THE GROWTH OF GROUPS IN THE ANIMAL KINGDOM

BY THE SAME AUTHOR

AN INTRODUCTION TO BIOLOGY

FOR STUDENTS IN INDIA

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PLATE 1: VARIETIES OF MUS RATTUS, × $\frac{5}{8}$

THE
GROWTH OF GROUPS
IN THE ANIMAL KINGDOM

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WITH TWO COLOURED PLATES

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PREFACE

THIS book deals with the subject of evolution in a somewhat discursive manner, but its main theme is indicated by the title. It points out that groups of like animals, such as are small in membership and temporary in duration, are common in nature. These groups are not considered to be equivalent to species, they are referred to as family groups. A large part of the book deals with the origin, fate, and significance of such groups. Here and there the question, What is a species? is raised, but not answered. The tenth chapter is an attempt to show that the Mutation Theory is important to the physician, and the last chapter borders on philosophy. The whole appears more compact, if it be regarded as an attempt to help on the new movement in biology, to spread those ideas referred to by some as the Mutation Theory, by others as Mendelism, ideas that are permanently associated with the names of Mendel, Bateson, and De Vries.

The writer's warrant for asserting himself lies not in the bulk, but in the variety of his experience, which has been derived from several sources, but from none in particular.

At the present day so many books are written by specialists for specialists that it is well to point out that

this one, though purely biological in scope, is the opposite of a specialist's book. It is hoped that if it comes into the hands of a taxonomist, a Mendelian, a pathologist, or an alienist, that he will take it as offered, not as an attempt to teach him his particular subject, but as an assortment of opinions which may be of suggestive value.

It is necessary to warn the reader, who may be inexperienced in biological matters, that the book is not in the least a review of the present position of biology, but an assortment of personal opinions, most of which are borrowed from well-known sources.

It may seem to the experienced reader that the main idea expressed in the last chapter is taken from M. Henri Bergson's "Creative Evolution," but most of the last chapter, including the attempted illustration of things varying in all directions, was written in 1910 before I made my first acquaintance with M. Bergson's enlightening philosophy. I wrote the passage "Variation is chaotic or 'in all directions.' Is it possible to know whether it is so or not? What does the phrase mean?" before reading the words of Bergson, "It is unquestionable that, when ordinarily we speak of disorder, we are thinking of something. But of what?" The matter is of little importance, but it must be referred to here, in mentioning the sources from which I have drawn.

My thanks are due to Mr. W. L. Tower of Chicago for permission to reproduce the illustrations on Plate II. It may seem that I have abused this kindness, by suggesting that his judgment of his own work cannot be considered final. It was, however, necessary for me to take this step, for if there is no evidence of the efficacy of mutants in the facts collected by Mr. Tower, there is less than none

in those collected by myself. My opinion, whatever its value, is derived more from his facts than my own. If I have misrepresented any of his facts to suit my own opinions, I have done so unintentionally and I offer my sincere apology.

The frontispiece is the work of Mr. B. L. Das. The figure on the left is of a dead rat lying upon its side, and is very true to nature. Of the others, one is from a stuffed specimen and the other from an animal living in a cage, consequently, these two have a less natural appearance, but in all the colour is a true representation. The other drawings are mostly traced from photographs which have already appeared in the publications of the Indian Museum. (Rec. Vol. III. Pt. I., Mem. Vol. II. No. I.)

The map of Poona was kindly provided by Captain Kundart, I.M.S., to whom I am otherwise much indebted.

I must also express my warmest thanks to Captain G. I. Davys, I.M.S., and to many other friends, for the most part my fellow-workers in the Indian Medical Service, for much help and hospitality during the progress of the investigations which form the chief subject of the book.

COLOURED PLATES

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I.	VARIETIES OF MUS RATTUS	*Frontispiece*
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THE GROWTH OF GROUPS

CHAPTER I

INTRODUCTORY

THE method of the origin of species is still an open question. Although most biologists regard Natural Selection as a creative agency, yet there are dissenters from the accepted opinion who cannot justly be disregarded as inexperienced. Dissent is the outcome of a difference of judgment which is inherent in the dissenter. We do not know which of the two conflicting opinions will survive the other. Time will show.

A book dealing with this question may be written in one of three ways. It may support one or other opinion, or it may support neither. The author may approach the subject with an open mind and describe the various observations which have been made, the various opinions which have been held. Books of that kind are of value to the student, with their help he may be able to form an opinion for himself; but they do less to hasten the survival of one or other opinion than those of the factional kind.

It is easy to be deceived as to what constitutes factional writing. An author may claim to have examined the evidence relating to a disputed question with an open mind and to have formed a decided opinion in one

direction ; but in so doing he deceives only himself, to the critical reader his personal conviction or bias is obvious from the first. It is well, therefore, to admit at the outset that this book is written in the factional spirit as an expression of opinion, derived mainly from the writings of others but supported by personal observation.

