members of them may have deviated so far, each from its proper type, as to render impossible the task of deciding from their appearance which group any of these individuals belong to.

Let us next consider a particular example of a class of animals in which the discrimination of species is difficult or impossible. This is said to be the fact with the majority of sessile animals—such animals as resemble plants in their stationary habit, and in no case are the problems of species separation more difficult than in the class of the stony corals. Now, attempts to determine the species of corals have so far been made almost entirely from a study of what may be called vegetative characters—usually from details of the shape and structure of the stony skeleton of the animals. How far these features may be affected by external circumstances has not been determined, but it must be noted that the so-called skeleton is entirely external to the living organism. Now we know that in the case of many of the higher plants vegetative characters are extremely liable to become modified owing to the action of the environment. Differences of moisture, light, soil, climate, and alti-

tude, are all capable of changing the general appearance of a plant so as to render it scarcely recognisable. Fortunately, in the case of the higher plants, the floral organs, which are the ones chiefly made use of for purposes of specific discrimination, are very little liable to modification by external conditions; but in the corals a similarly stable set of organs does not appear to have been discovered. It seems, therefore, hardly fair to regard the example of the corals as affording an established exception to what we must look upon as the general rule—namely, that species are on the whole definite and discontinuous groups.

As a rule, then, the species riddle presents itself definitely as the problem of the existence of a series of discontinuous groups of creatures, sharply marked off the one from the other, and often, too, existing among surroundings which afford no corresponding discontinuity, though each is well enough fitted for the life which it has to lead.

The problem which we have to face has been enunciated by Bateson in the form of the two following propositions:

'1. The forms of living things are various, and on the whole are discontinuous or specific.

'2. The specific forms on the whole fit the places they have to live in.

'How,' he continues, 'have these discontinuous forms been brought into existence, and how is it they are thus adapted? This is the question the naturalist is to answer. To answer it completely he must find

(1) the modes and (2) the causes by which these things have come to pass.'

The differences between existing species are open to study in more than one way. By way of limiting the discussion for the present, we shall consider the case of plant species only; but the methods of study which are applicable to animal species are of quite similar kinds.

Four methods at least are available. Firstly, that of comparison; secondly, the method of statistical examination; thirdly, the method of cultural experiment; and lastly, that of cross-breeding.

The method of comparison is the one to which the ordinary worker in descriptive botany is almost of necessity confined. In this way plants which closely resemble one another are grouped together as belonging to the same species, and separated from others, the appearance of which is different. By appearance is not meant simply the general habit of the plants; all morphological features whatever may be used for purposes of comparison, and the most minute are often of the greatest importance. But the systematist who works only in this way knows nothing of the real relationships between the plants with which he is dealing.

When a sufficient number of specimens is available, the methods of statistics can be applied. These involve the making of a series of accurate measurements or countings of the parts upon which depend the supposed differences or resemblances of the plants

under consideration. The resulting numbers are then ranged in order so that a precise view of the numerical characters of a large number of specimens can be readily obtained. By the use of such methods valuable information is often to be arrived at. But the same limitation affects them as in the preceding case.

So that the only way in which we can come to a definite decision as to whether a given putative species does or does not represent a definite and constant type is by resorting to our third method, that of sowing its seeds and actually rearing its progeny. And this is not so simple a matter as might appear at first sight, for a great many precautions have to be taken. Thus we must separately sow the seeds of many different individuals of the species which we are examining, so as not to base our conclusions upon a few experiments only. But in many cases, even when this has been done, we should only know one of the parents of our seedlings—that is to say, in cases where the pollen for fertilization may possibly have been conveyed by natural agencies from a different plant. In such a case we must either ensure self-pollination by isolating our plants, or we must artificially provide pollen from a separate known parent. If under these circumstances a particular group of plants preserves the characteristic differences which distinguish it from another group which has also been grown for a number of generations under the same conditions, we have at last reasonable grounds upon which to base the opinion that we are dealing with two distinct physiological species, even though the

visible differences between them may seem very small to an untrained eye.

Lastly, evidence of a confirmatory nature may be obtained by observing the results of cross-fertilization between a pair of closely allied species. Much, too, may be made out from the failure of such experiments, since the refusal of two plants to breed together is generally regarded as clear evidence of their specific distinctness. But for this reason the method of cross-breeding is more particularly adapted for the examination of forms somewhat nearly related to one another—for example, different members of the same species.

As the result of the methods presently to be described, the fact has been established that two entirely distinct sorts of divergencies may appear among members of a single family. Variations, that is to say, may be of at least two different kinds. In the first place we have those slight differences which invariably distinguish all the members of every family—individual variations which affect every part and every character. Such differences are known as fluctuating, normal, or continuous variations. As an example we may cite the variations in size or stature shown by the various members of any purely-bred race. When a large number of individuals are compared in respect of a character of this kind, they are found to fall into a continuous series ranging from a certain extreme of shortness on the one hand to an extreme of tallness on the other. Individuals of a medium height, however, are usually more numerous than either

of the extreme forms. Some further account of the study of continuous variations will be given in Chapter IV.

A second kind of variation is variously known as abnormal, definite, and discontinuous variation, and includes what are known as sports and mutations. Such variations, as is indicated by the terms applied to them, involve definite differences usually of considerable amplitude. A good example of a discontinuous variation would be afforded by the appearance of a child having six fingers in a family in which this abnormality was not previously known to occur. We shall pursue the discussion of discontinuous variation and of the methods of perpetuation of the types which thus arise in Chapters V. and VII.

A short account of the historical development of the theory of organic evolution is given in Chapter II. It is of particular interest to notice that the modern view of the mutationist is foreshadowed with remarkable precision in the passage from Aristotle's 'Physics' which is quoted in Chapter V. Passing to more recent times, three distinct accounts of the method of origin of specific differences have been proposed almost within the last century, and each of these theories still finds a number of supporters.

1. The view of Lamarck, published first in 1801, and in an enlarged form in 1809, was briefly as follows: Noticing that the organs of men and other animals are increased and strengthened by use, and particularly by conscious use, Lamarck assumed that this effect

could be passed on by inheritance from parent to offspring, and so accumulated from generation to generation. In the case of animals Lamarck conceived the production of a new specific form to take place in the following way: Owing to some change of external conditions, the desire to perform some new kind of action was set up in the parent species, and by the hereditary effect of the striving occasioned by this desire a modification of the organs affected into forms better fitted to carry out the new function was gradually achieved.

Thus Lamarck supposed that snakes were evolved from a pre-existing type of animal which was of a much less attenuated shape, and which possessed two pairs of limbs like any other vertebrates. And he supposed this evolution to have taken place owing to the constant striving of these animals to pass through narrow crevices; the effect of such striving being inherited, and so accumulated from one generation to another.

In the case of plants, in which conscious effort is precluded, a similar result was supposed to have been attained by an hereditary accumulation of the effects of the environment.

2. The explanation of Darwin, or at least the Neo-Darwinian form of it, as interpreted by Wallace, Weismann, and others, and as opposed to and excluding the view of Lamarck, was as follows: Two separate factors are primarily concerned: (1) the fact of fluctuating variation—the fact that no two members of the same family ever resemble one another exactly; and (2) the occurrence of a struggle for existence between organisms owing to the geometric rate of

increase of living things. From these two facts it follows that when a change of environment takes place, certain members of an existing species will be somewhat better adapted than others to withstand the new conditions, and the former will tend to survive to the exclusion of the latter. It is assumed that during a long series of generations this process will cause a steady change in the character of the species in the direction of better adaptation to the new conditions.

Thus we might suppose that among the ancestors of the snakes those which happened to possess the longest and thinnest bodies and the smallest limbs had the advantage over their fellows that they were able to crawl through narrower holes, and that for this reason a greater number of them survived to produce offspring. Here we have a better basis for reasoning than the supporters of Lamarck's doctrine, because we actually know that longer parents, in whom this quality was apparently not the result of taking thought, do tend to produce on the average longer offspring.

3. The view of the mutationists, already foreshadowed by Aristotle, and in recent years especially associated with the names of Bateson and de Vries, expresses the conclusion that the evolution of new species has taken place principally by the help of variations of the discontinuous kind. By this process there can arise at a single step new forms which have already the complete and definite character usually associated with a species specially adapted to particular conditions. Of these new forms, those which happen to be fitted for their surroundings as well as or better

than their predecessors will survive, whilst those which are worse will be destroyed by the action of natural selection.

Thus it would be an appropriate use of this conception to seek in a mutation the explanation of the final loss of the much reduced limbs presumably exhibited by comparatively recent ancestors of the family of snakes. This final loss is especially difficult to understand on the Darwinian theory. Moreover, changes of a closely similar nature are not hypothetical, but have actually been observed to take place. At the same time it must not be supposed that mutations are confined to the loss of pre-existing organs; indeed, the origin of a totally new organ is quite inexplicable on either of the two preceding theories. The very first inception of such an organ must, it would seem, of necessity be sudden.

After giving some account of the earlier theories of evolution, we shall next proceed to treat of those subjects with which we are more properly concerned—that is to say, the recent experimental observations on variation and natural inheritance, together with their bearing on the theories of evolution. And in the first place we shall describe some recent studies which are not strictly experimental, but which nevertheless deal to some extent with actual facts—namely, the statistical study of variations, particularly of continuous variations. This subject has been dignified by a special name, and is now described as the science of biometry.

Of even greater interest, however, are the more strictly experimental researches which have been published within the last five or six years. In the first place, we have the observations of de Vries, who has introduced a new method of study—that of cultivating great numbers of seedling plants with the object of discovering definite new forms or mutations among their number. Lastly, and in its results much the most important of all, we have the method of Mendel, published half a century ago, but only recently brought into prominence owing to its rediscovery and confirmation by three independent workers—Correns, Tschermak, and de Vries. This method consists in the cross breeding of strains of plants or animals which differ in definite characters, and in the statistical examination of the proportions in which these characters appear among the offspring obtained from the crosses.

Further experiments on the lines which Mendel indicated bid fair to revolutionize within a few years the arts of the breeders of plants and animals. This is due to the fact that such experiments are leading to the introduction into these pursuits of a degree of scientific exactness which was previously altogether unforeseen. The change in our ideas regarding the method of hereditary transmission of characters, which has resulted from these experiments, has been aptly compared with the change brought about in men's understanding of the science of chemistry by Dalton's conception of the atom. For the rest the new experiments tend on the whole to confirm the

experience of practical breeders; only the elucidation of one simple rule of inheritance has brought into order a host of phenomena, which were previously quite incapable of a coherent explanation.

The experimental results with which it is the province of this book to deal are, then, firstly those of biometry, or the statistical study of variations, and particularly of continuous variations; secondly, the results of direct observations bearing upon the origin of species by the discontinuous method; and thirdly, the results of experimental observations on heredity by the methods of scientific breeding. By these methods results of the utmost moment to mankind have been, and are being, arrived at, quite apart from their interest as bearing upon the problems of evolution. From a biologist's point of view, however, the latter is, of course, paramount. And so it has been thought fitting to begin with a brief discussion of the problems of evolution, and of the various solutions of them which have been from time to time suggested.

In a later chapter some of the more prominent recent results of the kindred science of cytology—the microscopic study of the minute constituent parts of organisms—will be briefly described, on account of the very close connection which recent progress in this subject bears to the experimental study of the inheritance of the grosser characters.

CHAPTER II

EVOLUTION

EVOLUTION may be defined as progress involving differentiation, an ever-growing complication of things which accompanies almost all the operations of Nature. The idea of a differentiation of this kind may be enforced by a homely and quite imaginary illustration of such a process. Imagine the proper ingredients of a plum cake to be very finely minced and intimately mixed together, so as to form a more or less homogeneous material. Then, if by any means the separate particles of currants, raisins, peel, and so forth, could be made to segregate out in such a way as to give rise to the ordinary structure of this pleasant confection, we should have arrived at the structure of a plum cake by a process of evolution involving considerable differentiation.

The progressive increase in complexity which is characteristic of so many natural processes is in great part occasioned by the fact that a single 'cause' is followed as a general rule by more than one 'effect.' This apparently simple circumstance was pointed out

by Herbert Spencer,* who has perhaps done more than any other to establish and emphasize the general applicability of the evolution idea. For the law of origin by evolution is by no means exclusively confined to the method of coming into existence of the species of animals and plants. On the contrary, it was equally well applied by Spencer himself to describe the manner in which are supposed to have arisen the stars and other heavenly bodies, the geological strata and geographical configuration of the earth, and the various gradations of human society.

The discovery that certain chemical elements exist which are themselves not immutable has been made since Spencer's time. Quite recently 'the phenomena of radio-activity have forced us to believe that radium is passing continuously into helium,'† and something more than a suspicion has been aroused that radium is itself derived from uranium. Thus the dreams of the alchemists are shown to have been not wholly without foundation, for the probability is strong

* Spencer gives the following illustration: Regarding the striking together of two bodies as a 'cause,' he points to the following possible 'effects': A sound; other vibrations or movements in the surrounding air; a disarrangement of the particles of the two bodies in the neighbourhood of the point of collision; the production of heat, and possibly of a spark—*i.e.*, of light.

Two words in this sentence are placed between inverted commas, to indicate that they are used in a strictly popular sense. The use of the words 'cause' and 'effect,' though seldom strictly scientific, is often convenient, and if used with caution, there is no reason why they should lead to misunderstanding. See Whetham, 'The Recent Development of Physical Science,' chapter i.

† Whetham.

that under suitable conditions other matter may be observed to behave in the same way as radium. More than this, Professor J. J. Thomson has been able to describe the atoms of the elements as different aggregations of a single kind of corpuscles, and to show that a progressive change in the number of corpuscles making up the atom is accompanied by a progressive alteration in the properties of the atom itself, so that it has now become possible to establish a theory of the evolution of the chemical elements themselves.

Passing from the almost immeasurably small to the almost immeasurably great, we may briefly consider the probable mode of origin of the solar system from an extremely diffuse cloud of material substance, according to the famous nebular hypothesis of Laplace. By a long-continued process of contraction under the influence of gravity the nebular substance came to be of varying density, and acquired a rotary movement in one plane. As the mass continued to contract owing to the mutual attraction of its particles, the velocity of rotation increased, until at last the increasingly rapid motion of the outermost ring of the now lens-shaped nebula gave rise to a centrifugal force great enough to counteract the tendency to contraction, and in the further condensation of the mass this ring was left behind. The ring next broke down at one point, and contracting on itself gave rise to a single spheroidal body which acquired a movement of rotation in the same direction as that of the parent nebula. This body was the outermost planet Neptune, and the rest of the planets were produced in a similar manner,

until at last a central mass was left, and this became the sun. Satellites were thrown off from several of the planets just in the same way as the planets themselves arose from the original nebula, and Saturn's rings are pointed to as showing this process even now in course of operation.

Such a description as this may appear fanciful at first sight, but it was worked out quantitatively as well as qualitatively by its author, and was shown to explain in detail a multitude of phenomena. Spencer points out that when we have, worked out by one of the first of mathematicians, a definite theory of planetary evolution based on established mechanical laws, and one which accounts in a satisfactory way for all the known phenomena, the conclusion that the solar system really did arise by a process of evolution is, to say the least, difficult to avoid.

The establishment and propagation of the idea that the present condition of the earth's surface arose through a course of gradual evolution, by the agency of such processes only as are known to be in operation at the present day, is the great contribution of Sir Charles Lyell to the science of geology. We may briefly trace the evolution of the idea itself, beginning with the speculations of Werner, who, from observations of the geological formations of a limited tract of country, came to the conclusion that the successive strata were precipitated one by one from an universal ocean. Here we see the first germ of the idea of evolution embodied in the notion that the stratified

rocks came into existence gradually and through the operation of a supposed natural cause.

A great advance upon Werner's theory was made by Hutton, who, observing the formation of strata at the present day from the sediment washed down by rivers, concluded that the ancient strata were deposited in the same manner. Since, by the long continuation of this process the continents must gradually become reduced to the level of the sea, Hutton supposed that at long intervals of time the action of subterranean heat came into play, and fresh continents were upheaved, a process accompanied by the outpouring of the igneous rocks, the true origin of which he had duly recognised. In this theory a hypothetical cause still survives, since we have no actual experience of vast upheavals of the kind which Hutton supposed to have taken place. Lyell showed that such slight changes of level as are known to be in progress at the present day, especially in association with the phenomena of earthquakes, might, if continued over a long series of ages, give rise to the necessary amounts of elevation. Lyell also pointed out a number of subsidiary causes of disintegration and deposition of strata of the kind which can still be seen in operation at different parts of the earth's surface. At the present time it is sometimes thought that Lyell went a little too far in his championship of the cause of uniformity. Lyell supposed that the agencies which may now be everywhere observed in operation, such as rain and rivers, the sea, volcanoes and earthquakes, were sufficient to account

for all the phenomena which the crust of the earth exhibits. It is now more generally supposed that in very early times forces similar in kind to those in action at the present day may have exhibited considerably greater violence.

To produce the present condition of the surface of the earth by the action, gradually accumulated, of such processes of denudation and upheaval as are now going on around us, vast periods of time are clearly necessary. The early evolutionists, having once got rid of the idea that the date given by Bishop Usher as that of the creation of the world is a necessary and integral part of religion, immediately allowed their imaginations to run riot with regard to the amount of time at their disposal. Since this question of the extent of geological time has an important bearing on the problem of organic as well as upon that of inorganic evolution, it will be well to pay some attention to more recent views upon the subject.

Some years ago the generous ideas of biologists as well as of geologists were to a great extent shattered by the calculations of Lord Kelvin. These were based upon three separate sets of data, which we may enumerate without entering into a lengthy explanation of the calculations involved. The evidence made use of consisted of (1) the rate of the earth's rotation, as affected by tidal retardation; (2) the rate of secular cooling of the earth, as deduced from the rate at which the temperature of the earth's crust rises on passing inward from the surface; and (3) the rate of cooling of the sun by radiation. The three calculations were

found to show a very fair measure of agreement, and they led to the conclusion that considerably less than a hundred million years has elapsed since the first formation of seas upon this planet, an event which must have preceded the possibility of aqueous geological action and the existence of living organisms

Allowing for the circumstance that geological processes may have gone forward with considerably greater rapidity during the earlier periods of the earth's history than is the case at the present day, the time thus allowed by the physicist is generally regarded by geologists as too little. Reckoning from the known rate of denudation, which is, of course, the same as the rate at which the same material is deposited beneath the sea, Geikie, who admitted, however, that such data are only of a very rough description, concluded that the space of a hundred million years would afford sufficient time for the laying down of the known aqueous strata. But there can be little doubt that the lower metamorphosed rocks represent a much longer period of time than the primary, secondary, and tertiary epochs added together; consequently, the respective estimates of Lord Kelvin and the geologists appear to be contradictory. The recent discovery of the enormous quantities of energy stored up in radio-active substances introduces a serious modification into the mathematical argument from astronomical data, and Sir George Darwin 'sees no reason for doubting the possibility of augmenting the estimates of solar heat, as derived from the theory of gravitation, by some such factor

as ten or twenty,' on the supposition that a considerable proportion of the sun's substance was made up of radio-active material.

The above remarks may serve to illustrate the importance of the theory of evolution as applied to the two sciences of astronomy and geology. We pass next to a brief historical consideration of the development of the evolution theory as a method of describing the origin of the species of animals and plants.

The views of the ancient Greeks cannot be said to have much more than a purely speculative interest. Some rudiments of the idea of evolution have been attributed to Empedocles as well as to several other early writers, and in the writings of Aristotle, for whom the too great faith of his successors for many ages has been followed by a somewhat unmerited degree of contempt in modern times, we find that the evolution idea had reached quite a respectable degree of development.

In the Middle Ages the adoption of the Jewish cosmogony by the Christian Churches effectually annihilated all useful thought upon the subject of species, since the hypothesis of separate creation affords no scope for further speculation or experiment, and it is not until the end of the seventeenth century that we find thoughtful men beginning to struggle against the ecclesiastical bondage. Thus Erasmus Darwin derived the idea of generation rather than creation of the world from David Hume, and himself waxes enthusiastic over the thought :

'That is, it (the world) might have been gradually

produced from very small beginnings, increasing by the activity of its inherent principles, rather than by a sudden evolution of the whole by the almighty fire. What a magnificent idea of the infinite power of THE GREAT ARCHITECT! The Cause of causes. Parent of parents. *Ens entium.*'

De Maillet, writing in 1735, showed a definite idea of the production of existing species by the modification of their predecessors. At the beginning of the nineteenth century similar speculations were published by Goethe and by Treviranus, and the latter was the first to apply the term 'biology' to the science of the phenomena of life. Lamarck about the same time provided a definite theory as to the method by which the modification of species takes place.

Before discussing Lamarck's hypothesis and the alternative theories more recently proposed, it will be well to pass in review the evidence upon which is based our belief that the species of animals and plants have arisen through the modification of pre-existing species, and to show that the greater part of this evidence is quite independent of any views which we may adopt as to the actual method by which a particular species came into existence. And in the first place we may point out the entire absence of any evidence, direct or indirect, in favour of the alternative supposition of a special creation of each separate species.

The evidence for evolution falls naturally into a number of fairly well defined sections:*

* A modification of the list given by Huxley, 'Collected Essays,' vol. ii., p. 205.

1. The Gradation of Organisms.—Both in the animal and vegetable kingdoms we may trace, in spite of certain gaps, a long series of gradations in complexity of structure, so that between the simplest and the most complicated of living things a great number of intermediate stages are to be found. When we pass to the lower end of the scale in either case, we come upon a group of creatures of comparatively simple organization. Among them we find members with regard to which we cannot definitely say that they are either animals or plants. Moreover, these unicellular organisms resemble in many ways the egg-cell from which every individual among the higher animals and plants originates.

2. Embryology.—All the members of a particular group of animals or plants as a rule resemble one another more closely in the early stages of their individual development than they do in the adult condition, and in the earliest stages of all they are often indistinguishable. These facts are explained if we suppose that such individuals have a common origin, that they are descended from a common ancestor, and that traces of their pedigree are still to be observed in the developmental stages through which each one passes. We do not find a complete parallelism between the development of the individual and the history of the race, nor should we expect to do so, since embryonic as well as adult stages may be modified in the course of evolution; what we should expect is a more or less vague historical sketch, and this is what is usually found remaining.

3. MORPHOLOGY.—On comparing together the different members of one of the great groups or classes of animals or plants, we find the same fundamental plan of organization running through all of them. Series of corresponding organs are often to be made out which are built upon the same general scheme, although their functions may be quite dissimilar; so that, for instance, in the hand of a man, the paw of a dog, the wing of a bat, and the paddle of a whale, almost identically the same series of bones can be traced. An obvious explanation is to be found in the supposition that these parts have arisen by the divergent modification of parts which were originally identical.

4. GEOGRAPHICAL DISTRIBUTION. — Observation shows that groups of closely allied creatures are often found living in neighbouring districts, and that when such a barrier as an ocean or a range of lofty mountains is passed an entirely new fauna and flora are usually to be met with. These facts may be explained by the hypothesis that allied groups of species originated by a process of descent in the same countries which they now inhabit, and they can be explained by no other known hypothesis.

5. THE GEOLOGICAL SUCCESSION OF ORGANISMS.—The general facts regarding the distribution of allied species of animals and plants in time point in precisely the same direction as those relating to their distribution in space. In a few cases, notably in that of the extinct horse of North America, a long chain of possibly ancestral types has been found leading back

to a remote and very different progenitor. This supposed ancestor of the horse was a creature little larger than a moderate-sized dog. It had four separate toes to each fore-limb, and three to each hind-limb, and its teeth were much simpler and less specialized than those of existing horses. The general distribution of organisms throughout the geological strata agrees, moreover, in a remarkable way with what is to be expected on the evolution theory.

6. Changes under Domestication.—Among domesticated animals and plants we know of numerous cases in which the actual origin of new forms has been observed. These have often differed from their predecessors by amounts quite comparable with the differences by which natural species or even genera are separated. A notable example of this process is afforded by the numerous breeds of pigeons known to have arisen under domestication from a single wild species. We have no reason whatever for supposing that domesticated species are more mutable than wild species, and there is consequently every reason to believe that changes of a similar character take place in Nature. The conditions of domestication, of course, afford much better opportunities of observing such phenomena.

7. The Observed Facts of Mutation.—Nevertheless, individual specimens of particular wild species are frequently found showing modifications which, if they occurred constantly in an isolated group, would afford a basis for the description of new species. In a few cases the actual occurrence of similar changes has been observed in wild species of plants.

We see, therefore, that the evidence in favour of the existing species of animals and plants, having arisen by a process of evolution, is of a most ample and convincing kind. The theory of organic evolution is, however, incomplete until we have arrived at a true account of the method or methods by which new species arise from old ones. The earliest definite explanation, as already stated, was that given by Lamarck, and we may next proceed to consider the Lamarckian theory of the origin of species.

Earlier writers had already supposed that species became modified through the action of the external conditions to which they were exposed. Lamarck laid special stress upon the observed facts that the organs of individuals become increased and developed through use, and that disuse is followed by a dwindling and loss of the power of action. By the inherited effects of use and disuse, and of modifications caused by external conditions, Lamarck supposed all evolution of species to have come about.

Reference has already been made to Lamarck's description of the method of origin of the characteristic form of snakes, owing to the endeavours of the snakes' ancestors to creep through narrow passages. Lamarck was quite consistent inasmuch as he explained the different types which have arisen among domesticated species by the same theory as he applied to the origin of species in a state of nature. Thus he supposed the differences between race-horses and heavy cart-horses to be the direct result of the different kinds of enforced exercise to which the ancestors of these races were

respectively subjected. Similarly, all the different breeds of dogs were supposed to have arisen owing to the different habits which the various successors of the first domesticated dogs acquired, small changes being accumulated by inheritance in each successive generation.

Turning now to species in a state of nature, the case of the giraffe is one of those most often quoted. Lamarck supposed a comparatively short-necked ancestor of the giraffes to have taken up the habit of browsing upon the leaves of trees, owing to the difficulty of obtaining other food in an arid region. In order to obtain their new food the animals were obliged to be continually stretching upward, and the effort to elongate their necks was attended with some small measure of success in each individual. This increase, being accumulated by inheritance in every succeeding generation, ultimately led to the great stature exhibited by the giraffes of the present day.

The stilt-like legs of many wading birds were ascribed by Lamarck to the result of the continued attempts of ancestors which had shorter extremities to obtain their food in shallow water without wetting their feathers. The long-continued endeavours of these birds to stretch and elongate their legs had the same effect as the similar efforts made by the ancestors of the giraffes. It has been suggested, however, by a critic of Lamarck's position that such birds would be likely to eschew fish dinners long before any notable increase in the length of their legs was arrived at.

If some of the above cases appear a little ludicrous,

there are other instances in which the Lamarckian explanation seems adequate, and where an alternative hypothesis is lacking. Such a case is afforded by the family of the flat fishes, including such well-known species as the sole and plaice. In the adult condition these fishes lie flat on one side; and during their development from the young condition, that eye which, if it remained in its original position, would look directly downwards travels round the head until it comes to lie quite upon the upper surface. As Darwin pointed out, agreeing in this with Mivart, a sudden spontaneous transformation in the position of the eye is hardly conceivable, and it is equally impossible to explain the origin of this remarkable feature by the action of natural selection, because a slight change in the position of the eye could be of no advantage so long as this organ remained upon the under surface. The very young fish, whilst still symmetrical, are known sometimes to fall upon one side, and when in this position to twist the lower eye forcibly upwards. Darwin himself therefore supposed that the origin of the adult structure is to be attributed to the inherited effect of efforts of this kind.

The interest of the last case lies in the fact that it relates to a structure, the origin of which does not appear explicable on the theory of natural selection; its bearing will therefore be better understood when we come to discuss that theory in the next chapter.

The inherited effects of voluntary striving can clearly have no application to the case of plants. Lamarck therefore supposed that evolution in the vegetable

kingdom had taken place entirely through the action of external agencies upon plants. The soil, for example, in which a plant grows has a direct influence upon its form. Altitude, moisture, heat, and light are other important factors, and the effect of their influence upon the plant was supposed by Lamarck to be inherited. The shape of irregular flowers was regarded as having been directly caused by the strains and pressures occasioned by bees and other insects whilst making their visits in search of honey or pollen.

Lamarck's theory turns entirely upon the question whether acquired characters are inherited, and if so, to what extent, since, if such inheritance is shown to be extremely slight, the cause, though a true one, may be insufficient to explain the effects attributed to it. Now, theories of heredity apart, and leaving aside the results of minute observations which had not been made in Lamarck's time, the natural supposition undoubtedly is that acquired characters are inherited just as much as any others. Given the observed fact that offspring resemble their parents more closely than they do other members of the same species, it is natural to believe that the child will take after the forms exhibited by its parents at the time of its conception rather than after those shown by them at any previous period of their lives. This seems to be the natural view in the absence of any other evidence for or against, and so accurate a thinker as Herbert Spencer, writing before the publication of the 'Origin of Species,' regarded the term inheritance as necessarily implying inheritance of this particular kind.

For this reason it has sometimes been thought that Darwin scarcely accorded to Lamarck the appreciation which he deserved; and yet Darwin himself fell back upon the Lamarckian explanation on the few occasions when natural selection seemed to have failed him.

When, however, we come to know more of the actual facts of sexual generation, we find that it is very difficult, if not impossible, to imagine any kind of mechanism by which this supposed transmission of acquired modifications can take place. We shall defer the further discussion of this subject, as well as the question of the existence of direct and other evidence of use inheritance, until the latter half of the next chapter, where we shall refer briefly to the controversy upon these subjects which followed the establishment of the principle of natural selection.

CHAPTER III

THE THEORY OF NATURAL SELECTION

In 1813 a communication was read before the Royal Society by Dr. W. C. Wells upon the differentiation which exists between certain races of mankind. In Dr. Wells's paper this differentiation was explained from the facts that, since no two individuals are alike, some would be better fitted than others to resist the diseases proper to a particular country, and would consequently tend to survive, whilst their less fortunate neighbours would perish in greater numbers. Wells supposed the dark races of mankind to be better adapted to warm climates than white races are, and he thus applied to the particular case of the human species the true Darwinian principle of a gradual evolution through the survival of the fittest.

A similar view was applied to the origin of species in general by Patrick Matthews in a book on naval timber and arboriculture published in 1831.

Both these works were unknown to Darwin at the time of the first publication of the 'Origin of Species,' and it is quite unnecessary to point out that their existence does not in the least prejudice the value or originality of that great work. Their interest at the

present time is merely historical, as showing the direction in which thought was tending in the earlier half of the nineteenth century.

Before the 'Origin of Species' was published, A. R. Wallace communicated to Darwin a paper in which the bearing of the same idea was worked out at some length, and this paper was read, together with an abstract of Darwin's own views, at a meeting of the Linnean Society in July, 1858.

With this notice of other claimants to the idea of natural selection we may proceed to give an account of the theory as it is developed in the earlier chapters of the 'Origin of Species.'

We must first glance at Darwin's method of using the term variation. Darwin applied this term to every kind of difference which is found to occur between parents and their offspring, or between members of the same family, no matter whether these differences were great or small. It has since been shown that a number of quite distinct phenomena were in this way regarded from a single standpoint, without a proper discrimination being made between them. But the differences between continuous and discontinuous variation, quantitative and qualitative variation, and the rest, were not pointed out until long subsequent to 1859. Thus, beyond recognising a distinction between sports and individual differences, and attaching greater weight to the latter kind of changes, as being those which chiefly led to the origin of new species, Darwin made no further analysis of the facts of variation, but accepted all sorts of differences between individuals as

affording the material upon which natural selection might be expected to operate.

The idea that a selective influence exists in Nature arose from a study of the remarkable effects produced in the case of domestic animals and plants by the action of *artificial* selection. Darwin seems, however, to have been a little credulous in accepting the statements of certain breeders with regard to their power of producing any desired new type to order. Now that scientific men are themselves beginning to make experiments in breeding, with the check of exact records to act as a drag upon the exuberance of the imagination, they are becoming somewhat sceptical as to the mystic and almost miraculous powers attributed to the old-fashioned breeders, though, indeed, Mr. Luther Burbank would seem to be a survival from the period we speak of, if the statements of his recent enthusiastic biographer are to be credited.* Less gifted but more methodical observers find that they have no creative powers worth speaking of, and that all they can do is to keep a sharp look-out for the novelties which Nature may send them.

Selection, whether natural or artificial, can indeed of itself have no power in the direction of creating anything new ; its influence is destructive or preservative, but nothing more than this. The breeder keeps the new forms which take his fancy, and destroys the rest ; that is the whole story.

* Harwood, 'New Creations in Plant Life.' Mr. Burbank certainly seems to have a really wonderful instinct for the discovery of curious and useful novelties.

Yet a remarkable number of new kinds of creatures are known to have arisen in this way, and their diversity is no less astonishing, as a visit to any great show of domestic plants or animals will at once demonstrate. Here may be seen varieties of pigeons, for example, like the carrier, pouter, fantail, and tumbler, which, if they were found existing in a wild condition, would be placed in separate genera by any ornithologist. The domestic races of fowls, dogs, horses, sheep, and cattle show scarcely less divergence, and modifications no less remarkable have been perpetuated in the case of many cultivated species of plants. Whilst these types have survived, being deliberately preserved on account of their use or beauty or curious appearance, a still greater number have doubtless been exterminated, either because they did not attract the breeder's favourable attention, or on account of their having passed out of fashion.

Darwin sought in Nature a substitute for the baleful judgment of the breeder, and found it in an extension of the Malthusian doctrine to organic beings in general. The idea which is identified with this expression did not, however, originate with Malthus, nor does that author claim it as his own, as the following extract from the first chapter of the 'Essay on Population' will show:

'It is observed by Dr. Franklin that there is no bound to the prolific nature of animals and plants but what is made by their crowding and interfering with each other's means of subsistence. Were the face of the earth, he says, vacant of other plants, it might be

gradually sowed and overspread with one kind only, as, for instance, with fennel ; and were it empty of other inhabitants, it might in a few ages be replenished from one nation only, as, for instance, with Englishmen.'

Malthus' 'Essay' was first published in 1798, and was subsequently much enlarged. Its author proved incontrovertibly, by a survey of facts gathered from almost all the countries of the world, that human population tends to increase in a geometrical ratio, and that, consequently, after a time, the less gifted classes of any community are bound to suffer from a stress of poverty, only partly relieved by a high infant mortality, periodic famines, and similar factors, or in less civilized countries by infanticide and other artificial checks.

Among animals and plants in a state of nature the rate of increase is often very much greater than in the case of the human family, and even where it is not so, unchecked breeding would in a comparatively few years lead to the overpeopling of the earth with the descendants of a single pair. As an example of the rate of increase shown by a wild species, we may consider the case of the elephant, instanced by Darwin himself, since this is regarded as being one of the slowest breeders among all known animals. Darwin assumes that the elephant begins breeding at thirty years, and continues to do so until it reaches the age of ninety, bringing forth six young in the interval, and surviving to the age of a hundred. Then, if there were no casualties, he calculates that after from 740 to 750 years there would be nearly nineteen million elephants alive descended from the first pair.

Let us also consider the case of a minute rapidly breeding animal of a typical kind. My friend Mr. R. C. Punnett has recently been engaged upon an experiment which involved the breeding of rotifers, a kind of animal barely visible to the naked eye. They were bred for sixty-seven generations, and each individual produced on the average thirty eggs. The whole experiment occupied less than a year, yet Mr. Punnett calculated that if he had been able to rear all the animals which, at this rate of breeding, for this number of generations, were theoretically obtainable, he would have become the possessor of a solid sphere of organic material with a radius greater than the probable limits of the known universe.

This geometrical rate of increase is common in a greater or less degree to all living organisms. Since the space and food-supply available for the support of any species has no corresponding tendency to increase, it follows that a large proportion of the individuals born must perish before they reach the adult state, or at least without producing offspring. Darwin's contention is that there will be a strong tendency for those individuals which show slight modifications in the direction of a better adaptation to their environment to survive at the expense of those of their brethren which do not exhibit similar modifications. This is the principle called natural selection by Darwin, and by Herbert Spencer the survival of the fittest. Let us quote Darwin's own summary of the process :

'If under changing conditions of life organic beings

present individual differences in almost every part of their structure, and this cannot be disputed; if there be, owing to their geometrical rate of increase, a severe struggle for life at some age, season, or year, and this certainly cannot be disputed; then, considering the infinite complexity of the relations of all organic beings to each other and to their conditions of life, causing an infinite diversity in structure, constitution, and habits, to be advantageous to them, it would be a most extraordinary fact if no variations had ever occurred useful to each being's own welfare, in the same manner as so many variations have occurred useful to man. But if variations useful to any organic being ever do occur, assuredly individuals thus characterized will have the best chance of being preserved in the struggle for life; and from the strong principle of inheritance, these will tend to produce offspring similarly characterized. This principle of preservation, or the survival of the fittest, I have called Natural Selection. It leads to the improvement of each creature in relation to its organic and inorganic conditions of life, and, consequently, in most cases, to what must be regarded as an advance in organisation. Nevertheless, low and simple forms will long endure, if well fitted for their simple conditions of life.'*

We have here a very definite and concise statement of the way in which Darwin believed the principle of natural selection to take effect in the production of new kinds of organisms. It will be our business in this and in succeeding chapters to show how far the modern

* 'Origin of Species,' sixth edition, p 96.

study of the nature of individual differences and of other kinds of variations, as well as of the manner of operation of 'the strong principle of inheritance,' has confirmed this view as to the method of origin of species, or has led to the introduction of modifications.

Let it be remembered that this suggestion of a natural means of modification had, within a few years, the effect of convincing practically the whole thinking world of the truth of the theory of organic evolution—an effect which all the other arguments recited in the last chapter were quite unable to produce, so strong was the then existing prejudice in favour of the doctrine of special creation.

The truth of the general principle of the survival of the fittest is quite untouched by recent criticism; but a great deal of argument has been expended over the questions: (1) how much fitness is sufficient to lead to survival, and (2) whether very small advantages in the way of fitness, even if they lead to the survival of the individuals which exhibit them, will be followed to an indefinite extent in succeeding generations by further improvements in the same direction. We shall find that a good deal of evidence has accumulated tending to show that the second of these questions must be answered in the negative, although the point is not yet settled to the satisfaction of everyone. The remainder of the present chapter will be occupied in discussing some of the arguments which bear upon this question.

The fact that organic beings on the whole are, as a general rule, very closely fitted for the conditions in

which they have to pass their lives is clearly shown by the study of adaptations. This is a subject which those followers of Darwin who believe in the all-sufficiency of natural selection have brought into considerable prominence. For a full account of many supposed beautiful adaptations, from the point of view of the most prominent member of the school in question, reference may be made to Weismann's recently published book, 'The Evolution Theory.'

On the theory of natural selection in its extreme form, all the parts of an animal or plant—or, at any rate, all the points in which one species differs from another nearly related species—are supposed to have arisen on account of their usefulness to the creatures possessing them. Every detail of structure is thus regarded as being more or less closely adapted to the circumstances which attend the life of the animal or plant in question. This adaptation is never, indeed, regarded as perfect, because natural selection is always in progress, and its work is never absolutely done; but the point is that the features of every part are aimed at some useful purpose; or, if they are not, then they have been useful in former times and under different circumstances, and are now undergoing a process of gradual removal, because the individuals in which the useless structure is least developed will now have the best chance of surviving. That the form and structure of an animal or plant is in general closely fitted to its environment is of course true; otherwise the creature would very soon cease to cumber the earth. But the student of adaptation goes into details, and

endeavours to find a use for every minute point of structure, on the assumption, which we shall presently see to be open to criticism, that but for their usefulness these details would not exist. We may proceed to glance at one or two examples of the kind of thing which is meant when it is said that an animal or plant exhibits very marked adaptative features.

The family of whales belongs to the class of mammals of which the more typical members are land animals possessing four legs, and having their bodies covered with hair. We may deal in particular with the Greenland or true whalebone whales, since these are in many ways the most specialized members of the group.

The Greenland whale* has a spindle-shaped body like that of a fish, and its fore limbs are modified into flippers resembling the pectoral fins of fishes. The hind legs are only represented by a few rudimentary bones, which are completely hidden within the body wall, and the function of propulsion, which is performed by the hind legs in such less completely aquatic animals as seals, is here taken over by a great tail-fin which resembles that of a fish, except that it is placed horizontally. Hair is absent, but under the skin a thick layer of blubber is developed, which prevents a too rapid loss of heat, and at the same time adjusts the specific gravity of the body to that of the surrounding water. External ears are entirely wanting, and the waves of sound are apparently transmitted to the drum of the ear directly through the bones of the head.

* Weismann, 'The Evolution Theory,' English edition, ii. 313.

The external openings of the nostrils are placed quite on the upper surface of the head, so that the animal can breathe whilst almost completely submerged; and the larynx is so modified that the function of swallowing does not interfere with that of breathing. Perhaps the most remarkable feature of all is the enormous development of the head, and especially of the mouth. The huge jaws, in combination with the extraordinary plates of whalebone which fringe the edges of the mouth and act as a sieve, enable the animal to get its nutriment from the minute free-swimming creatures with which the surface waters of the ocean abound. Associated with this special method of feeding is the fact that teeth are only to be recognised in the embryo, and afterwards entirely disappear.

The whales differ in all these points from any other mammals, and failing almost any of these differences, would not be able to live in the special conditions in which they find themselves. It must therefore be admitted that we have here a case of very close adaptation of an animal to its natural surroundings, and one which extends to almost every detail of its structure. Darwin himself, moreover, has been at special pains to show how some of the most remarkable of these structural adaptations may possibly have arisen through natural selection.

One of the most remarkable cases of mutual adaptation, in which an animal and a plant are associated together, is shown by the method of fertilization observed to take place in the flowers of the Yucca plant of the Southern United States. The act of

pollination is performed by a moth—*Pronuba*—which possesses special organs particularly adapted for this purpose, in the shape of peculiar maxillary tentacles which are found in no other kind of moth. The female has also a long ovipositor with which she can pierce the tissues of the ovary of the plant, and so lay her eggs within it. With the aid of her peculiar tentacles the female moth collects from several flowers a ball of pollen of considerable size, which she kneeds into a firm pellet. She then carries this to a different flower, and after depositing a few eggs in the ovary she climbs to the top of the style and presses the ball of pollen into the stigma. Thus the ovules of the flower are fertilized, and whilst some are eaten by the larvæ of the moth, others develop into seeds and reproduce the plant.

The foregoing are perhaps two of the most remarkable cases known of animals having peculiar habits, and possessing at the same time special organs which render them well fitted for these habits and no others; but many other cases of scarcely less wonderful adaptations have been pointed out.

Darwin himself indicated the direction in which the study of adaptation was to proceed, and his books on 'Insectivorous Plants' and on the 'Fertilization of Orchids' afford us a delightful insight into a number of adaptive contrivances which are to be seen in plants. Another very interesting series of adaptive characters are those which have been gathered together under the heads of Protective Resemblance and Mimicry, and these have a special interest for us, because they illustrate the way in which the zeal of the seeker after adap-

tive contrivances may run away with him if not kept well in hand. For there is scarcely any limit to the number of problematical cases which have been described as adaptive resemblances, and so explained as having arisen through natural selection, whilst the evidence in favour of such a supposition is in many cases highly questionable. On the other hand, in a number of well-marked instances, the theory of mimicry certainly seems to afford an adequate explanation of the way in which many curious characters and structures may possibly have come into existence.

The families of the *Mantidæ* and *Phasmidæ*, including the walking-stick and leaf insects, afford many examples of animals which both in their colour and configuration show a very close resemblance to surrounding inanimate objects. This resemblance must have the effect of concealing them from their enemies, and more particularly from their prey, as, indeed, a study of their habits indicates quite clearly.

Phyllopteryx, an Australian fish allied to the well-known sea-horse (*Hippocampus*), is provided with a number of irregular appendages of ragged skin resembling the seaweed amongst which this animal is found. In this way the characteristic symmetrical appearance of a live animal is got rid of, and the creature is rendered extremely difficult of observation. Here, again, the concealment afforded is probably useful in leading to the deception of the smaller organisms upon which the creature feeds.

Examples of this kind in which the shape of an animal leads to its concealment are comparatively

infrequent, although a considerable number might be collected. On the other hand, some resemblance between the *colour* of an animal and its surroundings is to be traced in the majority of the members of many groups. Familiar examples are afforded by the white colour of animals which live in snow, the tawny grey colour of most desert species, the green of grass-frequenting animals, and so on. It is perhaps not quite certain that in some of these cases the peculiar colour is not evoked by the direct action of some cause which affects different species in the same way; but such a cause, awaits discovery, and in the meantime natural selection has certainly a strong claim to be regarded as the proper explanation.

A more strict use of the term mimicry, however, is to restrict it to cases where one species apes the colour pattern or other external character proper to another species which inhabits the same region; and the idea of mimicry has been put forward as especially appropriate in cases where the mimicked species is common, and can be thought to possess some special means of protection. Several supposed examples of this phenomenon have been described in the case of different genera of tropical butterflies, but the best of them seem to be open to criticism, since there is nothing to prove that colour patterns of the same type may not have arisen from the same causes in quite different groups. In cases where the environment to which the different forms were exposed was similar—as would be the case in a single locality—such a process of parallel evolution might be thought to be all the more likely.

Resemblances can only be properly explained as representing cases of mimicry when both the species concerned—the mimic and the mimicked—inhabit the same locality; but plenty of cases of matching between the colour patterns of insects which live in quite different parts of the world could also be pointed out. Let us take a concrete example. Everyone is familiar with the flower-frequenting flies (*Syrphidæ*) which are to be seen hovering about plants in sunny weather. These insects closely mimic the appearance of various small bees and wasps, the habits of which are similar. Here, then, is surely a case where the deceptive resemblance to an animal well armed in its sting must cause prospective enemies to let these flies alone. In Southern Japan, as Dr. Andreæ pointed out to me, flies of this kind are surprisingly numerous, and their resemblance to bees particularly noticeable. So abundant are they that, from the point of view of the flowers which they visit, these flies doubtless provide an efficient substitute for the bees of other countries, which are here conspicuous by their absence. But if real stinging insects are wanting, or even very scarce, the supposed enemies of the flies can have no experience of the ill-effects produced by catching them. How, then, can these flies benefit from their resemblance to bees?

This kind of thing must make us somewhat suspicious of supposed cases of mimicry even between species possessing the same range.