Darwin has said, " We have no evidence of the appearance or at least of the continued procreation, under nature, of abrupt modifications of structure." These words call attention to the fact that abrupt modifications may occur ; in other words, that an offspring may possess some definite and obvious quality or qualities which were not possessed by its parents. Furthermore, the words " under nature " remind us that such modifications may be propagated continuously under man's protection. Several races of domestic animals and plants are known to have arisen abruptly from sports.

It seems, however, that Darwin's conviction as to the inefficacy of sports was unsettled, for in the sixth edition of " The Origin of Species " there appears the following well-known passage. " . . . variations which seem to us in our ignorance to arise spontaneously. It appears that I formerly underrated the frequency and value of these latter forms of variation, as leading to permanent modifications of structure independently of Natural Selection." Besides the admission they contain, these words are interesting as showing that gradual change should be looked upon as the essential condition of Evolution by Natural Selection. A race arising from a sport, in captivity or under nature, would occur independently of selection, as that process was conceived by Darwin. To speak of selection among mutants is to change the

common conception of that agency. In its new *rôle* it does not appear as a creative process.

Several writers have urged the importance of sudden and obvious variations such as are called sports, or mutants. Huxley expressed his belief that races were occasionally derived from sports. In 1892 Bateson pointed to the discontinuity of species and explained it as the natural outcome of discontinuous variation. If by species we mean those groups which are being defined as such at the present day, and if by discontinuous variation we mean obvious differences which may be seen, exceptionally, when comparing offspring with their parents; then it seems to me that Bateson's proposition is indisputable. The evidence to be described in this book is in support of it.

In the Mutation Theory of De Vries we find the same opinions in a more decided shape, amply supported by evidence from the vegetable kingdom, indeed so complete is the evidence that botanists more generally than zoologists regard mutation as the method of evolution. Above all, through Mendel's work and all that has followed from it in the last decade, we have gained an insight into the nature of living things, compelling us to recognize that the change which comes over them is not comparable to a gliding movement, but to a series of little steps, not always of the same width but often wide enough to be appreciated with certainty.

DIFFICULTIES ARISING FROM THE USE OF THE WORD SPECIES.

Discussions on the origin of species are usually unsatisfactory because the term species has no decided

meaning. The word may call up a different picture in the minds of independent persons. De Vries and other writers have emphasized the fact that most of the Linnæan species are divisible, that they are groups of lesser groups. No zoologist will deny that this is also true of the animal kingdom. The tendency at the present day is to split up those species defined by the older naturalists into lesser groups and to regard the latter as species. But there is much difference of opinion in regard to the reality of a species. There are two opinions, namely, that they are real, and that they are conventional. De Vries was unable to distinguish between the members of certain groups of plants and considered those groups as elementary or indivisible. He therefore regards species as real. An opinion on this question can only be formed by examining and comparing the attributes of mature organisms, and the opinion will vary according to the part of the living kingdom which is examined. An observer, while regarding the higher plants or simpler kinds of animals, may be sure that indivisible groups are appreciable ; but if he turns his attention to vertebrate animals he will doubt whether his former conclusion is applicable to them. When dealing with such complex forms, he may feel inclined to say, "Give me a thousand individuals of any of your so-called elementary species and I will split them into lesser groups by taking into account certain minute characteristics." The structure and function of a vertebrate animal are so complex that it appears as an illimitable field for observation, in which one can explore and describe up to a point, depending upon the acuteness of the observer and the means of discovery. When dealing with a collection of mammalian

animals we may say that they are alike as regards certain characteristics which may be enumerated, but we can never be sure that the enumeration is final as regards each individual. In defining any such collection as a specific group, we close our description at a point which is chosen in an arbitrary manner. Species defined in this manner are merely conventional. On the other hand, those botanists who speak of elementary species, must believe, in regard to any particular species, that they have explored the whole field of observation afforded by it, that no one could see in it more than they have seen. The botanist is more likely to arrive at this opinion than the zoologist because the field for observation is simpler in plants than in animals, there is less scope for disagreement in observation; he feels that he can see and describe all that there is to see, that no man could see more, hence to him species are indivisible, or real and not merely conventional.

If the reader is of the opinion that species of mammals are also indivisible let him take another step higher and regard mankind, the most complex of living things and the one with which we are best acquainted. Among mankind we see such a variety of inheritable characteristics, present in some individuals but not in others, that we cannot possibly regard any group as indivisible. The individual alone is indivisible.

When two persons discuss the origin of species they will misunderstand one another if the one holds species to be real and the other regards them as conventional. If they differ on this preliminary point it is idle to proceed further.

In this book, species are assumed to be conventional.

There is, in my opinion, no such thing as an indivisible group of vertebrate animals.

HOW ARE SPECIES DEFINED?