When the ideas of mimicry and protective resemblance are carried into the vegetable kingdom, as they have been by some writers, absurdities are soon found

to arise. Thus it has been suggested that the leaves of dead nettles resemble those of the common nettle for the sake of the protection so afforded, and that the mottled stems of certain tropical herbaceous plants gain a similar immunity on account of their resemblance to snakes.

In plants a great number of fanciful resemblances between different species can be detected, and some between plants and animals, very few of which can be supposed to be of any possible utility to the species which exhibit them. They must be regarded as cases of parallel evolution, the causes of which are quite unknown. Such resemblances as that between the shoots of *Casuarina indica* and those of the common horse-tail, between *Saxifraga hypnoides* and certain mosses, between the horse- and Spanish-chestnut, between the seed of a pine and the fruit of an ash-tree, are so frequent in the vegetable kingdom as to be the delight of malicious examiners in elementary botany. It is impossible to believe that in such cases the resemblance is in itself of any value to either species, and few people will be found to maintain that the likeness of a bee- or spider-orchis to an insect is of any utility to either animal or plant.

But if resemblances can arise which are useless, and which, consequently, cannot be explained through natural selection, it becomes uncertain whether this principle can hold good as the true description of the origin of any sort of resemblance. On the other hand, resemblances which are useful will tend to *survive* through natural selection in whatever way they may

have arisen. This last consideration will account for the frequency with which apparently adaptive likenesses are to be found in nature, even if we suppose that their origin was 'accidental,' or simply due to the operation of similar external causes. The same criticism applies to all cases of adaptation of whatever kind, so far as concerns their supposed method of origin by the action of natural selection upon individual differences.

Perhaps a still more serious criticism of the methods of those who spend their time in seeking out or devising cases of adaptation has been made by Bateson, who points out the logical difficulty that we can never make any quantitative estimate of the amount of benefit or the reverse which any particular structure may afford to its possessor. It is easy enough to imagine particular circumstances in which an organ developed in a particular way may be of undoubted service, but whether the net amount of such service throughout the life of the creature considered is greater or less than the strain upon its resources caused by the development of such an organ is quite beyond our powers of determination.

'The students of adaptation forget that even on the strictest application of the theory of selection it is unnecessary to suppose that every part an animal has, and everything which it does, is useful and for its good. We, animals, live not only by virtue of, but also in spite of what we are. It is obvious from inspection that any instinct or organ *may* be of use; the real question we have to consider is *how much* use it is.

To know that the presence of a certain organ *may* lead to the preservation of a race is useless if we cannot tell how much preservation it can effect, how many individuals it can save that would otherwise be lost; unless we know also the degree to which its presence is harmful; unless, in fact, we know how its presence affects the profit and loss account of the organism.'*

A great many other criticisms and objections have been brought at various times against the theory of the origin of adaptations by the action of natural selection, and many of these were considered and replied to by Darwin in the later editions of the 'Origin of Species.' We shall only consider here a few which have been put forward more or less recently. Before doing so it will be well to point out once more that no one questions the validity of natural selection as a means of exterminating types which are unfitted for their environment—there is clearly a tendency for the fittest types to survive once they have come into existence. Nor can there be any doubt that species in general are well adapted to the conditions which their environments present. But when this is admitted it does not necessarily follow that natural selection, directing the accumulation of minute differences, has been the method by which these adapted forms have originated.

The power of regenerating a lost part must clearly often be of service to the creatures which possess it. Such a power may in many cases be considered to be a well-marked adaptation. But, as Morgan has well

* W. Bateson, 'Materials for the Study of Variation,' p. 12.

pointed out, there are insuperable difficulties in the way of adopting the belief that such a power can have been acquired through the action of natural selection. Let us consider the power of regenerating a lost tail, such as is shown by the common gecko, or wall-lizard, of the tropics. To account for this power by natural selection we should have to suppose, firstly, that every stage in the growth of a partly regenerated tail, even its first small rudiment, was useful to the animal ; and, secondly, that there was so much competition between lizards which had lost their tails, that those which could regenerate them a little better would survive rather than the others. The first of these suppositions as to the utility of a partly regenerated tail is in the highest degree improbable ; but against the second there is an entirely fatal argument, since, if the lizards which regenerated badly were exterminated owing to competition with those which had better powers of regeneration, much more would all the injured lizards be exterminated in competition with those which had escaped injury.

The theory of sexual selection constitutes an important branch of the Darwinian account of the origin of specific structures. We are here concerned with this hypothesis only in so far as it leads to a criticism of the efficacy of natural selection from another point of view. By the theory of sexual selection Darwin attempted to explain the origin of two sorts of characters in particular, one or other of which frequently appears in the male sex only of many of the higher animals.

In the first place, we have to notice the presence of special weapons, such as horns or tusks, developed exclusively or to a special extent in the males of those species in which it is the habit of the members of this sex to strive together for the possession of the females. In such cases the stronger and better-armed males are supposed to survive, and to leave a greater number of offspring than their weaker rivals, so that this form of competition is regarded as acting in quite a similar way to natural selection.

In a second set of cases, of which many remarkable instances are to be seen among birds, the males are found to exhibit brilliant and varied colours, or to possess special decorations in the form of plumes or other appendages, or to be gifted with the power of song. It is to cases such as these that the term sexual selection more properly applies, since the females are supposed to bestow their favours upon the most beautiful males, and to reject the advances of those among their suitors which are less lavishly provided with ornament.

In these cases, where the development of brilliant colours or other ornamental arrangements is believed to have taken place owing to the choice of the females —particularly in such a case as is represented by the peacock's tail or the wings of the Argus pheasant— the supposed change must have come about in direct opposition to the action of natural selection, since the latter would favour a production of colours resembling those of the natural environment for the sake of concealment, and would hinder the formation of such

exaggerated appendages on account of the loss of activity which they must entail. We are, therefore, obliged to conclude that natural selection is much less rigorous in its action than some people have supposed, for if this principle is inadequate to prevent such an exuberance of form and colour in these particular instances, its action becomes open to question in other cases as well.

Similarly, Morgan finds a difficulty in understanding why natural selection has not led to the extermination of species which are handicapped by the existence of internecine strife between the males, in favour of other species which faced the battle of life with united strength. But in this argument it seems to be forgotten that examples of the kind of strife in question are most frequent among herbivorous animals, where the struggle for existence must be chiefly determined by the quantity of vegetable food which the individuals can obtain, so that the loss of the weaker males may not be a disadvantage. Moreover, Darwin's conclusion that natural selection is most rigorous between members of the same species is left out of account.

The preceding arguments seem to show that in particular cases certain structures and phenomena associated with species cannot be explained as having arisen through the unaided action of natural selection. When weighed against the great mass of evidence which Darwin accumulated in support of his theory, these few considerations cannot be said to be in any way fatal to the belief that natural selection of minute differences has played an important part in the origin

of species. Still, they add in some measure to the weight of recent evidence which points to the conclusion that many specific structures have had a different method of origin. We have already pointed out that there are two alternative methods, each of which has its adherents. Before passing to a consideration of the now prevalent view of mutation, something still remains to be said with regard to the remaining theory—the theory of Lamarck.

Darwin himself, as we have seen, admitted the minor importance of the inheritance of acquired characters, as well as that of the phenomenon of sporting, regarding both these processes as causes of the origin of new species subsidiary to the action of natural selection upon individual differences, whilst he looked upon the latter as the main process in organic evolution.

Later writers, however, have asserted that natural selection is the sole cause of the origin of species, and in particular they have denied any effect to the inheritance of acquired characters—the Lamarckian factor—asserting that there is not, and cannot be, any such inheritance. Among the most distinguished opponents of the theory of use-inheritance were A. R. Wallace, the co-discoverer of natural selection; and Professor Weismann, who has argued the case with particular ability. Much the most able defender of the principle of use-inheritance was Herbert Spencer, who was one of the few who had thoroughly convinced themselves of the truth of the theory of evolution years before the 'Origin of Species' made its appear-

ance. Since all arguments in favour of the evolution of species were incomplete unless some means by which such an evolution could take place had been suggested, Spencer adopted the Lamarckian theory of modification, and to this he always firmly adhered, though admitting the validity of natural selection as an additional factor in the process. Some of Spencer's arguments in favour of a belief in the inheritance of acquired characters are well worth repetition, since they have never been altogether refuted.

Herbert Spencer's argument consisted mainly in the enumeration of structures the origin of which cannot be explained by natural selection. On the other hand, the inheritance of acquired characters, if this form of inheritance could be proved to have a real existence —as Spencer believed it could—was shown to be a perfectly adequate explanation of the origin of the structures in question. In 1893, when Spencer upheld his opinion for the last time, Bateson had not yet pointed out that the facts of definite and discontinuous variation afford an alternative way out of some of these difficulties. In the absence, therefore, of any other effective cause, the result of the argument pointed strongly to the conclusion that the inheritance of acquired characters must be a reality.

The first of Spencer's arguments was based upon the different powers of tactual discrimination which are to be found in different parts of the human body. The degree of this sensitiveness may be estimated by the use of a pair of compasses, the points of which can be set at different distances apart. It is then found

that with the tip of the forefinger the points can be distinctly recognised as two when they are separated by no more than $\frac{1}{12}$ inch. When applied to the middle of the back, on the other hand, the points must be opened to a distance of $2\frac{1}{2}$ inches before the sensation of a single touch becomes resolved into two distinct sensations.

The distribution of this power of discrimination over the surface of the body is approximately as follows :

Tip of tongue	$\frac{1}{24}$ inch.
Tip of finger..	$\frac{1}{12}$,,
Inner surface of second joint of finger	$\frac{1}{6}$,,
Tip of nose	$\frac{1}{4}$,,
Cheek, palm of hand, and end of great-toe	$\frac{1}{2}$,,
Forehead	$\frac{3}{4}$,,
Back of hand, crown of head ..	1 ,,
Breast	$1\frac{1}{2}$,,
Middle of back, middle of thigh, middle of forearm	$2\frac{1}{2}$,,

Now, it is out of the question to suppose that natural selection can account for all these differences. An increased sensitiveness of the tips of the fingers might, indeed, be of so much use as to give the individual possessing it a definitely increased chance of survival. But it is hard to believe that it can be important for a man to have the tip of his tongue twice as sensitive as the tips of his fingers. And why should the tip of the nose be more sensitive than the cheek, or the cheek than the top of the head, or the breast than the back ? In the last case it might even be suggested

that in a savage, since the sense of touch is the only one with which his back is provided, it might be useful for that surface to have acquired a more delicate sense of touch than the anterior surface, which is guarded by the power of vision, as well as being more readily explored by the sensitive finger-tips. If such an argument is regarded as far-fetched, so in an equal degree must be any attempt to explain the actually observed distribution through the action of natural selection.

On the other hand, Spencer points out that the series of parts enumerated in the above table stands in almost exactly the order of the frequency with which the members composing it are actually exposed to tactual experience.

The tongue is perpetually in contact with the minute unevennesses afforded by the surfaces of the teeth.

The palm of the hand and the lower joints of the fingers are used chiefly in grasping, and not in the more minute manipulations for which the finger-tips are employed. And the experience of the back of the hand in coming into contact with various irregular bodies is not to be compared with that of the palmar surface, yet it is very much greater than that of so unexposed a part as the middle of the forearm.

For the carrying on of his argument, Herbert Spencer has shown that increased use of the power of discriminating small objects by touch is accompanied by an increased degree of sensitiveness in individuals. Blind people use their finger-tips in this way to a much greater extent than those whose sight is unimpaired.

Two blind boys examined by Spencer were both found to be able to distinguish with the tips of their fingers points separated by only $\frac{1}{14}$ inch. And two skilled compositors could both distinguish in this way points placed no more than $\frac{1}{17}$ inch apart, so that a person with a trained sense of touch acquires a considerably finer development of this faculty than an ordinary individual.

If, then, acquired characters of this kind are inherited, even to an extremely minute extent, such as would be scarcely perceptible in a single generation, the account of the origin of the observed phenomena would be complete.

As a second argument, Herbert Spencer points out the difficulty of accounting for the development of co-ordinated sets of structures by the action of natural selection upon separate minute variations of the several parts concerned.

The enormous horns of the ancient Irish elk, weighing in some cases over a hundredweight, required specially strong neck muscles, bones, and ligaments, and strong fore legs for their support. But an increase in the strength of a single muscle following increased weight of the horns would be useless if unaccompanied by a corresponding increase in many other structures, and, if useless, could not be selected. The chance of all the parts concerned varying simultaneously in a corresponding direction is very small if these variations are really independent, and the chance of their doing so repeatedly is in such a case infinitesimal.

Let us take another case of a similar nature. The

hind legs of such an animal as a cat are admirably adapted for the purpose of making a spring. In order to arrive at such a structure by the modification of limbs previously adapted only for running, changes must occur in almost all the bones, muscles, and ligaments of the limbs, and these changes must keep pace with one another so that one part may not grow out of proportion with the rest. It is quite impossible to suppose that this can be effected by the natural selection of minute fortuitous variations of the various parts, each occurring independently. But simultaneously with these changes the fore legs have become modified in a totally opposite direction. They have become straight, firm, and pillar-like for receiving the weight of the body in the downward leap. Compare, says Herbert Spencer, the silence of a cat's leap up on to a table with the thud made by the fore legs as it jumps down upon the floor.

Modification of the fore legs and of the hind must thus have proceeded in almost exactly opposite directions in the two cases, and in each a great number of parts are separately co-ordinated. For natural selection to have had any effect, all the co-ordinated parts of one pair of legs must have varied in one direction, whilst similar parts in the other pair of legs varied simultaneously in another direction. It is out of the question to suppose that this could have happened simply by chance.

'What, then, is the only defensible interpretation? If such modifications of structure produced by modifications of function as we see take place in each indi-

vidual are in any measure transmissible to descendants, then all these co-adaptations, from the simplest up to the most complex, are accounted for. In some cases this inheritance of acquired characters suffices by itself to explain the facts; and in other cases it suffices when taken in combination with the selection of favourable variations. An example of the first class is afforded by the change just considered; and an example of the second class is furnished by the case, before named, of development in a deer's horns. If, by some extra massiveness spontaneously arising, or by the formation of an additional "point," an advantage is gained either for attack or defence, then, if the increased muscularity and strengthened character of the neck and thorax, which wielding of these somewhat heavier horns produces, are in a greater or less degree inherited, and in several successive generations are by this process brought up to the required extra strength, it becomes possible and advantageous for a further increase in the horns to take place, and a further increase in the apparatus for wielding them, and so on continuously. By such processes only in which each part gains strength in proportion to function can co-operative parts be kept in adjustment, and be re-adjusted to meet new requirements. Close contemplation of the facts impresses me more strongly than ever with the two alternatives—either there has been inheritance of acquired characters, or there has been no evolution.'*

As we pointed out in the last chapter, there seems at

* Herbert Spencer, 'The Inadequacy of Natural Selection,' p. 29.

first sight to be no inherent difficulty in the way of acquired characters being inherited. Weismann has, however, pointed out a very serious difficulty, which is brought into prominence on making a study of the minute anatomy of the cells of organisms during the earlier stages of their development.

In the ordinary course of events every one of the higher animals and plants begins its existence in the form of a single minute cell—the fertilized ovum or egg. This cell exhibits no trace of the complicated series of organs which will develop from it when it is subjected to the proper conditions. When the egg is placed in favourable circumstances with regard to warmth, moisture, food-supply, and the like, it first divides into two equal portions; and microscopic study shows that elaborate precautions are taken to insure the equal bipartition of its minute constituent parts. Each of the two cells thus formed divides again into two further cells, and by a series of repeated bipartitions of this kind the cells which constitute the adult body are at last brought into existence. Since the body soon becomes differentiated into a number of unlike organs, it is clear that at certain stages of the process the two cells arising from a division must come to differ slightly from one another; and the cells ultimately produced show very considerable differences of form, structure, and size. Among all the cells which finally arise those which have undergone the least modification from their original condition are those from which are developed the sexual reproductive cells, or germ-cells, of the organism. Indeed, Weismann con-

cludes that there is no reason for supposing that these have undergone any modification at all.

If we consider the cells which build up an adult organ, and for the moment regard each separate cell as an individual, we see that each of these individuals possesses an ancestry of cells stretching right back to the fertilized ovum—the single cell in which the whole organism originated. So far as the later cell divisions are concerned, the cell-lineage of a particular organ is separate and distinct from that of the cells of any other organ. At a certain distance back in the history of the organism we shall come across a common cell-ancestor for the cells belonging to a pair of neighbouring organs, and the more widely separate the parts to which the cells we are considering belong the further back must we go before we find their ancestry merging in a single cell. In a similar way as with other organs, so it is found that the sexual cells or germ-cells of an adult organism have a history quite distinct from that of the cells of any other part of the body; and these cells are the only ones which are concerned in the formation of the offspring. Thus we see that the particular cell-lineage leading up to the germ-cells is the only one which is continued into another generation; all the others terminate with the death of the individual creature of which they form a part. From this point of view we may consider the nature of a given series of animals as being determined only by the particular series of cells which constitute the direct ancestry of the germ-cells in each individual; the cells which make up the bodily structure are the

result of so many offshoots which come to an end at the death of the organism, and leave no progeny of their own.

Wilson has expressed this view of Weismann's very clearly: 'It is a reversal of the true point of view to regard inheritance as taking place from the body of the parent to that of the child. The child inherits from the parent *germ-cell*, not from the parent body, and the germ-cell owes its characteristics not to the body which bears it but to its descent from a pre-existing germ-cell of the same kind. Thus the body is as it were an offshoot from the germ-cell. As far as inheritance is concerned the body is merely the carrier of the germ-cells which are held in trust for coming generations.' (The diagram illustrating Weismann's theory of inheritance is a modification of that given by Wilson.*)

G
G
S
G
S
G
Line of hereditary transmission.
Line of succession of individuals.

FIG. I.

G, Germ cells; S, Somatic cells.

In the light of this conception it may be seen that the idea of the inheritance of a modification acquired by an adult bodily organ is comparable with the supposition that if a man develops his muscles by exercise his brother's children will be thereby modified.

* 'The Cell in Development and Inheritance,' p. 13.

The minute study of the germ-cells, taken in connection with modern experimental work on the methods by which inheritance takes place, shows a strong tendency to confirm Weismann's view, so far as the case of distinct and definite characters is concerned. But if we regard such definite characters as having arisen by definite steps or mutations according to the view now gaining ground, the study of them will have no bearing upon the question of use-inheritance, since use does not lead to large and definite changes in the individual, but to comparatively small changes of a quantitative kind.

There are some, including de Vries, who regard all fluctuating variations (individual differences) as being of the nature of acquired characters, and as being at the same time capable of hereditary transmission, although de Vries believes the amount of progress possible in this way to be strictly limited. Let us see if there is any way in which a transmission of such characters can be conceived of.

It must be pointed out that the cells which make up an organism are not completely marked off and separated from one another ; on the contrary, it seems impossible to doubt that reactions may take place between them long after their first formation. Indeed, Sedgwick has shown that in a number of diverse kinds of animals there is never any sharply limiting barrier between cells at all, and this writer has gone so far as to speak of animals in general as being built up of a continuous network of protoplasm with nulcei at the nodes. In plants, too, though at first sight their con-

stituent cells seem to be cut off from one another like so many closed boxes, it has been shown that there is almost universal communication between the protoplasmic masses so enclosed, in the shape of minute fibrils of living substance which traverse the intervening walls.

It would thus seem possible for liquid or easily soluble substances to pass freely from one part of the body of an organism to another. It is possible, for example, supposing the enlargement and strengthening due to the exercise of a particular muscle to be associated with an increased production of some definite chemical substance, to imagine that an increased amount of the same substance might become enclosed in the germ-cells, so that this substance would be present in the offspring in greater abundance than would have been the case if the muscle of the parent had not been exercised. And this might facilitate a further development of the same muscle by exercise in the next generation. In a similiar way increased bulk following upon better nutrition might be inherited, and this de Vries seems to have succeeded in showing to be actually the case in plants. Such changes might normally be so slight as to be almost imperceptible in a few generations, and yet after many generations might accumulate to an important extent. It would be impossible in practice to distinguish changes of this kind from what are known as accidental individual differences, and, indeed, there is no evidence at hand to disprove de Vries' assertion that all continuous variations are of the nature of acquired characters—

and we know that continuous variations are inherited.

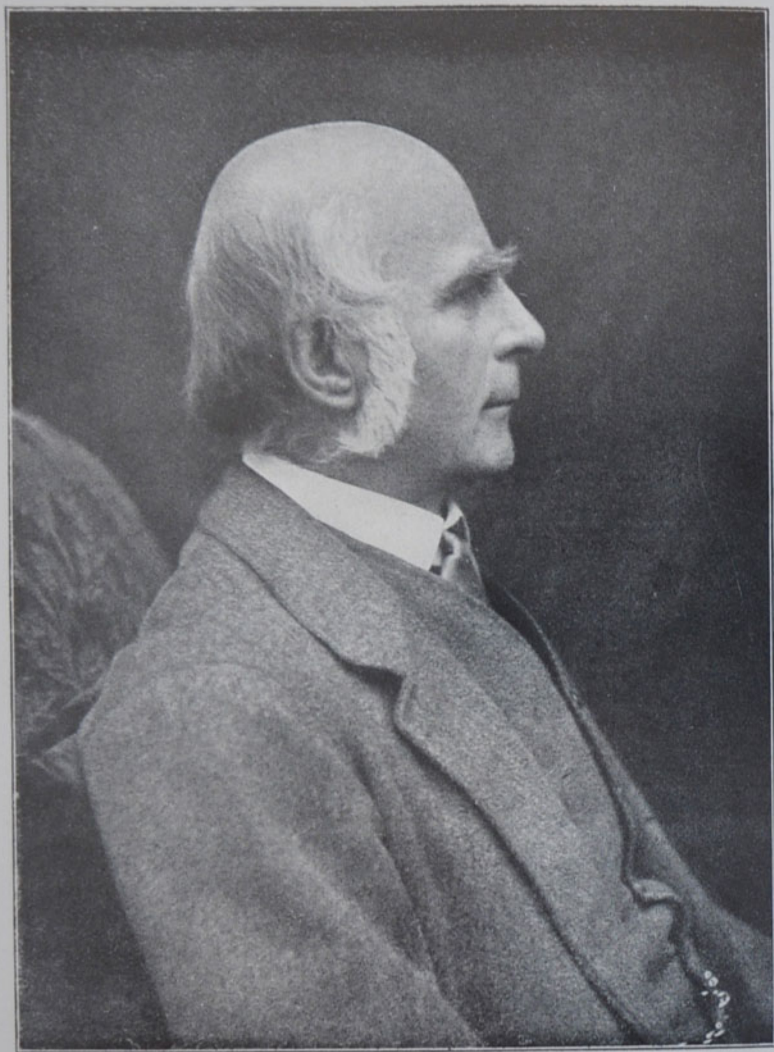
On the other hand, several lines of inquiry have separately led to the conclusion that a great number of the visible characteristics of organisms are of a definite kind, and are inherited definitely, their appearance being determined by the presence of definite structures or substances in the germ-cells. The evidence, as we shall see later on, points to the conclusion that such characters have arisen suddenly at a single step, and we must conclude that in such a case a definite change in the germinal structure has been followed by a definite alteration in the character of the organism arising from the germ; since no one can suppose that a large and definite structural alteration can be first acquired by the adult organism and then inherited by its offspring—such a process is unthinkable.