What is a species among vertebrate animals? Species are groups that have been defined as such, but these groups are not equivalent things; some are characterized by many obvious attributes, others by a few less obvious ones. Those groups, which are being described as species at the present day, are for the most part of less weight than those described in the past. But although the conception has changed, species are merely groups that have been defined and the problem is to describe how they came to be as they are. In defining a group we are at liberty to regard any characters we choose. Naturally, we select the most obvious ones first. We select a number of obvious characters and set up a convention in the form of an assertion thus, "Every animal in the world which possesses characters A, B, C, etc., is of the species X." The systematist defines species in this manner, but in doing so he does not claim to have described every attribute which can be perceived by any means in every individual of the species, he has merely selected a number of obvious characters and defined the species by declaring that every animal possessing them is of the species. In this sense a species is a verbal description. In referring to a species it is considered necessary to add the name of its author; by the species we mean the author's description of it. The author of any vertebrate species must feel sure that he has overlooked a number of microscopic or unobtrusive characters which are also present in every

individual of his species. There is no essential difference between these unobtrusive characters and the more obvious ones chosen. Some characters are visible to the naked eye; others come into view when we use a hand lens; others again can only be seen properly with the help of a microscope. Thus in appearance they are gradually less and less perceptible. Those that are scarcely perceptible cannot as a rule become more evident by accumulation. We also know that specific differences exist beyond the point where they cease to become visible with the help of the microscope. The precipitin test has shown us that there are specific differences in the quality of the living substance. Therefore, in describing a species we must close our description at a point. Microscopic characters are often considered negligible. The only assertion implied in the definition of a species is that all the characters enumerated are present in every individual of it. The systematist then, must overlook certain characters because they are difficult to appreciate, although they may be present in every individual of the species he is defining. But, besides this, he deliberately overlooks certain perceptible and perhaps obvious characters, because they are not present in every individual of the species as he has chosen to define it, but only in some individuals. Such negligible characters are said to be individual and not specific, but none the less they may be perceptible and heritable attributes, although they are not present in every member of the species as defined.

What is the usual fate of a species X which has been defined in terms of certain chosen characters A, B, C? Let us suppose that it has been defined by a naturalist

whom we may call broad-minded or old-fashioned according as his methods seem to us to be good or bad. Sooner or later species X will come under the notice of another naturalist, who may be referred to approvingly as one of the modern school or disapprovingly as a splitter of hairs. He may express himself thus in regard to the species X. "I have examined a large number of the species X as defined by my predecessor, I can recognize them plainly because they all possess the characters A,B,C as defined, but I do not propose to overlook those features which have been regarded hitherto as individual variations; thus, among the old species X, there are a number of individuals which possess the slight but perceptible character a, also there are a number of others possessing characters a, b: I will therefore split up the old species into three new ones x, y, z, which henceforth will be defined respectively as A, B, C, as A, B, C, a, and A, B, C, a, b." This splitting process is being carried on at the present day; there is no doubt that it depends on the existence of characters which, although unobtrusive, can be perceived, and it seems that there is no natural limit to the process. We can never be sure that any particular group of vertebrate animals is indivisible.

Let us also consider some other opinions which may be held in regard to the splitting up of the old-fashioned species. The older school of systematists who take the broad view, feel that the splitting process is harmful to their science, the aim of which is to trace the distribution of species. They have confidence in their belief that old-fashioned species such as X, which were defined in terms of many clear characters, arose in one place and spread from that place; but they have no confidence

in the belief that the new-fashioned species which perhaps have only one or two special characters of their own, arose in one place only; in fact they feel pretty sure that such groups may arise on many occasions in widely separate places, that is to say at various points of the area which is occupied by the species X. They therefore object to x, y, and z being known as species, for if a species may arise in more than one place, all arguments concerning its distribution are futile, hence they object to any group being known as a species unless it has many clear characters of its own. The justice of their contention cannot be disputed. There is good reason to believe that groups such as x, which by some are spoken of as varieties, arise in separate parts of the area inhabited by the parent species, but there is little or no evidence that groups such as X arise in more than one place.

This discussion about groups X and x, will be clearer if in place of the letters we substitute known groups of animals. Three animals are seen in the frontispiece. All are alike in many ways, in their bodily proportions, in the form of their skulls and in most other respects; in a broad sense all three belong to the species *Mus rattus*, a species such as X, for they have many characters in common; but as regards a single character which determines their coat colour they are different, one has a special character which finds its expression in the whiteness of the underparts, another has the melanic character. There is evidence that groups such as these arise from the parent species in separate places on various occasions, the method of origin being growth from unity. There is no evidence to show how the parent group *Mus rattus* originated. These so-called varietal groups with their

one special character cannot be regarded as different from a species, it is true that they are clearly marked by one character only, but there are many other groups which are marked off by the possession of two, three, or more characters of their own.