Thus we see that the inheritance of acquired characters, if such inheritance really takes place at all, must be confined to the transmission of changes of an indefinite and quantitative kind—to the case, in fact, of continuous variations or individual differences. Moreover, there is nothing to show that all continuous variations are not of the nature of acquired characters.*

* We know, at any rate, that continuous variations are not invariably due to the cause which Weismann supposed—namely, to the mingling together of characters derived from the two parents—a supposition which is of fundamental importance to his theory—because, as Karl Pearson has pointed out in this connection, parthenogenetically reproduced organisms, in which no such mingling has taken place, may be just as variable as those which owe their origin to the process of sexual generation.

It is possible that variations of this nature may gradually lead to important and even to specific changes, but whether this is the case still remains to be proved. On the other hand, we shall see that specific differences do sometimes arise at a single step, and there is strong but indirect evidence to show that this is the way in which a very great number of specific differences have actually arisen. Indeed, some have contended that this is the universal process by which such differences originate, but this again is not proved, nor is it altogether likely. In any case the inheritance of acquired characters can have nothing to do with that of definite and discontinuous differences.

This is a problem to which we shall return in the concluding chapter, in the light of further evidence concerning continuous and discontinuous variations and their manner of inheritance, which will be by that time available.



FRANCIS GALTON.

[*To face p.* 73.

CHAPTER IV

BIOMETRY

In the present chapter we have to consider in some detail the manner in which purely statistical methods have been applied to certain biological data, a proceeding to which the term biometry has been attached by Professor Karl Pearson. Before concluding our account we shall give a brief sketch of some of the more important evidence bearing upon the problems of evolution which has been brought to light by the methods of biometrical science.

The first investigator to apply the methods of statistics to the solution of biological problems was the Belgian astronomer, Quetelet. In 1845, in the form of a series of letters addressed to the Grand Duke of Saxe-Coburg and Gotha, Quetelet published an admirable account of the theory of probability and its relation to human affairs, and one in which the use of advanced mathematics was avoided. The pioneer of biometry in this country is Francis Galton, whose book on 'Natural Inheritance' embodies an extremely lucid introduction to the statistical study of variation and inheritance. From these two works are derived most of the ideas submitted in the present chapter.

The more recent advances in biometry are mostly the result of work published by Professor Karl Pearson; they consist largely in the elaboration of mathematical methods of dealing with statistical problems, and as such it would be inappropriate to give any further account of them here.

The mention of the word 'statistics' at once raises a certain prejudice in the ordinary mind; in common parlance, the unreliability of arguments based upon statistics is sometimes treated as proverbial, and as used in biology they have, as a matter of fact, one very serious danger at least. Statistics deal with groups and not with individuals, and there is a real difficulty involved in the fact that the average of a group may represent something quite different from any individual which the group contains, whilst at the same time a group may include individuals of very diverse natures. Nevertheless, when used without prejudice to the future examination of individual inheritance by more detailed investigations, the methods of biometry have undoubtedly yielded information of great value to the evolutionist, particularly in the case of such material as that afforded by the human race, since the application of precise experiments to this particular species is at present out of the question.

Some students of biometry, however, would go very much further than this, for it is their professed opinion that their own form of study is the only method by which any real advance in our understanding of the processes of evolution can be brought about. This opinion is based upon the assumption, of which proof

is wanting, that new species have arisen exclusively through the accumulation by natural selection of variations of a strictly indefinite, fluctuating, or normal kind. We have already seen reasons for believing that this is very far from being the case, and future chapters will be found to add considerably to the force and quantity of the evidence already adduced.

Normal variations, strictly speaking, are individual differences which can be supposed to depend upon a large number of small factors or causes—factors so numerous and so minute that the numerical distribution of the individuals examined, when ranged in order according to the feature chosen for examination, is found to conform closely to that which would be expected on the mathematical theory of chance. Such a distribution will only result when the differences considered can be strictly regarded as lying upon a linear scale, and when they are also evenly distributed along that scale. That is to say, the biometrician deals with continuous variations of a quantitative kind. It is to be hoped that these somewhat obscure sayings will be more easily understood in the light of what follows.

The facts of variation have not been found readily amenable to precise definition, but we shall endeavour to make plain by the aid of a few examples what kinds of variations do and what kinds do not appear to be legitimate objects for the application of biometrical methods. Thus it may be thought that the biometrician is outrunning his license when he ranks the colours shown by the iris of the human eye in a con-

tinuous series of eight shades, because in doing so he groups together a number of probably definite factors with others which are of an indefinite kind. When the colours of the human eye come to be studied in greater detail, there can be little doubt that they will be found to depend upon some such factors (among others) as the following:

1. (*a*) Definite differences in structure, and (*b*) the definite presence and absence of pigment in certain definite positions; as well as—

2. (*a*) Indefinite variations (individual differences) in structure, and (*b*) in quantity of pigment—if, indeed, the quantitative differences are not found to be also definite.

In the above example a suitable and legitimate object for biometrical investigation would be the differences in amount of a particular pigment.

But definite differences may also exist in the case of an apparently simple quantitative character. The accompanying figure (Fig. 2) shows the variations in length of the fruits of three different but closely allied species of evening primrose, as measured by de Vries. In this diagram the vertical distances are in each case proportional to the number of individuals having particular lengths of fruit, and the actual length of the fruit is in each case proportional to the horizontal distance from an imaginary vertical line some way to the left of the figure; the points thus plotted are joined by straight lines, so that a polygonal figure is obtained representing the nature of the variation in each particular case. The diagram shows at once that

the species A and C have each a characteristic mean size of fruit, and the existence of this definite mean is not affected by the fact that the range of variation

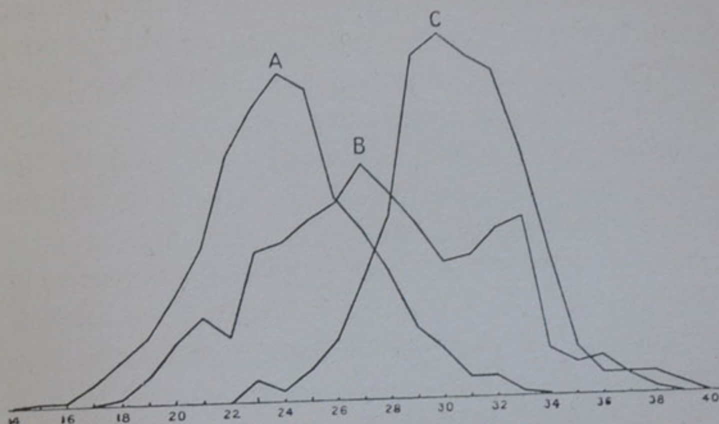


FIG 2.

overlaps in all three cases. Species B, on the other hand, seems to show signs of division into at least two separate groups.

Differences of a similar kind are sometimes to be found among the progeny of the same individuals. Races of garden peas may be selected which, amongst other differences, are characterized by the presence of large and of small seeds respectively. In each case there is variation of a normal kind about a mean value, but in each case the mean is quite distinct. There is evidence that if a race of large-seeded peas is crossed with a small-seeded variety, and the resulting cross-bred plants are self-fertilized, their progeny in the second generation will be separable into different groups, and some of these will show almost exactly the same size

characteristics as those which were exhibited by the two original parental strains. The only difficulty in the way of invariably distinguishing the two original kinds, after their segregation in the offspring of the cross, lies in the fact that the smallest seeds of the large type may be smaller than the largest seeds of the smaller strain, and this is a difficulty which applies equally to the original strains before crossing, as well as to the case of the evening primrose fruits just mentioned.

Now, it is clear that if we mixed together the seeds of several different races of peas in the proper proportions, the result might lead to a normal distribution of the kind presently to be described. The several races, however, would none the less be perfectly distinct, even though we could not separate the individual seeds belonging to each by any direct method.* Such a mixture of races would constitute a decided pitfall for the unwary statistician, and it is well to remember that, after even the most elaborate mathematical analysis, the final result cannot be clothed with any greater amount of certainty than the facts from which the calculation set out. Those who have made a large expenditure of intellectual effort in such processes have, unfortunately, a natural tendency to overlook this elementary fact.

Prior to the application of statistical methods to a particular case of normal variation a number of preliminary processes have to be gone through.

* It would generally be possible to decide which strain a particular seed belonged to by sowing it and observing the variation of its offspring.

Having selected a particular character for investigation, we must make a quantitative estimate of its development in each member of a fair sample of individuals which show the character in question. What is to be understood as a fair sample was well expressed by Quetelet when he wrote that statistics must be collected without any preconceived ideas, and without neglecting any numbers. We shall find that in this point the biometrical method differs from the method introduced by Mendel, since in the latter careful discrimination of data is an essential feature.

The quantitative determination of a character may be made either by counting or by measurement. That is to say, we must proceed by measurement if the character we are dealing with is one of size or weight, and by counting if the character shows a series of numerical values of its own—*e.g.*, if it is such a character as the number of veins in a leaf or the number of stigmatic bands on a poppy capsule. Before we make any determinations we ought to be quite certain that we are dealing with the same character in each individual, and that the individuals themselves are truly comparable with one another. Thus we might make a series of measurements of a particular bone in a particular limb of a particular race of human beings with some assurance that we should be dealing with homogeneous material.

Our measurements or countings will fall either naturally or artificially into groups. In the case of countings the groups are naturally limited by the

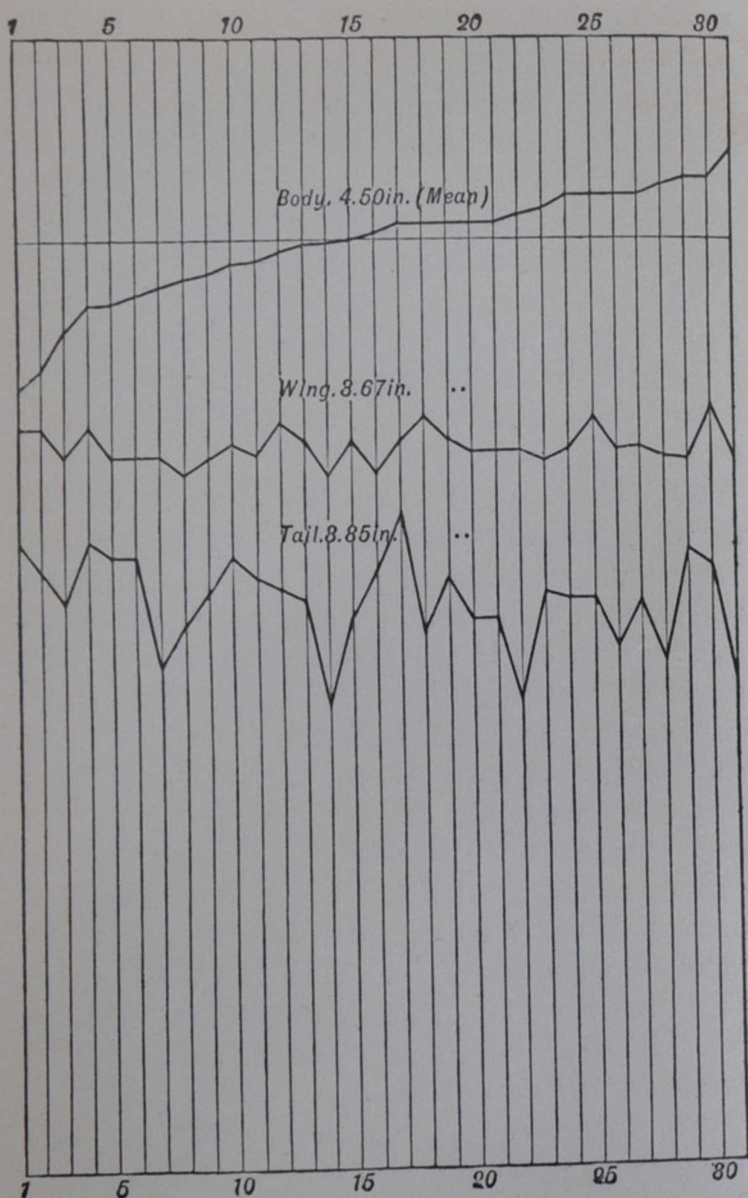


FIG. 3.

numbers which represent the character of each individual, whilst measurements are artificially limited through the fact that they have to be made in units of some kind—*e.g.*, to the nearest inch or some other value. Such groups, characterized by equality of range—each, that is to say, covering an equal number of units—are technically known as *classes*.

Thus if we are dealing with human stature, and if our measurements were made only to the nearest inch, all the individuals of 6 feet in height would fall into one class, those of 6 feet 1 inch into another class, and so on. If, on the other hand, we were engaged in counting the number of ray florets in the heads of daisies, a class would include all those heads which possessed a particular number of rays.

Without division into classes, however, a survey of a comparatively small number of measurements may be facilitated by ranging the values in some kind of order. This is done, for example, in the accompanying figure for the measurements to hundredths of an inch of the lengths of the body, wing, and tail of thirty-one specimens of a North American bird. The diagram is taken from A. R. Wallace's 'Darwinism.'

Even with this small number of measurements the diagram brings out two points very clearly. In the first place, there is no close correspondence between the variations in length of body, wing, and tail. Secondly, in the case of body-length, in respect of which the specimens are ranged in order, the number of individuals of a medium size is seen to be greater than the number of those which show extreme values. This

excess of mediocre individuals comes much more prominently into view as soon as a larger number of measurements can be considered, and the results arranged in a different way.

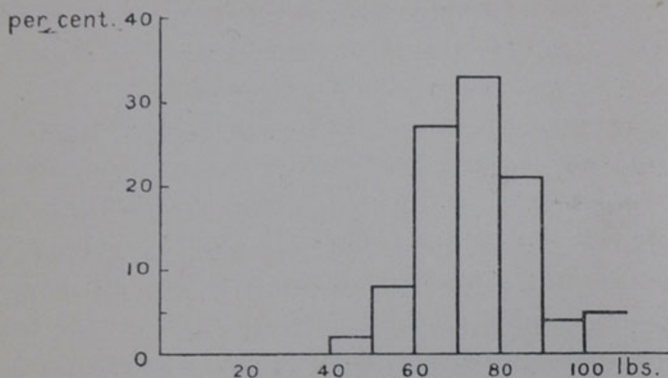


FIG. 4.

The above diagram is constructed from the entries in the third column of the accompanying table, which is taken from Galton's 'Natural Inheritance.' It represents the variations in the strength of pull (as exerted by an archer in drawing a bow) shown by 519 men as recorded at the International Health Exhibition in 1884. Here equal distances measured off along the base line represent equal increments in the strength of pull of the right hand, and the vertical heights of the rectangles erected upon these bases represent the percentage numbers of the men examined which exhibited each value of the character under consideration. In this example it is easy to see that the central class is the largest, whilst the extreme classes contain a comparatively small number of individuals.

TABLE I. (FROM GALTON).

STRENGTH OF PULL (519 MALES, AGED 23-26).

From Records made at the International Health Exhibition in 1884.

Strength of Pull.	Number of Cases observed.	Percentages.	
		Number of Cases observed.	Sums from beginning.
Under 50 lbs.	10	2	2
„ 60 „	42	8	10
„ 70 „	140	27	37
„ 80 „	168	33	70
„ 90 „	113	21	91
„ 100 „	22	4	95
Above 100 „	24	5	100
Total	519	100	

Finally, we may display in a somewhat more detailed fashion the result of a still larger number of measure-

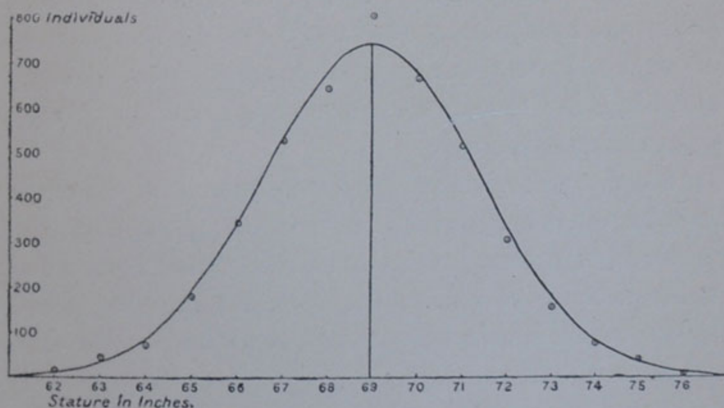


FIG. 5.

ments. Fig. 5 shows the variation in stature of a large number of members of Cambridge University of British

extraction, and exhibits in a concise form the result of 4,426 measurements recorded by the Cambridge Anthropometric Society. In this figure the stature in inches is indicated on the base line, whilst the perpendicular distances indicate the number of cases in which each particular height was recorded. The separate classes in this case include those who were found to fall within the limits of ½ inch on either side of each consecutive integral inch of stature, measurements which fell exactly half-way between two classes—*e.g.*, one of 69½ inches—being reckoned as a half to each of the classes in question. The continuous line in the diagram represents the form of the 'normal curve' which approximates most nearly to the line obtained by joining together the points actually plotted.

There seems to be good evidence that in such a case as that of human stature the figure obtained in this way will approximate more and more closely to the shape of what is known as a normal curve, according as the number of individuals measured and the accuracy of the measurements increase.

In order to arrive at a proper understanding of this fact, we must consider the derivation of the 'normal' curve from another point of view—namely, from the point of view of the mathematical theory of probability, which it will be our endeavour to present in as simple a manner as possible.

Let us consider the result of tossing up a number of similar coins simultaneously. If we toss up two coins only we may get any of the following results: (1) Head head, (2) head tail, (3) tail head, (4) tail tail.

And it is clear that any one of these combinations is equally likely to appear on any given occasion, if the coins are supposed to be strictly symmetrical, and are tossed up entirely at random. Now, the second and third results are the same unless the two coins are individually distinguishable. So we may write the most likely result of tossing up two pennies four times in the following way :

1 H H + 2 H T + 1 T T.

And in a similar way we may discover that the most likely result of tossing up three coins eight times is :

1 H H H + 3 H H T + 3 H T T + 1 T T T.

In the first case H T is twice as likely to appear as H H at any single throw, and in the second case H H T is three times as likely as H H H in any single toss.

It is possible to work out the most probable relative frequency of the various possible combinations in the case of any number of coins. Thus for ten coins the sequence of numbers runs :

TABLE II.

Heads.	Tails.	Relative Probability.
10	0	1
9	1	10
8	2	45
7	3	120
6	4	210
5	5	252
4	6	210
3	7	120
2	8	45
1	9	10
0	10	1

These values are plotted in the accompanying diagram (Fig. 6) as vertical distances above a base line. The figure obtained by joining together the points thus arrived at may be observed to show some resemblance to the previous Figs. 4 and 5.

The three series of numbers already given are those which are obtained on expanding the expressions $(1+1)^2$, $(1+1)^3$, $(1+1)^{10}$. In general the probabilities of the various possible combinations when n coins are tossed simultaneously are given by the expanded value of $(1+1)^n$.

Quetelet has worked out the relative probabilities of the most frequent combinations in the case of 999 coins simultaneously tossed—*i.e.*, the expanded value of $(1+1)^{999}$. A few of these values are given in the following table :

TABLE III.

Heads.	Tails.	Relative Probability.
500	499	1·0
501	498	0·996
502	497	0·988
503	496	0·976
504	495	0·961
505	494	0·942
510	489	0·803
520	479	0·432
530	469	0·155
540	459	0·037
550	449	0·006
560	439	0·0006

It may thus be seen that the likelihood that a result appearing in any given throw will show a still greater

difference in the relative number of heads and tails than 560 : 439 becomes very small indeed. Although

Tails 1 2 3 4 5 6 7 8 9 10

FIG. 6.

a throw of all heads or all tails is possible, the odds against such a result being ever actually seen are

460 470 480 490 500 510 520 530 540

FIG. 7.

almost inconceivably great. In Fig. 7 the middle values for $(1+1)^{999}$ are plotted—those combinations

being included which lie between 550 : 449 and 449 : 550. The points thus obtained are so close together that the eye can scarcely distinguish whether they are joined by straight or curved lines. We have, in fact, arrived at a close approximation to the normal curve.

The curve thus approximately indicated may be seen to be closely similar to the one shown in Fig. 5; in fact, the two curves are of such a kind that by altering the vertical and horizontal scales in one of the figures in a suitable ratio their form could be made practically identical.

The figure arrived at in this way approximates to a mathematical curve which is intelligible to the mathematician from the formula $y=e^{-x^2}$. The theoretical curve is really arrived at by supposing n in the expression $(1+1)^n$ to become indefinitely great. Practically, by making n very large we can get as near an approximation as we may wish to the normal curve of theory. Even in the case of relatively small values of n the approximation to the normal curve is fairly close, as may be seen by comparing together Figs. 6 and 7.

The example of tossing up coins was only taken as a means of illustrating the more general assumption of an event or a magnitude depending upon a number of causes of equal strength, which in the long-run act with equal frequency in two opposite directions. We can understand that human stature may afford a comparable case, when we consider the large number of bones and cartilages the lengths of which must be added together in order to make up the total stature of any individual, and that the separate length of each

one of these elements depends upon factors which we have no means of classifying exactly.

It now becomes necessary to mention one or two technical terms which are used in connection with the normal curve. The *mode* of such a curve is the longest perpendicular which can be drawn from the

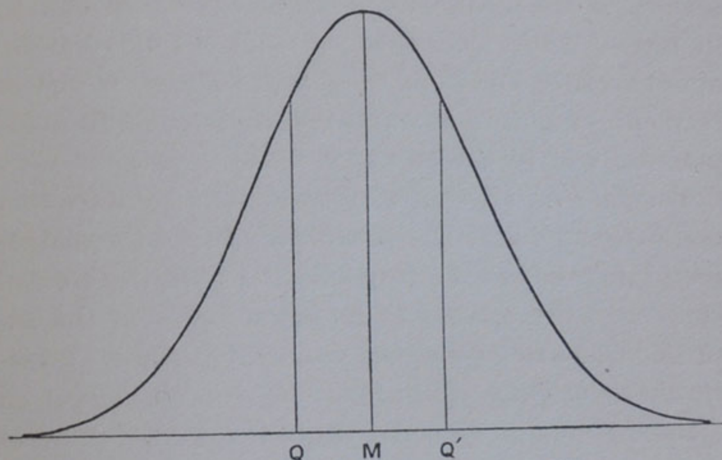


FIG. 8.—NORMAL CURVE.

base-line to meet the curve itself (M in the above figure). The curve is symmetrical on either side of the mode—that is to say, any two perpendiculars drawn from the base to the curve on either side of the mode and at the same distance from it will be equal in length.

When dealing with a symmetrical curve the position of the mode is identical with that of the *median*—the perpendicular line which divides the area of the curve into two equal halves, and the foot of this perpendicular also represents the *mean* or average of

all the values from which the curve is constructed. In any actual case obtained by practical methods the position of the mode, the median, and the mean will only be approximately the same, because such a curve is never perfectly symmetrical.

The same curve can always be reconstructed if the position and magnitude of the mode are known, and, in addition, any one other point on the curve itself. A convenient point to take for this purpose is the point at which the curve is met by a straight line erected perpendicular to the base at such a distance from the median that it divides the area enclosed by the median, the base, and half the curve into two equal parts. The distance of such a perpendicular from the median is known as the *quartile*. Any given curve will have two quartiles one on either side of the median; they are shown at Q and Q′ in Fig. 8.