A TACIT ASSUMPTION IN THE TERM SPECIES

Before inquiring how animals came to be in their present state, it is well to know that state. We cannot of course know this, for we cannot see the whole of the species at a moment, we can only form an idea of it, and this idea may be and often is erroneous. Every time that a species is defined and the type specimen laid down in a museum, the tacit assumption is made that there are in the world a very large though unknown number of animals like that type specimen. Since the number is unknown we should not assume that it is very large in every case. My opinion concerning type specimens is as follows. The type specimens in our museums are representatives of groups, but the groups are of variable numerical size. Many of them no doubt contain at the moment millions of millions of individuals, others contain tens of millions, others only thousands or hundreds. A considerable number of them have become extinct since the time of their definition ten or twenty years ago. By virtue of its specific name a small group of animals which occupies merely the corner of a field is made to appear equivalent to a vast multitude of another kind which is spread over most of the earth. Similarly, a group which made its first appearance a few years ago and has since become extinct is made equivalent to a

group which has come down unchanged from the Tertiary epoch. It is important to know whether this opinion is correct. It must be admitted that there is no reason for the assumption that every species contains an infinite and therefore negligible number of individuals. The museum worker who defines most of the species has little opportunity of forming an opinion as to the number of individuals which are represented by a given type specimen. The collector who supplies material to the museum is often able to form an opinion, but usually his attitude is humble. If the edict has gone forth that such and such a group is a species then henceforth it is and the matter is at an end, at least for the time. But when an inquirer fixes his attention on a group such as is called generic, and examines thousands of individuals belonging to that group extended over a wide area, he will come to know that groups called specific are very variable in the number of their component individuals, that some of them contain only a few thousands which are confined to a limited area. Few groups have been examined in this manner. The most complete inquiry of the kind, in the writer's knowledge, is Mr. W. L. Tower's investigation of the potato beetle, *Leptinotarsa*, in America, which extended over ten years. Most important are the results of the breeding experiments which were undertaken, but these will be referred to later on. The question to be considered here is—how were the beetles found in nature? A rare form called *pallida* was found, generally as single specimens isolated among great numbers of a commoner species, but on one occasion six specimens of *pallida* were found together; six is a group, but it is not what we call a species. Another form called *rubicunda*

composed a group which was much larger than this, but still it was limited. Thus, in August, 1903, it was found over an area of some seven or eight square miles, but in September, 1906, it was limited to a tract of about ten acres in extent, where only a very few specimens were found, hence the group was waning. The better known form *decemlineata* was first recorded in Nebraska in 1859, in all probability it was then occupying a limited area, at all events it was limited in comparison with the immense area which it came to occupy in the following fifty years. Taking the potato plant as its chief food it spread from Nebraska eastward as far as the Atlantic border, travelling at about the rate of sixty miles a year. The potato beetle is of great economic interest, consequently its distribution was carefully recorded. The relative importance of *pallida*, *rubicunda*, and *decemlineata* are known, but in the case of other generic groups which are not so well known the component species may have the same relative importance as those mentioned and yet be considered as of equal value, since each is represented by a type specimen and a verbal description. The only difference in our regard being that a group such as *pallida* would be considered rare, while a group such as *decemlineata* would be common. Now, it is well known that "rare" species are very plentiful. The verbal paradox, why are rare species so common, has occurred to many naturalists both of the past and present. It seems that *pallida*, *rubicunda*, and *decemlineata* are all typical of species so-called, and if we wish to explain how animals came to be as they are we must not shut our eyes to such groups as *pallida* and *rubicunda*, on the grounds that they are not exactly species since we know too much about them.

A species is usually regarded as a number of like animals; the number, being indeterminable in almost every case, is considered, perhaps for that reason, to be infinitely great in every case. When therefore we find a group containing so few individuals that the number of them can be determined, we say at once—this is not a species.

The conception of species as groups, which are invariably infinite in their component numbers, is not a true representation of nature and it hinders our understanding the origin of groups. There is no doubt that groups, such as are called specific, are variable in the number of their component individuals, and there is little doubt that many of them are quite small in this respect.

THE TERM GROUP

The word group appears in the title of this work and will be found throughout the text. It is used in place of the more usual terms species, sub-species, and variety. Whatever meanings may be applied to these terms it is evident that they are commonly used to indicate groups of like animals, hence the term group may be used instead of them. Set up any definition of the term species, or of the term variety; it will be possible to find groups of like animals which do not satisfy the requirements of either of those definitions. Take for example a common definition of the term species. A species is a group of animals which are fertile among themselves but sterile with all others. Take also a common definition of a variety as a kind of animal which is known to have been derived from a species. Let us now turn

to nature. The potato beetle, *Leptinotarsa rubicunda*, has appeared as the offspring of the commoner *L. multitæniata* and hence is derived from that species. But it is "almost completely" sterile with the parent species though perfectly fertile in itself. It is therefore almost completely a species in the terms of the first definition and entirely a variety in terms of the second.