In practice an approximation to the normal curve of variability is constructed by plotting the values of a number of separate measurements or other determinations made upon different individuals. A *variate* is one of the separate numerical values from which a curve of variability can be constructed; the biometrician usually deals with some such number as 1,000 variates. The total number of variates is represented by the area enclosed by the curve, and it will be seen that half the total number of variates falls between the two quartiles and half outside them.

A *class* (*cf*. p. 81) may be defined as a group of variates all of which show a particular value or a value

falling between certain limits. The *frequency* of a class is the number of variates which it contains.

The amount of variation shown by a particular group of variates is measured by the degree of slope of the curve. A flat curve indicates greater variability and a steep curve denotes less variability. The flatter the curve—supposing the area (the number of variates) to remain the same—the further from the mode will be the position of the quartile, so that the distance of the quartile from the mode may be taken as a convenient measure of variability. In a theoretically perfect curve the distance of Q and Q′ from M is equal. A curve obtained from an actual series of variates is never perfectly symmetrical, so that in practice the distance of Q and Q′ from M may not be quite the same. In such a case the average of the two distances is taken as the measure of the variability of the material in question, and this value may be briefly denoted by the letter q.

In the example of variability of stature represented by Fig. 5, q is equal to 1·6 inches. This amount of variability can therefore be compared with other values representing the variability in stature and in other characters shown by various other groups of individuals. This, then, is the first important biometrical result which we have arrived at—the determination of a numerical value representing the amount of normal variability in any given case.

A measure of variability more often used than the quartile, especially in recent work, is what is known as the *standard deviation* of a normal curve, and may

be expressed shortly as σ. σ represents a distance from the mode equal to $q \div 0{\cdot}6745$. Thus if σ is known, q can be readily determined, and *vice versa*. The reason for the more frequent use of σ is that it happens to be determinable with greater accuracy from an actual series of variates.*

The circumstance that half the total number of variates lies outside the limits of the quartiles and half within leads us to the consideration of what is known as the *probable error*. The probable error of any statistical determination is obtained by finding a pair of values lying one above and one below the true value required—*e.g.*, the average stature of the whole of a race—such that it is an even chance that the value actually found will lie between them. Or the same thing may be expressed in another way. If we plot in the form of a curve a long series of actual determinations of a particular value, the probable error of a single determination will be nearly equal to the quartile of the curve so obtained. We may illustrate this state of things from our example of tossing coins, or still better by the essentially similar case of drawing balls out of a bag which contains a very large number of balls—black and white in equal numbers. Here the value to be determined experimentally is

* σ is found by multiplying the square of the deviation of each class from the mean (or mode) by the frequency of the class, adding together the series of products so obtained, dividing this number by the total number of variates, extracting the square root of the result, and multiplying by the number of units in the class range (this last number is very often unity). For further details with regard to the properties of the normal curve Davenport's 'Statistical Methods' may be consulted.

the relative number of black balls to white, which we know as a matter of fact to be equality; and our single determination may consist in drawing out a hundred balls, which are afterwards returned to the bag. If we do this 1,000 times, and plot the number of black balls drawn each time, we shall arrive approximately at a curve having its mode at 50, and possessing a standard deviation which it is possible to determine from the instructions given in the footnote to p. 92. Multiplying σ by 0·6745 gives us the quartile, which represents the probable error of a single determination. That is to say, it is an even chance whether any single determination differs from 50 by more or less than q. In this particular example the quartiles would be found to lie very nearly at 46·6 and 53·4, so that the value of the probable error is 3·4.

The properties of the normal curve tell us a number of useful things about the probable error. In the first place its value varies inversely as the square root of the number of variates—that is to say, that in such a case as we have just described the probable error varies inversely as the square root of the number of balls drawn each time. We can realize this point more clearly when we remember that the linear dimensions of a curve vary with the square root of its area (the number of variates); the accuracy of our determination varies in fact with the quartile, which is the linear distance from the mode of a certain perpendicular.

We have seen that it is an even chance whether a single determination differs from the proper value by more or less than the amount of the probable error,

an amount which we may denote by the letter *e*. The chance that any particular determination differs from the true value by more than twice the probable error is 4·5 to 1 against.

The chance that it differs by more than	3*e* is	21 : 1 against.
" " " "	4*e* "	142 : 1 "
" " " "	5*e* "	1,310 : 1 "

This is clearly very valuable information to possess when we are dealing with any kind of statistics.

We must now pass on to consider what methods are available to the biometrician for dealing with the problems of heredity. His way is to take a large number of pairs of relations, each pair consisting, say, of a father and a son, and to find out how much more like the members of such a pair are to one another on the average than the members of similar pairs of individuals would be, if taken at random and without regard to relationships from among the general population to which these fathers and sons belonged.

Now we shall see later on that this is not the only way of looking at the phenomenon of heredity, nor is it the way which is most familiar to biologists. But it is important to remember that what the biometrician means by amount of inheritance is a numerical value which expresses the average degree of likeness between a particular pair of relatives—for example, fathers and sons.

In the accompanying 'correlation table'—a purely imaginary illustration—there are tabulated the statures of 4,503 fathers, and those of one son of each of them. Thus 14 fathers, each 62 inches high,

TABLE IV.

Heights of Sons (In inches).	Heights of Fathers (In inches). 62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	Total Fathers for Sons of each Height.	Modes of Arrays of Fathers.
62	—	—	2	5	4	2	1	—	—	—	—	—	—	—	—	14	65·5
63	1	2	4	11	15	10	5	2	1	2	—	—	—	—	—	53	66
64	2	4	6	12	16	20	10	6	3	1	—	—	—	—	—	80	66·5
65	3	10	12	17	31	42	32	14	12	10	3	1	—	—	—	187	67
66	4	16	21	33	49	58	61	45	31	18	15	2	2	—	—	355	67·5
67	2	9	18	42	57	85	102	83	54	40	17	10	2	—	—	521	68
68	1	3	10	30	55	101	127	132	103	51	28	11	4	1	—	657	68·5
69	1	3	6	17	45	84	135	179	127	89	46	16	7	2	1	758	69
70	—	1	3	12	36	59	105	130	128	103	57	37	9	4	1	685	69·5
71	—	1	1	10	18	41	56	82	99	85	55	39	17	12	3	519	70
72	—	2	1	4	12	18	31	48	56	57	45	35	19	15	3	346	70·5
73	—	—	—	1	2	10	10	15	28	45	30	18	9	10	4	182	71
74	—	—	—	—	2	2	4	6	6	19	18	12	6	4	2	81	71·5
75	—	—	—	—	—	—	1	2	4	9	16	10	4	3	1	50	72
76	—	—	—	—	—	—	—	—	1	2	4	4	2	1	1	15	72·5
Total sons for fathers of each height ...	14	51	84	194	342	532	680	744	653	531	334	195	81	52	16	4,503	69
Modes of arrays of sons	65·5	66	66·5	67	67·5	68	68·5	69	69·5	70	70·5	71	71·5	72	72·5	69	

are supposed to have had 14 sons, whose heights are given in the first column. The series of heights of sons corresponding to a particular class of fathers is known as an *array*. Thus each column of the table represents an array of sons, and similarly each line represents an

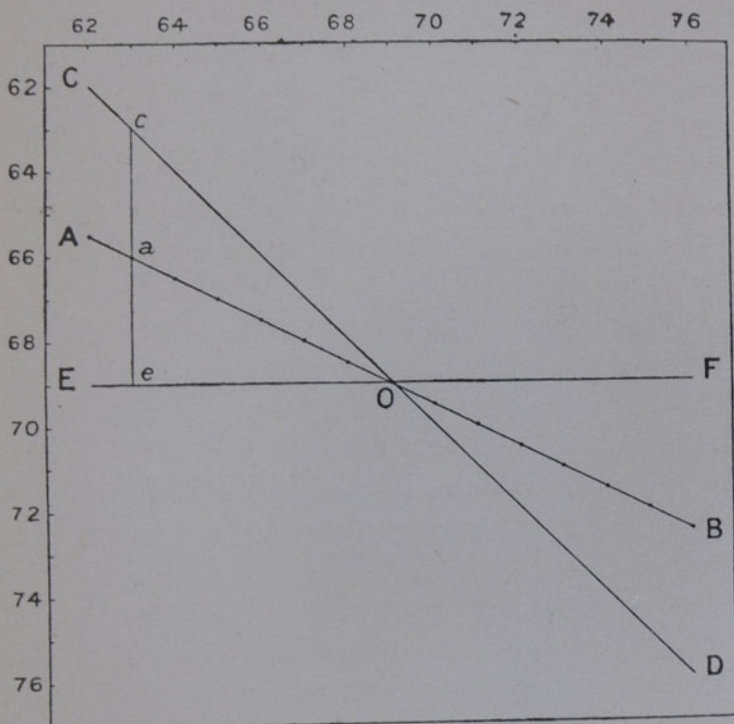


FIG. 9.—DIAGRAM OF CORRELATION.

array of fathers. The mode of each array of sons is given in the bottom line of the table.

Now if sons were on the average exactly the same height as their fathers, the modal value of each array of sons would be the same as the height of the corresponding class of fathers. If, on the other hand, there

were no correlation between the heights of sons and those of their fathers the mode of every array of sons would be the same, and this value would be identical with the mode of the heights of all the sons taken at once. The actual result is found to be intermediate between these two possible extremes. Thus we see that sons tend to be like their fathers in respect of stature, but not exactly like, and if the example given were a real one the fundamental fact of a positive resemblance or correlation between the statures of fathers and sons would at once be clearly established.

The way in which a numerical value is attached to this correlation can be shown graphically.

In the diagram opposite, the dots indicate the values of the modes of the several arrays of sons as read off on the vertical scale to the left of the figure, the heights of the corresponding classes of fathers being read off on the horizontal scale. It will be seen that this series of dots lies nearly in a straight line which is inclined at a certain angle to the horizontal.

Now if there were perfect correlation between the heights of fathers and sons, or if the modal value of each array of sons were identical with the height of the corresponding class of fathers, the inclination of the line obtained in the above manner would be one of 45 degrees, as in the case of the line CD which passes through the points at which the values as read off in the vertical and horizontal scales are identical. If, on the other hand, there were no correlation the line would be horizontal, as EF.

The value taken to represent the amount of correla-

tion is the degree of slope of the line AB. This is expressed mathematically as tan a, a being the angle which the line in question makes with the horizontal.

When there is positive correlation this angle falls between 0 and 45 degrees, and tan a between 0 and 1. In the present instance tan a is 0·5. This value is known as the coefficient of correlation, and affords the basis of a numerical comparison with other similar coefficients obtained for other characters besides stature, and in the case of other pairs of relatives besides fathers and sons.

It ought now to be clearly understood that a complete resemblance between each class of fathers of a particular stature and the average stature of the corresponding array of sons would be indicated by the close approximation of our plotted points to a straight line making an angle of 45 degrees with the base line—a line, that is to say, having a slope of 1 in 1, or unity; whilst the entire absence of correlation would be represented by a line having no slope—that is to say, a horizontal line. The actual result in the example given is represented by a line having a slope of nearly 1 in 2, or 0·5.*

* *Correlated Variability.*—A precisely similar method is employed to measure the correlation of two parts or organs of the same individual. For example, the lengths of the right and left arms of men are very closely correlated. In order to attach a numerical value to this correlation the lengths of the right arms of a number of men are treated in the same way as the statures of fathers in the example given, and the lengths of their left arms in the same way as the statures of sons. The proper correlation coefficient can then be found by plotting the result; or the labour of plotting may be obviated by a process of calculation.

In the following table there are set down the correlation coefficients for stature in the case of seven pairs of relations, as obtained from actual data of a similar character to that already given by way of illustration.

TABLE V. (FROM PEARSON).

CORRELATION COEFFICIENTS FOR HUMAN STATURE.

Father and son	0·514
Father and daughter	0·510
Mother and son	0·494
Mother and daughter... ...	0·507
Brother and brother	0·511
Sister and sister	0·537
Brother and sister	0·553

Of the above, the first four values, representing correlation between parents and children, are seriously affected by the fact that the statistics from which they are derived show the existence of a marked correlation between husbands and wives in point of stature, amounting, indeed, to as much as 0·28—the result of what is technically described as selective mating. In the absence of such a relation between the statures of the parents, the correlation between parent and child might be expected to be distinctly less than that between pairs of brothers or sisters.

The term correlation replaces to some extent the older term *regression* employed by Galton. When speaking of regression the facts already described are regarded from a slightly different point of view. It is sometimes found convenient to speak of the regression of the mean stature of an array of sons toward the mean of the general population, instead of speaking

of the correlation between the filial mean and the value of the parental class.

Regression represents the extent to which the average son is more like the mean of the general population than his father is. Correlation, on the other hand, indicates the amount by which the son is more like his parent than he is to the average of the general population. Thus, instead of being exactly like their parents, children are said to show regression towards the mean of the general population to which both parents and children belong.

In a case where the mean height of the fathers is identical with the mean height of the sons examined, and both are the same as the mean height of the general population, the coefficient of regression is simply equal to the reciprocal of the correlation coefficient between fathers and sons. In actual practice this condition is seldom realized, and it is then necessary to use a more elaborate method in order to determine the value of the regression coefficient.

Professor Pearson has extended the idea of correlation to the case of characters which are not capable of exact quantitative measurement. This extension is based upon the assumption that such characters follow a normal law of distribution in their variation, just in the same way as such a character as human stature was found to do. There is considerable doubt as to how far this assumption is justified, so that at the outset we may feel disposed to attach less importance to the actual values arrived at in this way than we should in

the case of characters which can be shown to vary normally. The method of calculation actually employed involves somewhat complicated mathematical processes, but on Professor Pearson's authority we may assume both the validity of the method and the accuracy of the results obtained—so far as the actual process of computation is concerned. For the purpose of making the necessary calculations the data were arranged in such a form as the following :

PARENTAL CORRELATION OF COAT COLOURS IN HORSES.

Fillies.	Sires.		Total.
Colour.	Bay and Darker.	Chestnut and Lighter.	
Bay and darker	631	125	756
Chestnut and lighter ...	147	147	294
Total	778	272	

By the suitable treatment of these figures the value 0·45 was obtained as representing the coefficient of correlation between sire and filly.

The amount of reliance which is to be placed in the above method of determining the value of a correlation coefficient was tested by arranging in a similar manner data with regard to stature which had already been treated in the form of a complete correlation table. The whole number of fathers was divided into two groups containing the individuals above and below a certain stature, and the same was done in the case

of the sons. And the separation into two groups was made in several different ways by taking the dividing line between the groups at various heights. By applying to the statistics disposed in these various arrangements the same method as was applied to the statistics of horse colour already referred to, values varying between 0·52 and 0·6 were obtained for parental correlation; whereas the value arrived at by the more usual and reliable method was 0·514. It would therefore appear that there is with this method a tendency to obtain too high a figure, as compared with that derived from the method of the complete correlation table. When this source of inaccuracy is taken into consideration, in combination with the doubtfulness of the assumption upon which the method is based, it seems clear that its use will only give us a roughly approximate view of the correlation actually existing in the cases to which it is applied. Having made this reservation, we may compare the values given in the following table with those which appeared in Table V.:

TABLE VI. (FROM PEARSON).

AVERAGE PARENTAL CORRELATION.

Human eye colour	0·495
Horse, coat colour	0·522
Basset hound, coat colour	0·524
Greyhound, coat colour	0·507

AVERAGE FRATERNAL CORRELATION.

Human eye colour	0·475
Horse, coat colour	0·633
Basset hound, coat colour	0·524
Greyhound, red in coat	0·700
Greyhound, black in coat	0·660

Thus if we use the term inheritance at present simply to express the fact that a more or less definite numerical value can be attached to the average amount of resemblance between any specified pair of relatives, we see that a considerable number of physical characters appear to be inherited at approximately the same rate in men and in animals.

More than this, Professor Pearson has shown, by the use of the same method as was applied to the case of physical characters not quantitatively measurable, that the average resemblance in *mental* characteristics between pairs of brothers, pairs of sisters, and pairs made up of a brother and a sister, can be expressed by the values given in the following table :

TABLE VII. (FROM PEARSON).

Character.	Brothers.	Sisters.	Brother and Sister.
Vivacity	0·47	0·43	0·49
Assertiveness ...	0·53	0·44	0·52
Introspection ...	0·59	0·47	0·63
Popularity	0·50	0·47	0·49
Conscientiousness ...	0·59	0·64	0·63
Temper	0·51	0·49	0·51
Ability	0·46	0·47	0·44
Handwriting	0·53	0·56	0·48
Mean	0·52	0·51	0·52

A sample of the collected facts from which this information is arrived at is given in the table on p. 104.

CONSCIENTIOUSNESS: BROTHER—BROTHER.

Second Brother.	First Brother.		Total.
	Keen.	Dull.	
Keen	970	216·5	1,186·5
Dull	216·5	287	503·5
Total	1,186·5	503·5	1,690

Every child was classified in this way as being either above or below an average standard in respect of each character. The estimations were made by teachers having at least six months' experience of the children in question.

The method of statistical treatment was, as we have said, the same as that employed in the case of physical characters not capable of quantitative measurement, and there is little doubt that it is equally valid in the present case. We may well feel, however, some hesitation in accepting as sound the data to which the method is applied. At the best this data can only be of a roughly approximate kind. The evidence is, however, undoubtedly sufficient to establish the conclusion that mental characters are inherited in man, and that they are probably inherited at a rate not greatly different from that at which physical characters are inherited. For it will be observed that the values given in Table VII. are in close agreement with one another, and that they also agree with the average value of fraternal correlation as found for a variety of

physical characters both in men and in other animals. Assuming—and the assumption seems to be a reasonable one—that equal fraternal correlations indicate the existence of equal correlations between parents and children, we arrive at the conclusion that the resemblance between parents and their offspring is of much the same kind and amount in the case of mental as it is in the case of bodily characteristics.

What we may perhaps describe as the main generalization so far arrived at by biometricians is known as the Law of Ancestral Heredity. This hypothesis supposes, or at least in its original form supposed, that every ancestor of a particular individual contributes its quota to the heritable qualities displayed by that individual. The law also states that the average amount of resemblance between an individual and any particular ancestor is capable of definite numerical expression. Thus the mean amount of correlation between (1) the two parents and the offspring, (2) the four grandparents and the offspring, (3) the eight great-grandparents and the offspring, and so on, is believed to diminish in a geometrical series, which is the same for all organisms and for all characters. The actual amounts of these correlations were expressed by Galton in the form of the series 0·50, 0·25, 0·125, etc. Pearson regards them as being more nearly represented by the more rapidly diminishing series 0·6244, 0·1988, 0·0630, etc.

Now, there can be no doubt that the law as stated above has been disproved in specific instances, and was indeed disproved by the work of Gregor Mendel before

ever it was enunciated, although Mendel's work was not generally known until later. According to Mendel's theory of inheritance, certain ancestors contribute *nothing* to the constitution of certain offspring in respect of certain characters. Furthermore, the modification of the law of ancestral heredity which applied to alternative inheritance, and which was assumed in working out the inheritance of coat colour in thoroughbred horses, has recently been shown not to apply to that particular case.

Unfortunately, most of the further biometrical generalizations are based upon the assumption that the law of ancestral heredity is strictly true. So that whilst we have spent some time in considering the facts of normal variability and of correlation between relatives, because these facts are quite independent of any theoretical assumption, the remainder of our review must be passed over at a more rapid rate. Until the theoretical conclusions now to be described have been revised by their authors in the light of recent knowledge, it is difficult to say how much reliance is to be laid upon them, but it seems quite likely that they will hold good as approximations. Indeed, though not applying to individual cases, the law of ancestral heredity does seem to hold good as a statistical statement of general results, so that there would be no objection to it on either theoretical or practical grounds if only it had been enunciated in some such terms as 'a law of average ancestral resemblance.' Thus it is quite possible that the total contribution of the eight great-grandparents of an individual may be on the average

correctly represented by Pearson's fraction, even though their individual contributions are not always the same.

Let us, then, briefly examine some of the further conclusions which have been drawn from the data of the biometricians.

Assuming the law of ancestral heredity, Pearson has arrived at very interesting conclusions with regard to the effects of artificial selection when the correlation coefficients have those values which have been actually found for them in the case of the human race. In the statement which follows, ancestors are supposed to have been selected showing in each generation a deviation h from the general mean of the population. Thus, suppose the character selected to be stature: suppose the mean height of the population to be 6 feet, and the selected individuals to be 6 feet 6 inches high; h is then 6 inches, and only individuals of a height of 6 feet 6 inches would be selected as parents in each generation, so that after three generations of selection we should be dealing with children whose parents, grandparents, and great-grandparents were all of this particular height.

Pearson calculates that after one generation of selection the immediate offspring will show 0·62 of the character selected (0·62 h). After two generations they will show 0·82 h, after three 0·89 h, and after a great number of generations 0·92 h. Thus in a comparatively small number of generations the development of a character may be raised to within 90 per cent. of the value selected, but, after this, further selection has

very little effect. If selection is stopped after one generation, and the selected stock is then inbred, it was calculated that the first generation of inbred stock would show 0·59 *h*, the second 0·56 *h*, the third 0·52 *h*, and the tenth 0·35 *h*. If, on the other hand, inbreeding was started after the selection had continued for a large number of generations, the first generation of inbred stock will show 0·86 *h*, the second 0·81 *h*, the third 0·77 *h*, and the tenth 0·51 *h*. So that inbreeding of a selected stock is followed by a very gradual return towards the mean character of the original race.*

It must be remembered that in the calculation which led to this result perfect normal variability was assumed, and the contribution of every ancestor of the same degree to the hereditary endowment of the offspring was supposed to be exactly equal. Since both these assumptions are very unlikely to be realized in any actual case, the statement here given must only be regarded as an approximate indication of what is likely to take place.

Some remarkable observations have recently been published by Professor Johannsen, of Copenhagen, and from them are drawn conclusions which seem likely to lead to a distinct advance in our understanding of the process of so-called continuous variation, and of the way in which such variations are transmitted.

* From this it seems necessarily to follow that it is impossible to establish a permanent breed simply by a process of selection.

Johannsen's experiments have so far only been published in the form of a preliminary abstract, but his conclusions are of such interest that it seems necessary to draw attention to them. The proviso must be made, however, that further evidence is necessary in order to justify their complete acceptance.

The experiments in question were made upon plants which could be self-fertilized for a series of generations. In this way many complications were avoided which are inevitably introduced in the case of biparental inheritance. Barley and kidney beans were among the plants examined, and the simplest character considered was the size of the seeds of the latter as measured by weighing. In this particular series of experiments each plant was regarded as being characterized by the average weight of the seeds which it produced.

All the descendants arising from a single plant by self-fertilization are spoken of by Johannsen as making up a 'pure line.' And the members of such a line showed, in respect of the weight of their seeds, normal variability about a mean or type value. The general population of bean plants, made up of a great number of such pure lines, also exhibited a normal curve when the weights of the seeds were plotted. The pure lines composing such a population showed various types, some of them close to the modal value of the population, but others differing widely from it. If now a somewhat widely deviating member of a particular line was selected for propagation, its offspring showed regression to the type of this par-

ticular line, and not to the mean value of the general population.