Let us consider some other definitions. Species are groups which differ from one another in all their characteristics, whereas varieties differ from one another or from the parent species in only one or two well-marked characters. If we employ these definitions, we may often be sure that this group is a species and that a variety; but frequently we shall be in difficulty, since there are many groups in the animal kingdom which appear to have two, three, or more characters of their own, but yet do not differ from allied groups in all respects. Such groups are common among the higher animals, and although they are neither species nor varieties in the terms of the last-mentioned definitions, yet they are always regarded as species by naturalists. It is necessary to define the term group as used in this book, and in doing so a certain conception will be employed which has found wide but not perhaps general approval. Mainly as a result of Mendel's researches, we perceive that organisms contain certain attributes, which are distinct in their relation to the rest of the organism. These attributes or characters appear as separate entities, since each as a unit may be present in the body or absent from it without affecting the remainder of the body. This is assumed here to be true without discussing the physiological proof, the segregation of characters in the germ cells.

Some biologists, however, do not regard characters as perceptible things. Others, while admitting that characters are perceptible, deny that they are units. In the absence of a standard definition of the word unit, it is difficult to discuss such a question. In a strict sense there are no units in the organic world. At any rate, I assume that living bodies contain these separate and distinct attributes called characters. We may now define the term group.

A group is a number of individuals (more than one), each possessing some particular character or characters which are chosen arbitrarily as the distinguishing marks of that group.

OPINION AS TO CHARACTERS

Since characters are to play a part in that which is to follow, my conception of them must be described, although it is somewhat uncertain.

1. Every living body contains a large number of distinct attributes or characters. The colour of a flower, the brown pigment in the front layer of the human iris, the beard of wheat, are characters in that they may be present in or absent from the organism, apparently without affecting the rest of the organism.

2. As a rule characters appear to be superficial in the constitution of the organism. They are small details affecting the outward form or colour or small peculiarities which may be observed in any of the underlying parts, as for example in the skull of a vertebrate animal (Fig. 6). They are also visible to us as kinds of activity, such as we call instinct, habit, or behaviour. Certain of the more fundamental parts of the body

sometimes appear as characters; for example, the whole tail in mammals and birds and the horns of cattle which may be congenitally absent without affecting the rest of the body.

3. There is a relation between characters and man's perception of them which may be clearly recognized. Some characters are easy to see, while others are more difficult. We are apt to regard this relation as though it were quantitative, but it is not so in reality. A character that can only be seen with the help of the microscope may seem to be smaller than one visible to the naked eye, but quantitatively it is not so; for if we measure characters by their degree of visibility, where in the scale shall we place characters such as immunity from a particular disease, or where shall we place a character which is appreciable as an instinct or kind of activity. To regard this relation between various characters as quantitative is confusing, since there seems to be a real quantitative relation between characters which is a different matter.

How shall we express this peculiar relation? To call them major and minor is objectionable, since one is not larger than another. We should not speak of them as important and trivial, since an invisible but otherwise appreciable character may be of the utmost importance. The taxonomist usually regards them in this manner and overlooks those which are trivial. We must not speak of them as clear and obscure, for we can only deal with them when they are clear enough to be real. It is best to think of them as obtrusive and unobtrusive. We may say that various characters obtrude themselves upon our notice differently, and we must remember that their

relation in this respect is not quantitative. Unobtrusive characters cannot become obtrusive by accumulation. The pattern on a scale measuring less than a millimetre in diameter cannot be rendered visible by accumulation. Unobtrusive characters are not usually chosen as the marks of identification of species. Consequently they play no part in the problem of the origin of species. But since they are definite and inheritable they are important in evolution, especially as we see it among mankind at the present day. From this point of view there is a difference between evolution and the origin of species. The factors in evolution are unobtrusive characters, the factors in the origin of species are obtrusive characters. We are obliged to divide the problem in this way since the conception of species has been forced upon us. There is no essential difference between an obtrusive and an unobtrusive character. They obtrude variously. But since it is only the former that are considered to be specific we may divide the problem as suggested. If we investigate unobtrusive characters with great accuracy we can split up any so-called species into many sub-groups. In respect to its unobtrusive characters, every group of vertebrate animals is polymorphic. In taxonomy there is a progressive tendency to take into account the less obtrusive characters and to consider them as specific. The result is that the number of species is becoming greater and greater, although the species are becoming less obtrusive, so to speak, than formerly.

4. There is a real quantitative relation between certain characters. Many characters which appear to be units have arisen by accumulation, but the fact that

a character is obtrusive is not in itself a proof that it arose by accumulation.

Consider, for example, a structure such as a horn, a foot in length, growing out from the head of a mammalian animal. In terms of the definition it appears as a character because we know that it may be lost at a step, it may be absent without affecting the rest of the body. But there are several possibilities as to its origin. It may have arisen as a whole at one step, although we should be astonished at the occurrence if we had witnessed it. There is a second possibility. The race with horns a foot in length may have arisen at a step from a race with horns six inches in length. The sudden origin of a breed of cattle called Franquieros, having very large horns, from a race with smaller horns, is mentioned by some writers. It is therefore possible for the final condition to be reached by the accumulation of two increments, each a large fraction, a half of the whole, if we may speak of the final state as a whole. And there are of course further possibilities. The final state may have been reached by the accumulation of smaller fractions, *i.e.* by more numerous and shorter steps. But between each step the various races must have been in a state of stability, unless the animal kingdom of the past was different from that of to-day, when every individual is assignable to its group.

The several characters which no doubt took part in the building of the horn had, it seems to me, a quantitative relation to one another. We see quantitative characters affecting the total size of the organism or the size of a part in proportion to the whole.

5. Characters, more especially obtrusive ones, are used

by taxonomists as the identification marks of species. The taxonomist holds that every individual animal is of one species or of another. If this is true to-day it was no less true in prehistoric times. If it is false it is time some one exposed the fallacy. It is often ignored but never called in question. Some persons who are unacquainted with the subject regard taxonomy as a vague and unsatisfactory branch of biology. It is vague only because the word species has been brought into it, and various workers use the term differently. The characters are not vague, they are present or absent. If the principles of taxonomy are true, namely, that every individual belongs to a group and that each group has its group marks, how can the groups undergo change except by the addition and subtraction of the characters which are their marks?

6. If we examine a large number of animals in respect to a certain character which they have in common, we shall see that the character is present in various degrees in the different individuals. Those which possess it in a particular degree, the mean amount, are more numerous than others possessing it in greater or less degree. These others are fewer and fewer in number, according as they possess the character in a degree which is further and further removed from the mean amount, both above and below it. The name fluctuating variation was applied to this phenomenon by De Vries. So far as I understand, he regarded these departures from the mean as inheritable but ineffectual in evolution. As it may be expressed, they are ineffectual in the origin of species but effectual in evolution. Other writers, following Johannsen, speak of fluctuating variation as uninheritable, fortuitous occurrence

being regarded by them a special property of that variation which is of the body but not of the germ. If we are to take this view of the case, we must admit that in any population exhibiting fluctuating variation in respect to a certain character, there must be a considerable number of individuals which are capable of transmitting their particular divergence to their offspring. It is difficult to see clearly in such matters at present, but at any rate we may be sure that very minute variations may be inherited, while other more obvious ones are not transmitted. It also seems to be true that the great mass of variation within the species, the so-called individual variation, is due to the varied distribution among the species of characters such as may be called unobtrusive; characters which can never become obtrusive or specific, owing to their very nature such cannot become accumulated or built up into obtrusive or specific characters.

7. There are certain attributes in animals which appear to contrast with characters. It is necessary to qualify this statement at some length by the following illustration in which characters are brought into contrast with something which has been called "place variation."

There is a large but unknown number of rats of a certain kind living in the Eastern Himalayas and in Java, each possessing a peculiar combination of characters, peculiar in that the same combination does not occur in any other kind of animal. They are spoken of collectively as the species *Mus jerdoni*. If we wish to identify a rat of this species, we must see if it contains this combination of characters. The species may be described briefly by comparison with the common brown *Mus*

rattus shown in the frontispiece. In *jerdoni*, the fur of the whole lower surface is white; the tail is somewhat longer and has no pigment in the lower half of its circumference from root to tip. There are certain small peculiarities in the skull, the most noticeable of which is the smallness and flatness of the tympanic bulla. Another character, remarkable for its triviality, is an extra ridge on the palate. Most if not all of the *muridæ* have five ridges in the roof of the mouth lying between the molar teeth, each ridge is interrupted in the middle line. In rats of the species *jerdoni* there is a sixth ridge. The normal series of five is not disarranged, the extra one is interpolated between the two hindmost. In all probability this sixth ridge is constant in the species, at least it has been recognized both in Indian and Javanese specimens.

These are the most decided peculiarities of *jerdoni*, but as regards the general contour of the body, the size of the foot and ear in proportion to the size of the body, the peculiar arrangement of the pads on the soles of the feet and in many other ways, the species does not differ from *rattus*. *Mus jerdoni* is a good example of a morphological species, *i.e.* a group of animals each possessing a certain combination of characters, which are used as marks of identification by the naturalist. It is typical of thousands of other groups such as are called species.

Now let us consider some other group which is less distinct than the last-mentioned but yet is perceptibly a group. As an example, the rats of Poona, a town in the west of India, will be mentioned. They are slightly smaller than the common house rats of many other Indian towns

and their fur is slightly darker in tint. They were carefully compared with the rats of Belgaum, a city lying about two hundred miles to the south. The difference in the mean of the combined length of head and body was about half an inch, being less in the case of the rats at Poona. The difference in colour cannot be expressed in a satisfactory manner, but it is distinctly appreciable. If a large number of dead rats captured in both places were to be mingled together, it would be possible to sort them out with some confidence. Although a few mistakes might be made in sorting, yet it is undeniable that the general appearance of the group is slightly different in the two places. It is the rats of Poona which have departed, although to a very small extent, from that which may be called the Indian type of *M. rattus*, those of Belgaum may be considered as typical. If we could determine the mean length of the rats in several towns in a very accurate manner, we should find, no doubt, that in each case the results would be different from one another, some being further removed from the theoretical mean than others. It happens that the rats of Poona depart from the mean to such an extent that many people would notice and be sure of it, without mensural proof, while the differences exhibited by those of other towns, being smaller, would be overlooked. Now let us consider the group *jerdoni* in comparison with the rats of Poona taken as a group. It seems to me that the peculiarities of the former are of a different order from the slight peculiarities of the latter. Those of *jerdoni* appear to be characters, the kind of attribute which has been the subject of so much experiment in the last decade. On the other hand, the peculiarities of the rats