The case is indeed precisely similar to the supposed example of a mixture of races of peas, which was made use of as an illustration at the beginning of the present chapter. In other words, a pure line consists of a group of individuals which has a normal variability of its own, and the offspring of which by self-fertilization breed true to the type of their own particular group, and show no regression towards the type of the general population to which the group belongs.

If we were to carry on this conception to the case of bisexual inheritance, we should find that the different pure lines would become crossed and confused together in a way which would be very difficult to disentangle. There is no reason to doubt that statistical treatment of such a population would yield similar results to those actually obtained by biometricians from the data at their disposal; and we may notice that a fortuitous mixture of a considerable number of pure lines, having slightly different types, would admirably fulfil the conditions we have seen to be necessary in the case of material, to which methods based upon the theory of chance are to be applied. The phenomena which follow upon the crossing together of two or more pure lines still remain to be worked out, but it is not unlikely that they will be found to conform to those laws of heredity associated with the name of Mendel which are explained in Chapter VII. If this should be found to be the case, it is not impossible that the theory of pure lines, in combination

with the method of inheritance referred to, may adequately serve to describe those phenomena to account for which the law of ancestral inheritance was called into existence.

The conclusions to which Professor Johannsen's experiments lead him may be summed up as follows: Individuals which differ (in size, for example) from the mean of a population give rise to offspring which differ from that mean value in the same direction but to a smaller extent. Selection, therefore, will produce a change in the average character of a population taken as a whole. Selection *within a pure line* produces no effect of this kind. The average character of the offspring of typical members of the line is the same as that of the offspring of members which show the widest deviations from the type.

Selection *in a population* consists in the partial separation of those lines the types of which differ in the required direction from the average character of the population. This effect must of necessity come to an end when the most eccentric line is completely isolated. The great complications introduced when the lines are intermingled through mixed breeding may make this process of isolation a very tedious one.

It will be seen that the values calculated by Pearson to represent the result of selection in a population agree quite well with Johannsen's explanation of the constitution of such a population out of a number of pure lines. The result of Professor Johannsen's further experiments will therefore be awaited with great interest by biologists and biometricians alike.

On the theory of pure lines it is to be noticed that the personal character of a particular ancestor has no influence upon his descendants; it is only the type of the line to which he belongs which influences the offspring, so that this theory is in perfect agreement with Weismann's theory of inheritance as described on p. 68.

It is also to be observed that the principle of the pure line applies only to quantitative characters—such characters of size, or of weight, or of proportion, as are very seldom made use of by systematists for the distinction of natural species.

Hugo de Vries.

[To face p. 113.

CHAPTER V

THE THEORY OF MUTATION

MUTATION is the term applied by de Vries to express the process of origination of a new species, or of a new specific character, when this takes place by the discontinuous method at a single step—a process which he regards as the most important if not the sole method by which new species or specific characters arise. We shall see that although de Vries has recently done much to forward the propagation of this idea, the belief that such a discontinuous process is the normal method by which new species come into existence has been developing for a considerable time.

We have seen that those who accept the idea of evolution by the action of natural selection upon a series of minute and almost imperceptible variations are confronted with the difficulty of explaining how by this method there could arise a number of different structures or parts so co-ordinated as to share in a common function. Moreover, a closer examination of the actual processes of variation and inheritance render it doubtful whether the selection of continuous variations of even a simple characteristic can ever lead to the development of a permanent new race. The

result of Pearson's calculations, described in the preceding chapter, seems to indicate that the selection of a certain value of a particular character for many generations will never lead to the formation of a race in which the mean value of the character is as high as the selected value. But, says the selectionist, it will happen in Nature that as the standard of the race is raised by selection, the value selected will be still further raised, and so on, and in this way an indefinite amount of improvement is rendered possible. If Johannsen's conclusions are well founded, this is clearly not the case; on the contrary, there is a perfectly definite limit to the effect which selection can produce.

The question whether or not a gradual method of evolution is possible has not yet been absolutely decided for any single species or character, but it certainly seems that now for the first time the possibility of a definite decision is within sight. At the same time it is impossible to prove a universal negative. If we look at the other side of the problem we shall find that the evidence in favour of an alternative process has multiplied even faster than the evidence against the continuous accumulation of minute differences; and the present tendency is certainly to look for other sources of specific distinctness than that which is offered by the natural selection of continuous variations.

Even before the new evidence which we have briefly outlined was available, Herbert Spencer found the difficulties in the way of accepting the purely

Darwinian explanation to be so great, that he adopted the hypothesis of the inheritance of acquired characters, as being the only adequate explanation of the phenomena which was in his time available.

Unfortunately, satisfactory evidence that such a form of inheritance ever actually takes place has never been forthcoming in sufficient amount to lead to universal conviction. Indeed, at the present day the consensus of opinion among experts is undoubtedly to the effect that acquired characters are not inherited at all, except in so far as better nutrition of the parent may lead to the production of more vigorous offspring. And it seems clear that such an effect as the latter cannot go on accumulating for more than a few generations.

Thus we see that in the purely Darwinian view there is something wanting, whilst the Lamarckian explanation is ruled out of court for the present for lack of direct evidence. If, at this point, we find that in Nature a co-ordinated set of structures can and does arise in an already perfected condition at a single step, and that such phenomena take place with sufficient frequency to give ample opportunities for the survival of the new type so arising, we have at once discovered an alternative way out of the difficulty. Such a discovery must throw abundant light on the obscurity overshadowing the methods by which evolution has taken place, even though we may not yet have arrived at any kind of explanation of the cause of this phenomenon of co-ordinated and definite variability.

The actual observation of variations of this kind is

of quite recent date, and their recognition is largely due to the exertions of Bateson. But the idea that this is the way in which evolution takes place is very ancient, as a few quotations will clearly demonstrate.

The idea that definite structures may arise, each as a whole and in a perfect condition, was clearly propounded by Aristotle in a passage which it is a little curious to find quoted at the beginning of the 'Origin of Species.' Darwin's note is to the following effect: After remarking that rain does not fall in order to make the corn grow, any more than it falls to spoil the farmer's corn when threshed out of doors, Aristotle adds, 'So what hinders the different parts of the body from having this merely accidental relation in nature? as the teeth, for example, grow by necessity the front ones sharp, adapted for dividing, and the grinders flat, and serviceable for masticating the food; since they were not made for the sake of this, but it was the result of accident. And in like manner as to the other parts in which there appears to exist an adaptation to an end. Wheresoever, therefore, all things together—that is, all the parts of one whole—happened like as if they were made for the sake of something, these were preserved, having been appropriately constituted by an internal spontaneity; and whatsoever things were not thus constituted, perished and still perish.'

Upon the above passage from Aristotle Darwin comments as follows: 'We here see the principle of natural selection shadowed forth, but how little Aristotle fully comprehended the principle is shown by his remarks on the formation of the teeth.' We

may state at once that these very remarks upon the formation of the teeth almost exactly embody the views of the modern mutationist.

Perhaps the earliest use of the actual word 'mutation' in this sense is to be found in 'Pseudodoxia Epidemica,' by Dr. Thomas Browne. I quote from Book VI., chapter x., 'Of the Blackness of Negroes'* (second edition, 1650): 'We may say that men became black in the same manner that some Foxes, Squirrels, Lions, first turned of this complection, whereof there are a constant sort in diverse Countries; that some Chaughes came to have red legges and bills, that Crows became pyed; All which *mutations*, however they began, depend upon durable foundations, and such as may continue for ever.'

The experiments upon cross-breeding, which are described in a later chapter, will be found fully to bear out the idea that 'mutations,' or definite characteristics which have arisen in a definite way, do depend upon durable foundations.

The late Professor Huxley's emphatic approval of the 'Origin of Species,' as signalized in his reviews of the first edition of that work, was tempered by the following mild criticism: 'Mr. Darwin's position might, we think, have been even stronger than it is if he had not embarrassed himself with the aphorism "Natura non facit saltum," which turns up so often in his pages. We believe . . . that Nature does make jumps now and then, and a recognition of the fact is

* I am indebted to my friend Mr. R. C. Punnett for this reference.

of no small importance in disposing of many minor objections to the doctrine of transmutation.'*

The first person to formulate a more or less precise view upon the subject of definite variation was Francis Galton, although this author never entered into the question at any great length. Galton's attitude towards the problem in its early stages may be gathered from the following quotation from his 'Natural Inheritance': 'The theory of natural selection might dispense with a restriction for which it is difficult to see either the need or the justification—namely, that the course of evolution always proceeds by steps that are severally minute, and that become effective only through accumulation. That the steps may be small, and that they must be small, are very different views; it is only to the latter that I object, and only when the indefinite word "small" is used in the sense of "barely discernible," or as small compared with such large sports as are known to have been the origins of new races.'†

But more than this, the idea of the existence of stable forms, such as may be supposed to have arisen by large and sudden variations, is very well expressed by Galton in his division of varieties into the three groups of primary types, subordinate types, and mere deviations from the latter. A most luminous analogy is afforded by the three types of public vehicles which at the end of the nineteenth century were characteristic of the streets of London; and it is impossible to resist quoting Galton's account of them. These three

* 'Collected Essays,' vol. ii., p. 77.

† 'Natural Inheritance,' p. 32.

kinds of carriages, 'namely, omnibuses, hansoms, and four-wheelers, are specific and excellent illustrations of what I wish to express by mechanical types as distinguished from subtypes. Attempted improvements in each of them are yearly seen, but none have as yet superseded the old familiar patterns, which cannot, as it thus far appears, be changed with advantage, taking the circumstances of London as they are. Yet there have been numerous subsidiary and patented contrivances, each a distinct step in the improvement of one or other of the three primary

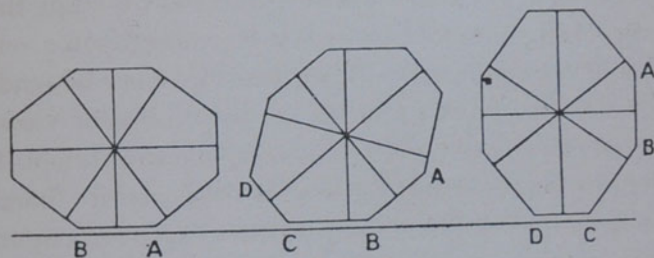


FIG. 10.

types, and there are or may be an indefinite number of varieties in details, too unimportant to be subjects of patent rights.'*

More recently Galton might have pointed out the introduction of motor traffic as illustrating a distinct mutation.

The distinction between primary and subordinate positions of stability is further excellently illustrated by the model which is here represented, and which is known as Galton's polygon (Fig. 10).

* 'Natural Inheritance,' p. 26.

The first position of the model, resting upon the side A B, may be taken to represent the condition of a type or stable form. A comparatively small push (variation) will lead to the production of the subtype illustrated by the position B C. When in this new position, it is easier to cause the model to return to its original position A B than it is to make it pass on to the new and more modified position resting upon the side C D. A strong push (mutation) may force the model to pass through the position C D until it comes to rest on the side opposite to A B. This fresh position represents a new stable form, and it is now once more surrounded by positions of subordinate stability—subtypes.

One more analogy before we pass on to consider the more recent observations upon discontinuous variations or mutations. We may compare the difference which exists between deviations and stable forms, arising by fluctuating and by definite variation respectively, with the behaviour of the atoms of chemistry, as expressed in the account of their structure recently given by Professor J. J. Thomson. Such an atom is regarded as being made up of a number of electrons or corpuscles bearing definite relations to one another in space. In certain circumstances it seems that it may be possible to remove a series of these corpuscles from the atom one at a time, in which case every such successive removal would be accompanied by a comparatively gradual and progressive change in the properties of the atom so modified. But after a certain time a point would be reached at which the removal of one more electron would necessitate a

complete rearrangement of the remaining corpuscles in order to arrive at a new position of equilibrium, and this change would be accompanied by a marked alteration in the chemical properties of the atom itself. In like manner the chemical composition of the living substance of a race of organisms may be conceived to alter step by step, every such step being accompanied by comparatively unimportant changes in its visible characters, until the time arrives when any further alteration must be associated with a deep-seated revolution in the constitution of the living substance, and with a corresponding marked mutation in the external features of the members of the race.

The first really definite attempt to collect and co-ordinate the facts of discontinuous variation was made by Bateson in his book entitled 'Materials for the Study of Variation,' published in 1894. The introduction and concluding remarks at least of this volume ought to be read by everyone who is interested in these subjects. The bulk of the book contains a mass of material of great value to specialists.

After pointing out the difficulties which prevent his acceptance of the orthodox belief in the origin of discontinuous and apparently adaptative types of animals and plants through the action of natural selection on minute variations, difficulties to which we have already paid some attention, Bateson records his conviction that the facts of discontinuous variation afford a way out of the difficulty. He shows (1) that differences of the kind which are generally used to distinguish separate species may arise as single variations;

(2) that such a form of variation is by no means so uncommon a phenomenon as was formerly supposed; and (3) that variations of this kind may occur in every description of organ and part in a number of different plants and animals. The facts with which the main bulk of the book is concerned have reference to the animal kingdom.

We shall find it profitable to consider the views expressed in this book a little more closely, though it would occupy too much space to give even a brief summary of the facts upon which they are based, and for which reference must be made to the original.

In the first place Bateson calls attention to the phenomenon of *symmetry* as being a characteristic feature common to almost all organisms. This symmetry may manifest itself in a number of different ways. In *bilateral* and *radial* symmetry the parts symmetrically disposed are related to one another in the same kind of way as are an object and its image reflected in a plane mirror. Such symmetry, as, indeed, every kind of symmetry, is usually associated with a repetition of parts. In the present instances the parts are repeated in pairs, as with the two eyes in the human face; or in a radial series, like the arms of a star-fish, or the petals of a buttercup. To this phenomenon of the repetition of parts, generally occurring in such a way as to produce a symmetry or pattern, the term *merism* is applied.

Symmetry may affect the proportions and shape of the body of an animal or plant as a whole, or, on the other hand, separate parts or organs may show a

separate symmetry of their own. For the phenomena thus distinguishable separate terms are proposed. A *major symmetry* is a form of pattern which includes the body as a whole, as in the case of most animals where the two sides of the body closely resemble one another. A *minor symmetry* is a pattern completed in a separate organ or part—for instance, in the flower of a plant or the limb of an animal.

Once more we may lay stress upon the universal existence of pattern among living things. Bateson points out that in collecting any kind of living creature it is the symmetry of it which, as a general rule, first catches the eye and distinguishes the organized body from surrounding inanimate objects.

The phenomenon of merism or repetition of parts being understood, we are in a position to consider the subdivision of variations into *meristic variations* and *substantive variations* respectively.

Meristic variations are variations in symmetry and in the number of repeated parts. A change in the number of organs in a series may conceivably take place gradually by the addition or subtraction of successive fractions of a part. But, as a matter of fact, this is very seldom the case. The increase or decrease usually involves one whole member at a time and sometimes more, so that this kind of variation is, as a rule, discontinuous. Abundant illustrations of this fact are to be found in the case of changes in the number of such parts as the teeth or vertebræ of mammals ; and a particularly good instance is afforded by the variations which take place in the number of ray florets in various

composite plants—*e.g.*, the daisy and chrysanthemum. It is suggested that meristic variations are connected with definite changes in the mechanical relations of dividing parts, and that it is in the mechanics of cell-division that the explanation of their discontinuous appearance is to be sought for.

Thus when, for example, a tulip-flower appears having its parts perfectly developed in sets of four instead of in sets of three, it is suggested that the arrangement in fours, like the arrangement in threes, fulfils certain conditions of equilibrium among the forces which affect the cell-divisions in the rudiment of the flower, and that these conditions of stability would not be equally well provided for by any intermediate arrangement.

Substantive variations are changes in the actual constitution or substance of the parts themselves. For example, a plant with coloured flowers may give rise to offspring the flowers of which are white. There seems to be no mechanical necessity for such variations to be discontinuous rather than continuous; it is quite possible to imagine a gradual dilution of colour taking place throughout a long series of generations. Discontinuous substantive variations are, however, not infrequent, and in such cases it is suggested that they may be associated with definite changes in chemical composition. Thus, for example, definite alterations in the colour of offspring as compared with their parents seem almost necessarily to be of this nature.

The further evidence contained in the book we are considering refers entirely to meristic variation.

An important point with regard to repeated parts is to be observed in the fact that in a pair of allied species, in which a series of repeated organs in the one is clearly comparable with a similar series in the other, all the parts in one form may differ from those in the second by the same kind of distinction, whether this be qualitative or numerical. The facts suggest strongly that such cases are to be accounted for by all the parts in question in one or both species having varied in a similar way at the same time rather than in succession. The occurrence of such a similar and simultaneous process of variation of repeated parts clearly simplifies in a marked degree the process of evolution, and greatly reduces the time which would be required for this process, if similar changes in repeated parts always took place successively. If we take an extreme case the latter supposition becomes absurd. In the albino or pure white types which occur as variations in many species of birds and mammals it is obvious that every hair or feather has taken on the white colour at the same time and for the same reason, whatever that reason may have been. Hairs or feathers are very good examples of repeated parts of the kind of which we have been speaking. It appears, too, that colour patterns may originate and change in a similar manner. In the case of such a bird as the peacock we should expect on this view that the pattern varied in all the tail feathers simultaneously, nor is it necessary to suppose that even this process took place by a very long series of minute steps. If we find that the splendid coloration of the peacock's tail arose

by a few marked variations, each of which occurred simultaneously in all the feathers at once, several serious difficulties are avoided, and on the analogy of similar known cases we have every reason to believe that this was so. And similar changes may take place in cases where the pattern depends on the coloration of a group of feathers or hairs. Indeed, if we consider, we shall find it very difficult to picture such a process as taking place in any other way. We can scarcely suppose the spots of the leopard, for instance, to have arisen one at a time.

An important kind of discontinuous variation is that to which Bateson has applied the term *homœosis.* The same sort of change had previously been described by Masters in the case of plants under the name 'metamorphy,' but the latter expression has also been employed in other senses. Homœosis consists in the assumption by one member of a meristic series of the form or character proper to another member of the same series; for example, the modification of the petal of a flower into a stamen, or of the eye of a crab into an antenna-like organ.

'In these cases a limb, a floral segment, or some other member, though itself a group of miscellaneous tissues, may suddenly appear in the likeness of some other member of the series, assuming at one step the condition to which the member copied attained presumably by a long course of evolution.' *

The phenomenon of homœosis is frequently to be seen among the parts of flowers. Double flowers in

* 'Materials for the Study of Variation,' p. 570.

many cases—for instance, in the case of the rose—arise by the development of petal-like organs in the position which would properly be occupied by stamens. A parallel process is to be seen in the heads of composite flowers, such as the chrysanthemum. In a double chrysanthemum the florets of the disc develop in the likeness of ray florets. Both these cases would be classed as examples of *outward homœosis*, because the parts concerned resemble organs normally developed in a whorl exterior to themselves. A case of *inward homœosis*, on the other hand, is afforded by the appearance of a petaloid calyx—for example, in a tobacco-plant—the outermost whorl of the flower taking on the appearance of a whorl internal to itself.

In cases such as these we observe once more the occurrence of a marked and definite change, which, though at first sight quite distinct from the method of similar and simultaneous variation, yet bears a certain resemblance to that process in the fact that the direction in which a particular part varies is not wholly unrelated to the behaviour of other parts of the same organism. The process thus briefly described seems likely to have had considerable importance in evolution, notably in the origin of differences in the numerical relations of the bones in various parts of the spinal column in different vertebrate animals.

The preceding account of the conclusions drawn from Bateson's laborious study of variation has involved a good deal of technicality, but this is, unfortunately, unavoidable. The point chiefly to be emphasized is the frequent occurrence in Nature of

variations of a definite or discontinuous type—the fact that differences of the kind which are constantly used to distinguish natural species can and do arise in Nature at a single step, so that it is not necessary for such differences to be built up gradually by the action of natural selection.

De Vries, in his 'Mutations Theorie,' goes further than this, and attacks the position held by those who accept the doctrine that natural selection of individual differences can ever lead to definite and permanent, or specific, distinctions. Indeed, one of the chief contributions of this author to the species controversy is to point out that the belief that artificial selection acts in this way upon domestic plants is based upon a misapprehension. De Vries himself has carried out a number of experiments in selection, and he comes to the conclusion that selection of ordinary individual differences has no permanent effect at all.* The actual effect of this kind of selection is well illustrated by the results of the processes employed in the sugar-beet industry, in which elaborate care is taken to select those roots which contain the highest percentage of sugar for the purpose of propagation. This process was followed at first by a rapid improvement, but the rate at which the percentage of sugar increased soon fell off, until at the present day all that selection can effect is to keep up the standard of excellence already attained. Moreover, that this process of improvement

* Compare, however, Johanssen's more recent conclusions see p. 111).

was a very gradual one is to be accounted for in part, at least, from the fact that the methods of selection themselves gradually improved from year to year. There is no reason to doubt that a thoroughly efficient method of selection would have worked its full effect in a few generations. A similar state of things is said to be the case with the cereals, such as wheat and barley, which have been selected largely for the size of the grains. From his own experiments, de Vries has come to the conclusion that, when selection is really efficient, the full possible effect of this process is exhausted in quite a small number of generations, and that then the only further effect of selection is to keep up the standard already arrived at.

We have seen that the theoretical conclusions of the biometricians are in agreement with the opinions here expressed, so long as selection is understood to be confined to the choosing out of parents which show a definite standard value of the character under consideration, this value being the same in each generation. Under these circumstances, Professor Pearson concludes that in the first two or three generations a marked advance in the desired direction will take place, but that further selection (in this sense) will have comparatively little effect. But the believer in continuous evolution maintains in addition that selection will be followed to an indefinite extent by further variations in the direction of selection, since otherwise selection could never lead to important changes in organization. In the face of the strong contrary evidence, and of the fact that alternative

methods of evolution are now known to be available, the burden of proof of this proposition seems to lie with those who maintain the all-important influence of continuous variation and selection. At present we are free to reply in the words of Malthus, who long ago protested against the extravagant powers which were ascribed to the selection of small differences.