of Poona do not appear to be characters. The distinction is of course assumptive. The probable difference between the two kinds of attributes may be illustrated in the following manner. If all the rats of Poona were to be exterminated and the town restocked with rats from Belgaum, it is probable that after a few generations had passed the descendants of the new-comers would be rather small and dark like the old stock which had been exterminated, the change being due to some local condition. Peculiarities of this kind have been called "place variation," they certainly appear to be of a different order from characters such as those of *jerdoni*. Surely an extra palatal ridge must be something different from peculiarities of this kind. At present, however, we have no knowledge of the grounds of the difference.

It may be mentioned in passing that some taxonomists regard peculiarities, such as are exhibited by the rats of Poona, as specific. Having satisfied themselves that the animals of a certain area have a perceptible peculiarity, however slight, so long as it is visible and constant among the group, they apply a specific name to them. From a practical point of view species are those groups which have been described as such. If, then, using the term in this sense, we inquire into the origin of species it might be possible to explain the origin of some without explaining that of all. It might be possible to explain how the rats of Poona became small and dark but impossible to explain how *jerdoni* obtained an extra palatal ridge and a flat tympanic bulla.

Most of our defined species, however, are groups of animals, each possessing a particular combination of characters which appear to be as distinct as those of

jerdoni. Groups such as these will be dealt with in this book, but no limitation will be placed upon the number of the characters which are special to the various groups, they may be one or many; and no limit will be placed upon the number of the individuals in the groups.

Before leaving the subject of place variation, we will consider as a further example, the colour of the human skin. The amount of pigment in the skin of man increases steadily from northern latitudes towards the equator. In India, races which are reputed to have arrived after the aborigines are less dark than they. Human pigment therefore appears to be different in its mode of occurrence from that which is found in melanotic animals. The former appears in innumerable degrees and is dependent somehow upon the sun's rays. The latter does not occur in many degrees nor is it related to any known physical condition. This difference cannot be overlooked. The black sheep in every fold is proverbial, but human beings with dark brown skins are never born of white parents unless a coloured person was admitted into the line. One of the first and most convincing arguments in favour of the discontinuity of evolution is that species are discontinuous while environmental change, from place to place, is usually not so. There are gaps in the living series, but not in the environment, as a rule. But latitude and human pigmentation are an exception to this rule, as no doubt others have pointed out. There is no gap in latitude from the poles to the equator, nor is there a gap between the fairest white man and the blackest negro. There is, however, a sharp gap between the melanotic varieties and their

parent species. It is for attributes such as melanism that I use the term character, attributes that we may recognize as present or absent.

THE NUMERICAL CONSTITUTION OF GROUPS

Let us now return to our definition of the term group. A group is a number of individuals each possessing some particular character or characters which are the distinguishing marks of the group.

A group is a number. How large a number? Can two form a specific group? can ten? The answer would usually be given without hesitation in the negative. But what answer shall we give to the question—can a thousand individuals form a specific group? The answer, though hesitating perhaps, would be in the affirmative; for, if not, the number specified in the question might be raised until the question be answered in the affirmative.

If then there are a thousand organisms in proximity on a certain part of the earth's surface, each possessing a particular combination of characters which are not found in other animals, they will be collectively a specific group. Let us suppose that such a group is found in nature, its numerical strength being recorded at a certain moment, and again after an interval of several years. It may be found at the second census that the numerical strength of the group has increased a thousand-fold. This increase would not excite wonder, for it is well known that an expansion of the kind has occurred in the history of certain groups, and we may be sure that such might occur in many cases, provided that the interval of time between the two enumerations was sufficient,

and the conditions suitable. We are able to believe that a thousand animals of a kind may grow to be a million, but unable to believe that one may grow into a thousand. To explain the increase of a specific group after it has become established is considered easy, but to explain how the group became established is regarded as a different problem, which for many biologists has been solved by the theory of Natural Selection. It seems to be a tenet of the theory that there are two processes in the history of a species, that the origin is a process distinct from any subsequent expansion or contraction which it may subsequently undergo. Although, according to the theory, the action of selection never ceases, yet its visible effect on the species comes to an end as soon as the species has arrived. The date of arrival may be long ago, as in the extreme case of Lingula, an animal form which is common to-day in some part of the world, and has been preserved practically unchanged since the commencement of the record of fossils.

As opposed to this view, the following propositions will be made:—

1. There are not two processes in the history of a group, but only one, a process of expansion and contraction. The moment of arrival of a group, if it is to become a group, is when the first member of it is born.