'I have been told,' Malthus writes, 'that it is a maxim among some of the improvers of cattle that you may breed to any degree of nicety you please, and they found this maxim upon another, which is, that some of the offspring will possess the desirable qualities of the parents in a greater degree. In the famous Leicestershire breed of sheep, the object is to procure them with small heads and small legs. Proceeding upon these breeding maxims, it is evident that we might go on until the heads and legs were evanescent quantities; but this is so palpable an absurdity that we may be quite sure the premises are not just, and that there really is a limit, though we cannot see it or say exactly where it is.' *

The only recorded example I am aware of in the case of animals, which shows the result of long-continued selection acting upon a quantitative character, is afforded by the case of the American trotting-horse. In this case it appears highly probable that we are dealing with a character which varies in a strictly continuous fashion. In his book upon 'The Trotting and Pacing Horse in America,' Hamilton Busbey gives a table from which the diagram on the opposite page

* 'Essay on Population,' 6th ed., vol. ii., p. 11.

is constructed. The entries in this table show the fastest times recorded for the feat of trotting a measured mile in various years beginning with 1818. The vertical scale contains the times, which vary from three minutes down to one minute fifty-six seconds, and the horizontal scale shows the year in which the record was

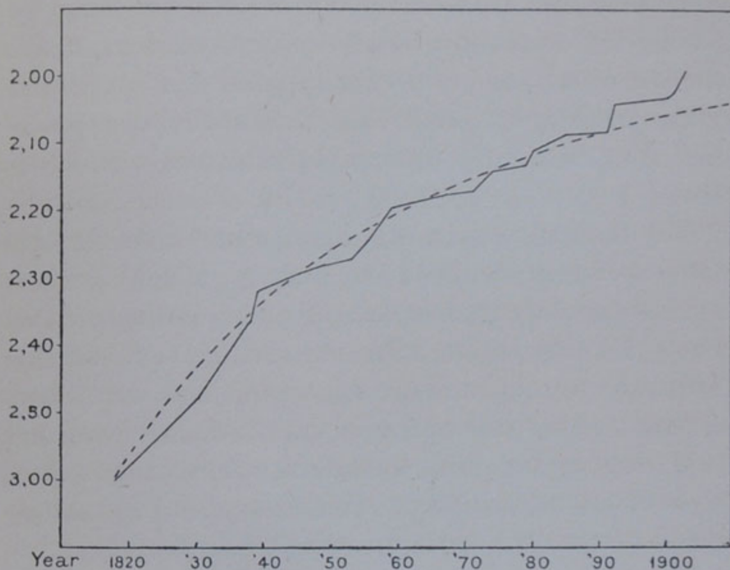


FIG. 11.

The figures to the left of the diagram are to be read as minutes and seconds.

made. Some part of the improvement shown is clearly to be associated with better tracks, improved methods of training, etc., but these will scarcely affect the general character of the improvement due to selection. As may readily be seen from the diagram, the improvement is at first rapid, but afterwards becomes

9—2

gradually slower and slower. At the end of the series two sudden steps upward break the general regularity of the series of records. But on examination of the evidence it is found that these are associated with special conditions, and are not really exceptional. The first of these breaks—that which occurs in 1892—is coincident with the introduction of a new type of sulky, having ball bearings and other improvements; whilst the record of 1903 was accomplished behind a pace-maker carrying a wind-shield. Neither of these records, therefore, is strictly comparable with the rest of the series.

The observations in this case do not, indeed, seem to be sufficient to afford the basis for a final decision against the theory of the indeterminate power of selection. Yet Malthus' criticism clearly applies very definitely to such a case—*i.e.*, there must be a limit beyond which the speed of the trotting-horse will never improve without a fundamental change taking place in his organization. It seems, therefore, safe to conclude that the curve to which the series of recrods approaches is of the character of a parabola—*i.e.*, one which continually becomes more and more nearly horizontal as the speed of the horse gradually approaches its highest possible limit.

De Vries, then, contends that all new domestic breeds have arisen by the discontinuous method as definite novelties. Darwin himself was perfectly aware that this is usually the case, but the conclusion which he drew from the fact was a different one, as the following passage shows:

'He (man) often begins his selection by some half-monstrous form, or at least by some modification prominent enough to catch the eye or to be plainly useful to him.' But he goes on : 'Under Nature, the slightest differences of structure or constitution may well turn the nicely-balanced scale in the struggle for life, and so be preserved.' *

Of the origin of a new type of plant in this definite and sudden fashion, the Shirley poppies afford an excellent example. These originated in a mutation of the common wild field-poppy (*Papaver rhœas*). In 1880 the Rev. W. Wilks, Vicar of Shirley, near Croydon, noticed among a patch of this plant growing in a waste corner of his garden a solitary flower, the petals of which showed a very narrow border of white. The seeds which this flower produced were sown, and next year, out of about two hundred plants, there were four or five upon which all the flowers showed the same modification. From these, by further horticultural processes, the strain of Shirley poppies originated. We may point out in passing that if the original plant had been self-pollinated, a much larger proportion of the new type might have been expected to appear in the next generation.

In the course of his own experiments, de Vries has obtained quite a number of new types of plants by methods like the above. It is to be observed that the novelty in these cases usually shows a considerable range of normal variability of its own, and that its first appearance is generally in the form of an extreme

* 'Origin of Species' 6th ed., p. 60.

negative variation* from the proper type of the new variety. In this way the novelty may not appear to be very far removed from a high normal variation of the original type. The behaviour of the progeny of the two types, however—types which might thus in themselves be readily confused—is entirely different, and a ready means of distinguishing them is thereby provided. Each set of offspring shows regression to its own proper modal value; so that the offspring of the novelty show a further marked development of the new features, whilst the offspring of an extreme normal variation resemble the type of the original form more closely than they do their own immediate progenitor.

If new types are not produced among domesticated productions by the action of artificial selection, and all that selection can effect is to pick out definite novelties when they occur, the analogy between natural selection and artificial selection breaks down, and a large and important section of the evidence in favour of the production of natural species by the action of natural selection is destroyed. In the place of this explanation de Vries would put the theory of mutation, according to which new species arise by single steps as definite novelties, just in the same way as we find that domestic varieties are produced. More than this, de Vries believes that he has discovered a set of new species in the very act of originating from an old one in this way, a discovery which affords the basis

* *I.e.*, a variant belonging to a class situated some distance from the mode of normal variability of the novelty, and on the side of it nearest to the mode of the original type.

and groundwork of the views which he puts forward.

The plant which afforded the material for this discovery is known as *Œnothera Lamarckiana*—that is to say, this is the name of the old species from which the new species were found to be arising. *O. Lamarckiana* is an American plant, but the specimens which de Vries found to be in a state of mutation had made their escape from a garden, and were running wild over a disused potato-field near a town called Hilversum, in Holland. On examining these plants, de Vries found two distinct new forms, which were quite unlike the remainder. Each kind occurred in an isolated patch, as if it had arisen from the seed of a single plant.

No description of either of these forms was to be found in botanical literature, nor were there specimens of them in any of the great herbaria. But when de Vries took seeds from some of the plants and sowed them in his garden, he found that the new forms came true to type—the plants produced resembled the parents from which the seeds were taken, and not the normal form of *O. Lamarckiana.*

Here, then, we have a case in which two new species had originated from an old one in a state of nature. But de Vries went further than this, and took measures for observing the actual origin of new forms in the cultivated offspring of the semi-wild *Œnothera.*

For this purpose he transplanted a number of roots from the field where they were growing, and also took seed from a number of other plants, and from these he cultivated large numbers of seedlings for a series of

generations. The net result of his experiments was this : out of about 50,000 individuals which were grown to a recognisable stage, more than 800 showed mutation—that is to say, they differed specifically from the parent *O. Lamarckiana*. The 800 individuals belonged to about fifteen new kinds, most of which appeared repeatedly, though some were more frequent than others. The process of mutation had, therefore, taken place in about 1½ per cent. of the seedlings which were grown, and owing to various reasons this estimate is probably considerably too low. For example, many of the new forms were very weakly, and often died before it was possible to distinguish them. Others, again, could not be recognised until an advanced stage of their growth had been reached, whereas only a small proportion of the seedlings raised could be grown after they had reached any considerable size, owing to considerations of space.

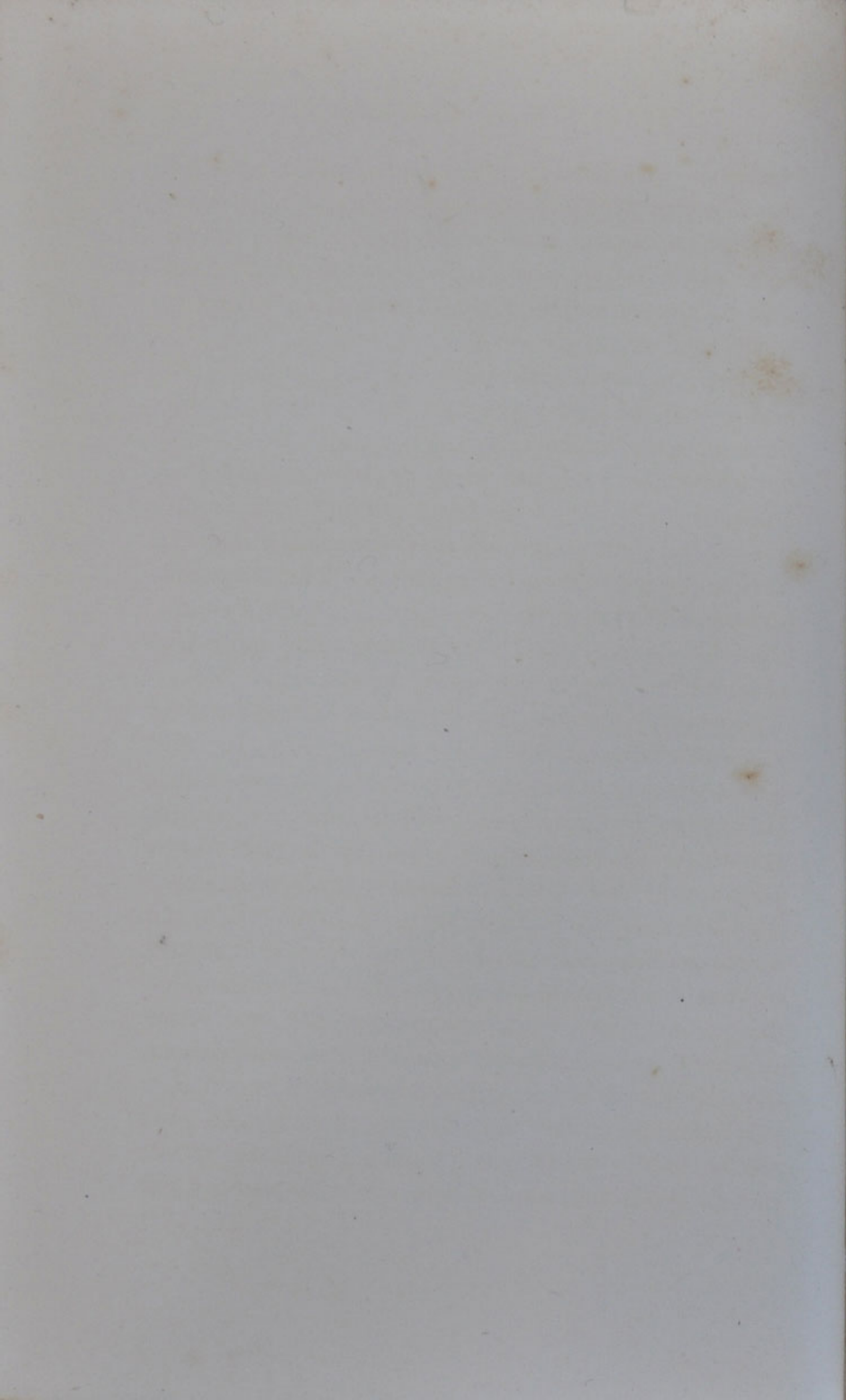
We cannot now follow de Vries very far into his elaborate account of his new species and of the way in which they originated ; a few general remarks only must suffice. Many of the new forms were recognisable as quite young seedlings, notably *O. albida*, others not until a much later period of their growth. *O. gigas* was the finest and strongest of the new forms, but only made its appearance on two occasions. *O. lata* also appeared to be as strong as the parental type, whilst two other forms were able to survive in nature in competition with the original species, as has been already described. Other forms which were grown and flowered were plainly less well fitted for

FIG. 12.—MUTATION IN ŒNOTHERA.

(*From de Vries.*)

Top row	*Lam.*	*lata*	*Lam.*
Second row ..	*subovata*	*albida*	*Lam.*
Third row	*albida*	*albida*	*lata.*
Fourth row ..	*oblonga*	*lata*	*Lam.*
Fifth row	*Lam.*	*Lam.*	*rubrinervis.*

[*To face p.* 136.



the battle of life than *O. Lamarckiana*, and only reached the flowering stage by the help of careful cultivation, and others, again, were never got to flower at all. Some of the latter, however, were readily distinguishable by the strikingly original types of radical leaves which they exhibited.

When they had once made their appearance, the majority of the new types came true to seed. Sometimes new mutations appeared among their offspring, but these always appeared in smaller numbers than among the offspring of the parent *O. Lamarckiana*, and some of the commoner mutations were usually omitted, so that it appeared as if the process of mutation was accompanied by a tendency towards a fresh stability. Some of the most marked new forms came quite true so far as the observations were carried.

Speaking generally, the nature of the differences which distinguished the new forms from the parental species was just of the same type as that of those which distinguish Jordan's species when found in nature. The differences were not, as a rule, of the sort shown when new garden varieties arise as sports. An example of this latter kind occurred, however, in the case of the new form *O. nanella*, which was a dwarf or permanently stunted form, but in other respects closely resembled the parent type. Apart from this, the new forms appeared to be given off quite at random, without showing any definite tendency towards progress in a particular direction. One of the new species was almost sterile as far as its ovules were concerned, though producing good pollen, whilst in another the

formation of the pollen was very defective. None of the others was lacking in either of these respects. Each new form was distinguished by certain definite features which affected almost all its parts, not by one new character only; and these features were never separable, but always appeared in common on the same plant.

The new species, of course, showed normal fluctuating variability, and, as an extreme result of this variability, forms occasionally appeared midway between one of the new species and the parental type. In such cases, when the self-fertilized seed of the plant showing such an intermediate character was sown, the offspring were found to group themselves round the normal form of the new species *or* round that of the parent *Lamarckiana*, thus affording evidence as to the true nature of their parent.

Whether or not we are prepared to accept the whole of de Vries' conclusions from his experiments, we can see at least that from one point of view they are of the very greatest importance. For before de Vries published this work it had been supposed to be quite impossible to make direct observations upon the manner of origin of new species in Nature. De Vries has now shown that such observations can be made, and this is in itself a most valuable piece of information. He has introduced an entirely new method into the domain of species research, and one by the use of which it is to be hoped that before long a definite answer will be obtained to the question whether species in general arise by definite steps, or with an imperceptible degree of slowness.

When results of the novelty and importance of those which have been published by de Vries are brought to our notice, we are naturally disposed to reserve our acceptance of the conclusions which they seem to indicate until observations have been made in confirmation of them by some competent observer. This has now been done by Professor MacDougal at the New York Botanic Garden. MacDougal has carried out observations similar to those above described upon the offspring of seeds sent by de Vries from Holland, and with closely similar results. Thus he has observed all the new forms which de Vries described, as well as some additional ones; and he has obtained an even higher percentage of 'mutants' than de Vries himself—namely, about 3 per cent. of the total number of seedlings grown. This last result is probably only due to the application of more thorough methods of investigation, and to a smaller mortality of the weakest plants, arrived at by greater care, and rendered possible by the warmer summer climate and by American efficiency in method. De Vries himself, in one of his later generations, when particular care was applied to the methods of cultivation, obtained nearly 3 per cent. of new forms. MacDougal also states that he has observed undoubted cases of mutation taking place in other species besides *Œnothera Lamarckiana*.

It appears, then, that there can be no doubt about the genuineness of the phenomenon described by de Vries. But it is, of course, quite a different thing to assert that all natural species arise in this fashion, and this is what de Vries' theory, as distinguished from

his facts, amounts to. De Vries made observations upon a large proportion of the plants of his district by the method of growing great numbers of their seedlings, but he failed to find the same phenomenon going on in any of them. He therefore supposes that species are subject to comparatively short periods of mutability which recur at relatively long intervals, and that all the species he examined except the *Œnothera* were in this intermediate staple period of their existence. Direct proof of this suggestion is naturally out of the question.

It will be well to summarize briefly the conclusions at which de Vries has arrived, as the result of his observations upon *Œnothera*.

The following are the points to which he attaches chief importance:

1. The new species arise suddenly at a single step, without transitional forms.

2. They are usually fully constant from the first moment of their origin.

3. The distinctive characters of the new forms agree in kind with those which distinguish from one another such old and established species allied to *Œnothera Lamarckiana* as *O. biennis* and *O. muricata*. Only one of the new forms—namely, *O. nanella*, a dwarf type—is analogous with any ordinary kind of variety of garden origin.

4. A considerable number of individuals of the same sort usually make their appearance at the same period.

5. Although the new types vary in a normal fashion, and frequently transgress the limits dividing them

from the parental type, yet their first appearance has nothing to do with normal or continuous variability.

6. The mutations take place indefinitely, showing no special tendency in any particular direction.

7. The tendency to mutate recurs periodically. But, as was previously stated, there is no direct evidence of this last supposition.

In addition to what has already been said with reference to the method of origin of garden varieties in general, de Vries has described a number of special phenomena regarding the behaviour of garden varieties of plants, some of which are of considerable interest. Taken together, the facts substantiate to a great extent the view that selection does not of itself lead to the production of specific characters. But de Vries also introduces certain new conceptions which require to be briefly described on account of their great general interest to practical breeders and gardeners. They consist in the idea of races existing intermediate between a species and a complete variety or sub-type of it. Such *between-races* are of two kinds, of which it is unusual to find both in the case of the same species; moreover, either of them may occur even when the complete variety is quite unknown. In the case of a *half-race* a small percentage only of seedlings is found to produce plants which show the racial character, the remainder being of the original specific type; and even if the racial type is selected for several generations, the percentage of plants of this type which is produced does not notably increase. A *mid-race*, on the other hand, can readily be improved by selection, and when

best developed as a rule either shows the racial character in about half of the seedlings produced, or else exhibits in the great majority of its members a combination of the character of the species with that of the race. As an example, we may take the case of variegated plants, in which the leaves show streaks or patches of a yellow colour owing to the want of development of the proper green tint. An ordinary variegated plant, then, is looked upon as showing a combination of the green type with the yellow character of a completely modified race—the *aurea* variety, although the latter exists as such only in a few rare cases, in which the plants bear leaves showing no green pigment at all. On the other hand, many species of plants produce a small proportion of variegated individuals at each sowing, as is often the case, for example, with Indian corn; and this circumstance, according to de Vries, indicates the existence of the corresponding half-race.

The relative development of the two coexisting characters in such cases is highly variable, as anyone may observe for himself in variegated grasses and similar plants.

It might be supposed that it would be possible to pass from the species to the half-race, thence to the mid-race, and so on to the complete race simply by selection. De Vries shows that this is very rarely, if ever, the case. He regards the passage from a half-race to a mid-race, for example, as a mutation, and his observations seem to show that this transition is not more frequent than any other mutations.

FIG. 13.—TRIFOLIUM PRATENSE QUINQUEFOLIUM.
(*From de Vries.*)

[*To face p.* 143.

As a further illustration of what is meant by a between-race, mention may be made of the five-leaved race of purple clover (*Trifolium pratense*) obtained by de Vries, and developed by a process of selection. It would appear that the plants occasionally found growing wild, which bear a single four-lobed leaf, usually belong only to a half-race. De Vries was fortunate enough to find two plants upon each of which several of the leaves showed this anomaly, and from these, by an elaborate process of selection extending over several years, a race was obtained, the leaves of which in the majority of cases showed five lobes, whilst some had six or seven. Since, however, it appeared impossible to get rid of a certain proportion of three-lobed leaves, and equally so, on the other hand, to obtain leaves with more than seven lobes, de Vries concluded that his experiment exemplified the development of a mid-race, and not that of a constant race or true variety.

Some of the experiments upon which the above-mentioned conclusions of de Vries are based are open to the criticism that sufficient precautions do not seem to have been taken to prevent intercrossing between the selected plants and other types of different constitution, and it is only just to mention that such cases are usually pointed out by the author himself. In these cases the possibility of cross-fertilization between the novel type and normal members of the species from which it was derived renders the conditions of experiment closer to those obtaining in Nature ; but full precision in the interpretation of the results is thereby sacrificed, and it will be of interest to have evidence

of confirmatory cases in which thoroughly definite methods of pollination have been used.

The views of de Vries with regard to the actual origin of new species may be summed up as follows: Broadly speaking, species arise by mutation, by a sudden step in which either a single character or a whole set of characters together become changed. In the former case a new variety in the strict sense of the word is the result; in the latter a new species (according to Jordan's definition) is produced.

But mutation may be of several kinds. In the first place, an entirely new character or set of characters may make its appearance. To such a phenomenon de Vries applies the term of *progressive mutation*, and it is by steps of this kind that he believes the main divisions of the vegetable kingdom to have been built up. In the case of such mutations the new character is supposed to come into existence first in a latent or hidden condition, and it may be only after many generations that it makes its appearance visibly. On this view the period of mutation is preceded by a premutation period, during which the appearance of the new character is being prepared for.

A second method of species formation, entitled by de Vries *degressive mutation*, is indicated when a change takes place in the partial latency of a character. A completely latent character is, indeed, unrecognisable as such. But characters may also be only partially latent, and in these cases they exhibit themselves from time to time in rare individuals in the form of sports or abnormalities—a phenomenon which we have already

seen to be characteristic of half-races ; indeed, a half-race might have been defined as a strain in which the character of the complete race is usually latent, and only rarely appears. An active character, on the other hand, is apparent in the great majority of the individuals of a race. If, now, a change from latency to activity occurs suddenly, this is a form of mutation. The reverse case, too, may occur—a character previously active may become latent ; the character then appears to be lost, and the mutation is said to be *retrogressive.* De Vries regards the great variety of allied species which is to be found in many groups as being to a large extent the result of retrogressive mutation. This type of mutation is also frequent among cultivated plants. Thus, the appearance of a white variety of a species previously only known to produce coloured flowers may constitute a good example of a retrogressive change. Mutations may also be atavistic, consisting in what is known as a "throw-back" to a previous ancestor. In the most usual form of this phenomenon an ancestral character which had previously become latent shows itself once more in the active condition. Finally, new and distinct types may arise by the intercrossing of separate species, but this is not regarded by de Vries as being an important source of permanent new forms.

Without following de Vries into all the niceties of his theory as to the particular kinds and methods of mutations, we must admit that his experiments go far to establish the doctrine, in support of which a considerable amount of evidence had previously been

accumulated, especially by Bateson, that the origin of species in Nature is generally a definite process, and takes place by steps of considerable amplitude. What, then, is the meaning of individual differences, of that continuous variability which is often so considerable, and of the inheritance of this kind of differences which the biometricians have been at so much pains to prove? De Vries points out that for no two plants are the conditions of life exactly the same; a considerable degree of diversity among the plants themselves is therefore advantageous, even when these belong to the same specific type. Upon continuous variability depend local races, forms adapted to wetter and drier situations, highland and lowland races, and the like, but none of these are permanent. As regards the cause of this variability, apart from the effect of sexual reproduction, which combines the tendency to vary of two separate parents, de Vries believes that individual variability depends entirely upon nutrition; but under this head he includes practically the whole environment of plants—light, space, soil, moisture, and the like. Characters acquired in a similar way by previous generations are inherited, and the effect of conditions upon the developing seed whilst still borne upon the parent plant may be considerable. Thus easily does de Vries dispose of the puzzling question of the inheritance or non-inheritance of acquired characters. Acquired characters are inherited; they are not of any importance in the origin of species.

According to the view upheld by Wallace, Weismann, and others, the actual origin of specific distinctions

takes place by natural selection acting upon individual differences ; and in this case it is to be observed that it is the struggle between individuals of the same species which is of primary importance. On the mutation theory it is only the competition between allied species which interests us from the point of view of evolution. Natural selection is thus regarded as having no influence in the formation of species themselves. On the other hand, the gaps existing between genera and still larger groups, such as families and classes, are still supposed to be due to the destructive action of natural selection determining the survival of the fittest species, so that this principle is by no means ousted from its prominent position in the philosophy of evolution even when the latter is expounded by the mutationist.

One further point. On the theory of mutation the survival of useless structures becomes readily comprehensible. Indeed, a structure which is actually of the nature of a handicap to its possessor may fail to cause extinction if it is either combined with a vigorous constitution, or correlated with other characteristics which are sufficiently useful to make up for the disadvantages entailed. The survival of many apparently useless and some apparently harmful structures is very difficult to understand on the hypothesis of a continuous evolution by the survival of the fittest individuals. This is an argument upon which de Vries lays considerable stress, although it may be pointed out that it is usually very difficult to form a judgment as to the real usefulness or otherwise of organs.

CHAPTER VI

THE OLDER HYBRIDISTS

THERE is one side of the practical study of heredity which dates back to the middle of the seventeenth century—namely, that branch of the subject which is concerned with the hybridizing or artificial cross-breeding of different species and varieties of plants. Quite recently the great importance which attaches to this method of study has been realized once more, and the interest thus awakened has led to a closer examination of the accounts of experiments undertaken a century or more ago, with the result of showing that much of the work then carried out in this direction had attained to quite an astonishing degree of excellence. In the brief sketch of the history of hybridizing work here following, account will be taken almost exclusively of experiments of which the interest is not historical only, but which possess an actual scientific value. Amongst other matters of interest, it will be found that more than one observer came very near to anticipating Mendel's epoch-making discovery, and thus arriving at the clue which should unravel almost all the complex problems which beset the early hybridizers.

Joseph Gottlieb Kölreuter, 1733-1806.

(*After an engraving by* J. Cederquist.)

[*To face p.* 148.

Following the modern usage, we shall apply the term 'hybrid' to all individuals arising from a cross between parents which belong to distinct groups, no matter whether these groups are separated as distinct genera or species, or whether they are regarded as representing only different races or varieties. This wide interpretation of the term hybrid has only recently been reintroduced. The use to which it has returned is, indeed, the original one; but many intermediate writers, including Darwin, confined the employment of this expression to cases of crossing between species, and applied the word 'mongrel' to the offspring of crosses between races or varieties of the same species. Darwin, however, did not regard species as differing in kind from varieties, and he even particularly emphasized the smallness of the distinction which can be drawn between the behaviour and properties of hybrids and mongrels respectively. Indeed, he came to the highly important conclusion that the laws of resemblance between parents and their children are the same, whatever may be the amount of difference between the parents in question—whether, that is to say, they are distinguished only by individual differences, or whether they belong to separate varieties or even species. We have already seen that the more recent facts of biometry point strongly towards the conclusion that individual and race differences are inherited at approximately the same rate. It seems, however, to be at present somewhat doubtful whether all sorts of specific differences follow the same law of propagation on cross-breeding.

Between 1760 and 1766 Joseph Gottleib Kölreuter carried out the first series of systematic experiments in plant hybridization which had ever been undertaken. These experiments not only established with certainty for the first time the fact that the seeds of plants are produced by a sexual process comparable with that known to occur in animals, but also led to a knowledge of the general behaviour of hybrid plants, which was scarcely bettered until Mendel made his observations a century afterwards.

Kölreuter found that the hybrid offspring of two different plants generally took as closely after the plant which yielded the pollen as after that upon which the actual hybrid seed was born. Indeed, he found that it made little or no difference to the appearance of the hybrid which of the parental species was the pollen-parent (male), and which the seed-parent (female)—that is to say, in the case of plants the result of reciprocal crosses is usually identical. Thus, for the first time it was definitely shown that the pollen-grain plays just as important a part in determining the characters of the offspring as does the ovule which the pollen-grain fertilizes. This was a wholly novel idea in Kölreuter's time, and the fact was scarcely credited by his contemporaries.

Kölreuter had no means of discovering that the contents of a single pollen-grain unite with the contents of a single ovule in fertilization. But he ascertained by experiments that more than thirty seeds might be made to ripen by the application of between fifty and sixty pollen-grains to the stigma of a par-

ticular flower, so that, if he had had any hint of the actual microscopic processes of fertilization, he would have been quite prepared for the more fundamental discovery.

Kölreuter, indeed, believed that the act of fertilization consisted in the intimate mingling together of two fluids, the one contained in the pollen-grain, and the other secreted by the stigma of the plant. The mingled fluids, he supposed, next passed down the style into the ovary of the plant, and arriving at the unripe ovules, initiated in them those processes which led to the formation of seeds. In this belief Kölreuter simply followed the animal physiologists of his time, who looked upon the process of fertilization in animals as taking place by a similar mingling together of two fluids. Now that we know that fertilization consists essentially in the intimate union of the nuclei of two cells, one of which, in the case of plants, is the ovum contained within the ovule, whilst the other is represented by one of a few cells into which the contents of the pollen-grain divide, we can understand more clearly the bearing of Kölreuter's observation. And it is greatly to this eminent naturalist's credit that he succeeded in carrying out his observations with so much accuracy, when the full meaning of those observations was of necessity hidden from his comprehension.

Kölreuter was the first to observe accurately the different ways in which pollen can be naturally conveyed to the stigma of a flower. This may take place either by the pollen-grains falling directly upon the

stigma, or by the agency of the wind, or, lastly, the pollen may be carried by insects visiting the flowers. And he recognised many features characteristic of flowers apt to be fertilized in one or other of these ways in particular. Thus he was aware, for example, of the nature and use of the nectar which so many flowers produce—namely, that it is the substance from which the bees—by far the most diligent visitors of flowers—obtain their honey.

Curiously enough, Kölreuter was not aware of the existence of any natural wild hybrid plants. But he was quite right in contending that supposed examples of such hybrids required for their substantiation the experimental proof, which could only be afforded by making actual artificial crosses between the putative parent species.

The first hybrid made artificially by Kölreuter was obtained in 1760 by applying the pollen of *Nicotiana paniculata* to the stigma of *Nicotiana rustica*. The hybrid offspring of this cross showed a character intermediate between those of the two parent species in almost every measurable or recognisable feature, with a single notable exception. This exception was afforded by the condition of the stamens and of the pollen grains produced by the hybrids. These organs were so badly developed that in all the earlier experiments self-fertilization of the hybrid plants yielded no good seed at all, nor were the pollen grains of the hybrid any more effective when applied to the stigmas of either of the parent species. On the other hand, when pollen from either parent was applied to the

stigmas of the hybrid plants, a certain number of seeds capable of germination was obtained, although this number was much smaller than in the case of normal fertilization of either parent species. This partial sterility, affecting in particular the stamens and the pollen which they produce, is a feature common to the majority of hybrids between different natural species. Many such hybrids, indeed, are altogether sterile, so that a further generation cannot in any way be obtained from them. On the other hand, the members of different strains or varieties which have arisen under cultivation yield, as a rule, when crossed together offspring which are perfectly fertile.

In subsequent years Kölreuter was able to obtain a very few self-fertilized offspring from hybrids of the same origin as the above. The resulting plants were described as resembling their hybrid parent so closely as to be practically indistinguishable from it.

The offspring obtained by crossing the hybrid plants with pollen from either parent showed in each case a form more or less intermediate between that of the original hybrid and that of the parent species from which the pollen was derived. But the plants were not all alike in this respect, some of them being much more like the parent species than others, and some, again, varying in other directions. There were also considerable differences between the different individuals in respect of fertility, so that some of the plants were more and some less sterile than the original hybrids. Also, there was some tendency to the production of malformations of the flowers and other parts.

One of the most noted of Kölreuter's experiments was that which consisted in repeatedly recrossing a hybrid plant with one of the parent species from which the hybrid was derived. By continuing to pollinate the members of one generation after another with the pollen of the same parent species, plants were at last arrived at which were indistinguishable from the parent in question. We shall return to this fact later on, when the reader will be in a position to appreciate its importance more fully.

Kölreuter found that the result of reciprocal crosses is usually identical—that is to say, the offspring obtained by fertilizing a plant A with pollen from a plant B are not to be distinguished from those obtained when B is fertilized with the pollen of A. But the two opposite processes of fertilization are not always equally easy to carry out. An extreme instance of this circumstance was met with in the case of the genus *Mirabilis*. *Mirabilis jalapa* was easily fertilized with pollen from *M. longiflora*. During eight years Kölreuter made more than two hundred attempts to effect the reverse cross, but without success.

It was shown by Kölreuter that hybrids between different races or varieties of the same species are usually much more fertile than hybrids obtained by crossing distinct species. Indeed, he believed that varieties of a single species were in all cases perfectly fertile together, whilst hybrids between species always showed some degree of sterility. But in this case Kölreuter based his definition of a species upon the very point at issue, and when he found forms, which other

botanists regarded as good species, to be perfectly fertile together, he immediately regarded them as being only varieties of a single species.

One curious point is worth quoting in this connection. Five varieties of *Nicotiana tabacum* were found to be perfectly fertile with one another, but when crossed with *Nicotiana glutinosa* one of them was found to be distinctly less sterile than the rest.

Another interesting point observed by Kölreuter was the fact that hybrid plants often exceed their parents in luxuriance of growth. Upon this fact, as we shall see later on, Knight and afterwards Darwin based theoretical conclusions of considerable importance in connection with the problem of sex.

Thomas Andrew Knight, who was also a botanist of high reputation in other fields, was the earliest observer to lay stress upon the practical aspect of the study of hybrids, and he occupied himself to a considerable extent with the improvement of useful races of plants by cross-breeding. Breeders of animals had already made important improvements by the method of intercrossing different races, and selecting the most notable types which made their appearance in consequence, when Knight bethought him of applying the same principles to the improvement of plants, and particularly of fruit-trees.

Knight also carried out a series of experiments with domestic peas, the results of which were published in 1779. These experiments have a particular interest from the historical point of view, since it was by dint of similar experiments upon the same kind of plants

that Mendel's law was afterwards discovered. This very discovery might even have been made by Knight himself, if he had only realized the importance of ascertaining on a large scale the numerical proportions in which the different kinds of plants, arising in the second generation from the crosses, made their appearance. Unfortunately, this particular form of inquiry never seems to have occurred to him.

Knight's experiments were made with a different object in view—namely, that of discovering whether a cross with a distinct race would provide the stimulus necessary to restore its lost vigour to a strain of plants which was supposed to have become debilitated, owing to its members having been bred exclusively by self-pollination for a long series of generations.

The result of the experiments undoubtedly established the fact that in some cases the hybrid offspring of two distinct races shows a more vigorous habit of growth than either of the parental types. The following extract from Knight's own account will indicate the nature of the experiments upon which his conclusions rest:

'By introducing the farina of the largest and most luxuriant kinds into the blossoms of the most diminutive, and by reversing this process, I found that the powers of the male and female, in their effects on the offspring, are exactly equal. The vigour of the growth, the size of the seeds produced, and the season of maturity, were the same though the one was a very early, the other a very late variety. I had in this experiment a striking instance of the stimulative effects

of crossing the breeds, for the smallest variety, whose height rarely exceeded two feet, was increased to six feet, whilst the height of the large and luxuriant kind was very little diminished.'

We shall see, however, that the phenomenon last alluded to admits of a different interpretation.

It was upon the somewhat slender basis afforded by this experiment that the generalization known as the Knight-Darwin law was originally established. Knight's own expression of this idea was to the effect that 'Nature intended that a sexual intercourse should take place between neighbouring plants of the same species.' And the same conclusion was expressed still more forcibly by Darwin in the aphorism : 'Nature abhors perpetual self-fertilization.' But although it may be true that in a considerable number of cases advantages are gained from the process of cross-fertilization between different members of the same species, which do not accrue when self-fertilization takes place, yet several cases are now known in which self-fertilization really does seem to be indefinitely continued.

Knight crossed a pea having white flowers and seed-coats, and green stems, with one in which the flowers and stems were coloured purple, and the seeds grey. The seeds immediately resulting from the cross were unchanged in appearance, but the plants arising from these took closely after their coloured male parent. On crossing the cross-bred plants once more with a white strain a certain proportion of white plants was again obtained, though what that proportion was

Knight failed to notice. He observed, however, that white crossed by a purple strain invariably gave purple, whilst the cross-bred purples, when crossed again with white, yielded some white and some purple plants.

In 1822 John Goss recorded the fact that a 'blue' pea crossed with a 'white' yielded from the crossed flowers pods with white seeds only, the seeds contained in other pods upon the same plant being, of course, blue. The plants produced from the white seeds bore some pods with all blue, some with all white, and many pods with both white seeds and blue ones; and a coloured plate is given which shows one of the latter pods together with its contents. The blue seeds, when sown separately, yielded plants which produced blue seeds only, but plants arising from the white seeds yielded a mixture of blue and white seeds.

Knight pointed out quite correctly that the colours of the seeds which are here referred to are occasioned by the colour of the cotyledons or seed-leaves of the pea, which are visible through the semitransparent seed-coat. Green cotyledons give rise in this way to a bluish appearance, whilst, when the cotyledons are yellow, the resulting appearance of the seed is described as whitish.

The Hon. and Rev. W. Herbert was another observer who made many important experiments in hybridization towards the beginning of the nineteenth century. These led him to the conclusion that Kölreuter and Knight were wrong in their assertion that hybrids between distinct species were always sterile. Herbert considered that only generic or family types were

constantly sterile, and this led him to the further conclusion, now believed to be erroneous, that the separate genera or families were those which were originally created, whilst he believed that the separate species of the same genus arose from a single original type by a genuine process of evolution.

The most prolific in work of all the hybridists, however, was undoubtedly Carl Friedrich v. Gaertner (1772 - 1850). Gaertner made a great number of crosses between species belonging to all sections of the natural system, and his book, published in 1849, contains a great mass of valuable information. Gaertner's theoretical conclusions, for the most part, only amplify and confirm those of Kölreuter, upon whom in this direction he made but little advance.

C. Naudin's essay, entitled 'New Researches on Hybridity in Plants,' made its appearance in 1862. The author pointed out that the facts of the return of hybrids to the specific forms of their parents, when repeatedly crossed with the latter, are naturally explained by the hypothesis of the disjunction of the two specific essences in the pollen grains and ovules of the hybrid. The idea may, perhaps, be made somewhat clearer as follows: Let us consider the case of a species A crossed with another species B. Naudin supposes that some of the pollen grains and ovules of the hybrid plant will be potentially* of the exact

* When it is said that a pollen grain or ovule potentially resembles the species A, it is meant that the germ-cell in question is of such a kind that, when united with one derived from an ovule or pollen grain of similar constitution, it would give rise to a plant exactly resembling A.

character of one species (A), whilst others will bear no potential resemblance to A, but will be precisely similar in nature to the ovules and pollen grains of the pure species B. In cases where this separation of the materials representing the two types in a potential condition is complete, forms exactly resembling the parents might be obtained. As we shall see, this hypothesis makes a remarkably near approach to that of Mendel; and the importance of the fact that the first hybrid generation is generally uniform, as contrasted with the diversity of types often appearing in the second generation, is clearly recognised by Naudin. This observer considered the hybrid in the adult state to consist of an aggregate of particles, homogeneous and characteristic of a single species when taken separately, but mingled in various proportions in the organs of the hybrid, which is thus looked upon as a kind of living mosaic.

The only other discovery of first-class importance,* in addition to that of Mendel, made during the nineteenth century in the domain of hybridization, was that published by Millardet in 1894.

Millardet's principal experiments were made upon strawberries, of which plants he crossed together a number of different species and varieties. Contrary to what had been observed in the majority of such crosses between other species of plants, in which the offspring was usually more or less intermediate between the two parents from which it arose, Millardet

* That is to say, if it is really genuine. The phenomena do not appear to have been seen by anyone else.

found in a considerable number of cases that the offspring resembled one parent only, from which it was indeed indistinguishable, whilst no trace of likeness to the second parent could be detected in it. In some cases the resemblance was to the paternal species (pollen-parent), and in others to the maternal species (seed-parent). In several instances the hybrid offspring, on being self-fertilized, bred true to the type which they already exhibited, so that the second generation, like the first, seemed to derive its whole constitution from one parent, to the total exclusion of the other.

The precise meaning of this remarkable phenomenon is not clearly understood. There is some doubt as to whether Millardet's experiments were really sufficient to establish it as a scientific fact. Moreover, Millardet's observations have never been confirmed by later workers. In the absence of directly contradictory evidence it seemed necessary to draw attention to the facts as they have been described.

Great numbers of observations upon the characteristics and behaviour of hybrid plants and animals have been from time to time recorded, and the preceding pages contain only a brief selection of such facts as are most necessary for a proper understanding of modern work in hybridization. Until quite recently the laws of transmission of characters in hybrids were still completely hidden. The facts were wonderful enough, but they showed no signs of falling into an orderly arrangement. In the next chapter it will be our business to describe the remarkable discovery

which has introduced order into this previously chaotic region, and which has enabled a few workers to establish in half a dozen years the foundations of a great science, the importance of which is not at all generally realized.

Gregor Johann Mendel.

[To face p. 163.

CHAPTER VII

MENDELISM

We have already had occasion to point out how important it is, when engaged upon questions of heredity, not to treat whole animals or plants as units, but to deal with their separate characters one at a time. In the course of the present chapter the reason for proceeding in this way will appear more clearly, and we shall find that the adoption of this method is fully justified by the results which it enables us to obtain, and which could not have been arrived at in any other way. We shall also find reasons for believing that this method is the correct one from a theoretical point of view.

Naturally, considerable care is necessary in determining what are and what are not separable characters. At the outset it is not always possible to make this discrimination with certainty, but during the course of the experiments which follow it is almost always possible to arrive at a clear definition of each character, and in many cases the distinction of characters is quite obvious from the beginning.

Up to the present time the experimental study of heredity by the methods of definite breeding has yielded

clear and definite information only when applied to cases where clearly definable characters have distinguished the parental forms examined. This, however, is in great part due to the fact that the experimental method has scarcely yet been used in dealing with characters of a less definite nature. The science is still in its infancy, and attention has naturally been first paid to the simpler problems which it affords. The difficulties of treatment which confront those who would deal with highly variable characters and those of a 'more or less' nature are considerable, although there is no reason for supposing that such problems are insuperable. As we have seen, however, the majority of characters which distinguish species or races from one another appear to be of a perfectly definite description, so that the limitation just referred to is not so serious as might appear at first sight. The recent revival of work upon the subject of inheritance by the use of breeding methods has, as a matter of fact, already been rewarded with results as valuable and as clear as could possibly have been anticipated—results which are sufficient in themselves to show that the discovery made by Mendel was of an importance little inferior to those of a Newton or a Dalton.

It is important to remember that every animal or plant, which has come into existence in the ordinary way through sexual generation, owes its individuality to the mingled natures of two separate parents. The following lines, in which the poet Goethe speaks of his own hereditary endowment, have been quoted more than once in this connection :

> 'Vom Vater hab' ich die Statur,
> Des Lebens ernstes Fuhren,
> Vom Mutterchen die Frohnatur
> Und Lust zu fabulieren.'

In such a case we must always look upon the corresponding character of the second parent as existing in the offspring side by side with the character which finds expression, only the former is overpowered by the latter, and forced to remain invisible. That the hidden character is nevertheless actually present is shown by the fact that a feature characteristic of a particular grandparent, which did not appear in the parent, may reappear in the child. For instance, a characteristic masculine feature of the maternal grandfather may thus make its appearance in the son.

It is found that any individual may be looked upon as being to a large extent an aggregation of separate characteristics. In a pair of allied races a great number of the separate characters are the same in the two cases, the distinction between the two forms depending upon a few definite features only. The majority of salient characteristics are identical in such a pair, but some of the corresponding characters are opposed. Thus in different races of mankind complexions may be dark or fair, eyes blue or brown, hair straight or curly, and the like. Now the offspring of parents which had smooth and curly hair respectively might exhibit smooth or curly or intermediate (wavy) hair, according as one or the other character, or both in combination, made its presence obvious; for in the simplest cases both will necessarily

be present, though one may be hidden. What will happen in the grandchildren ?

The manner in which characters comparable with the above are actually transmitted has been worked out in the case of many races of animals and plants, and in cases where experimental matings can be readily carried out, and a large number of offspring reared, it is found that a simple rule applies which holds good in every example thoroughly examined hitherto. This law was discovered by Mendel about the year 1865, and has since been called by his name. Before enunciating it we shall consider the information afforded by the case of a single pair of simple characters. Afterwards we shall endeavour to show the application of the law to the more complex cases in which combinations of characters are concerned.

A grain of Indian corn or maize contains a germ or *embryo*, which under suitable conditions will give rise to the future plant. The embryo is surrounded by a certain amount of reserve food material constituting the *endosperm*—a store which is made use of by the young plant during its germination. The embryo arises as the result of a process of fertilization which takes place in the following manner : The ovum, or female cell hidden in a flower, contains a nucleus, and this on fusion with one of the nuclei derived from a grain of pollen initiates the vital processes which lead to the development of an embryo plant.

Nuclei are the central and, from the point of view of heredity, the most important parts of cells—the con-

stituent units of the plant body. The cells which, together with their nuclei, take part in the process of fertilization are known as *gametes*, or *germ-cells*—male and female respectively, the latter being the ovum.

It is less generally known that the endosperm of a grain of Indian corn arises by a very similar process to the one which gives rise to the embryo itself. A second nucleus derived from the same pollen grain fuses with a nucleus situated near the ovum, and to the product of this fusion the endosperm owes its origin. It is further found, so far at least as those characters are concerned to which we shall at present confine our attention, that these two important nuclei hidden in the same female flower are exactly alike in hereditary constitution, and so are the two generative nuclei derived from a single pollen grain. In consequence of this fact, the observed character of the endosperm may be regarded as a true guide to the nature of the plant into which the associated embryo will afterwards develop. The hereditary qualities of the two are exactly the same.

It is not difficult to find a variety of Indian corn in which the endosperm is yellow, and another in which the colour of this tissue is white, owing to the absence of any visible yellow pigment. If a female flower of a white variety is fertilized with pollen taken from a yellow variety, the resulting grain shows its hybrid nature by the presence of the yellow colour in its endosperm. This is found to be a regular rule. Grains upon a plant of a white strain which has been pollinated with 'white pollen' are white, but if pollinated from

a yellow strain the grains are yellow. On the other hand, the grains upon a plant belonging to a yellow strain retain their yellow colour even if the flowers which produce them have been pollinated from a white variety.

These facts are expressed in technical language by saying that yellowness is *dominant* over whiteness, and the latter is said to be *recessive*.

Let us now suppose that we have sown a number of the yellow grains derived from the cross *yellow* × *white** or *white* × *yellow*, and that we have exposed the female flowers of the resulting plants at the proper stage of their existence to the influence of pollen derived from a pure white strain, taking care that none of their own hybrid pollen falls upon them at the same time. The result of this experiment takes us at once to the very heart of the Mendelian theory. Half the total number of grains obtained in this way —from the cross (*white* × *yellow*) × *white*—are white, and half are yellow.

Thus in an experiment carried out in the manner described there were obtained upon ninety-five plants :

Yellow grains 26,792, or 50·03 per cent.
White grains 26,751, „ 49·97 „

But we must go further than this. On sowing the white grains obtained in this second generation (F_2), and allowing the plants obtained from them mutually to pollinate one another, cobs were obtained bearing exclusively white grains without any trace of yellowness.

* × is to be read 'fertilized with pollen from.'