2. All limitation as to the numbers of individuals composing a specific group should be removed from our conception of such a group. We place no limit on the greatness of the number, neither should we limit the smallness of the number.

3. The growth of a group is comparable to its extinction; just as the extinction of a group is brought about

by a process of diminution ending in unity and zero, so its growth is brought about by numerical increase from unity.

The term group is used here in preference to the term species, since, although it is no less conventional, it is far more definite. It cannot be made to vary as can the term species. In any discussion on the origin of species one is always brought face to face with difficulties arising from the varying meaning of the term species. In such discussions one must point to a certain so-called species X and attempt to demonstrate the history of its origin, but even if one succeeds in the demonstration, one's opponent may say, "After all, X is not a fair example of a species."

The term group, as used here, is not synonymous with the usual term species, although it seems evident that a large proportion of our defined species are groups, in this sense.

AN OBJECTION

The chief objection to these propositions is the supposed evil effect of inbreeding. They will appear absurd to any one who is convinced that a pair of animals or a single plant cannot give rise to a million or more descendants, because such an occurrence would be contrary to the laws of nature. What is the evidence for the ill effect of inbreeding? Most practical breeders are of the opinion that inbreeding is bad, but a few have considered that it is apparently harmless and the only method of raising pure stock. In considering the question we must remember that prejudice may influence our opinion, since for many centuries incest has been naturally

repulsive to mankind. Darwin, who gave the matter much attention and was inclined to believe in a law against inbreeding, considered that the available evidence " plainly shows that good follows from crossing, and less plainly that evil follows from close interbreeding." In these words there are two separate statements of opinion. From the context we see that the first statement is partially the outcome of the general experience that mongrel stock are large and vigorous, but chiefly the result of Darwin's own experiments, which proved that hermaphrodite plants produce more seed when they are the offspring of a crossed union than otherwise. It is evident however that Darwin's experiments were directed to show that the various cross-fertilizing mechanisms of plants were of utility, he did not consider it impossible for a single plant, which happened to set seed, to stock a large area with its descendants. This is plainly shown in the following words, which are his: " When a single plant of a new species is introduced into any country, if propagated by seed, many individuals will be raised, so that if the proper insects be present there will be crossing."

Now, as to the second statement of opinion which is contained in the second half of the first passage quoted: it is important for my argument to emphasize the fact that Darwin, after giving the matter much attention, thought that the evidence for evil following close inbreeding was not conclusive. It must therefore be admitted that it is uncertain whether inbreeding is harmful in all cases. On the other hand, there is evidence to prove that inbreeding has not been deterimental in certain cases. Rabbits, pigs, and goats, a few at a time, have been introduced into islands on many occasions and

their descendants have usually flourished. When we consider how a lone island became inhabited with animals it is enough to ask how a pair or two of the various kinds arrived there on the first occasion, we do not suppose that successive reimportations are necessary in order to counteract the evil effect of inbreeding.

The special question which will be raised in this book is how far inbreeding is detrimental to rodents. Fortunately this question has been answered by experiment. I am not acquainted with the original description of the experiments, but they have been summarized in the following words:—

"Ritzema-Boz inbred rats for thirty generations; for the first four years (twenty generations) there was almost no reduction of fertility, increase of mortality, and decrease of size." In these experiments of course all the descendants of the original pair were not preserved, they were far too many. But since inbreeding for twenty generations is harmless, we know that it is possible for a pair of rats to give rise to many millions of healthy and apparently fertile descendants.

A group, such as this, would occupy a considerable space in nature. There is no reason to believe that the group, having reached such dimensions, would inevitably deteriorate. In spite of artificial conditions in the experiment, the descendants of the pair reached the twentieth generation before deterioration commenced. This proves that a pair in nature is able to establish a large group, but the fact that deterioration eventually occurred does not prove that such a group would deteriorate in nature. Indeed, the history of rabbits in Australia and other similar cases afford evidence to the contrary.

It is well known that captive animals are frequently sterile. A stock which is fertile for a time may become infertile. It is usual to explain the fact as due to inbreeding, but the actual cause of the infertility is seldom known. Certainly there are other causes of infertility. For example, the fertility of captive animals often depends on space. Wild rats will seldom produce young when confined in small cages, but breed readily enough in a large enclosure. A pair of pigeons will produce no young in a small cage. The same pair, however, if removed to a large cage will breed, but the group of their descendants will cease to expand as soon as the cage becomes crowded, the numerical size of the group depending on the capacity of the enclosure. It is not suggested that the deterioration noticed by Ritzema-Boz was due to overcrowding but it is suggested that want of space is in itself a direct cause of infertility, that there must be many other unknown causes, and that one or other will intervene sooner or later in any line of inbred stock under artificial conditions. The fact that inbreeding was carried on for twenty generations before deterioration occurred is remarkable, but it merely proves that the event was possible. In another experiment under other unknown conditions, degeneration might have set in after the fourth or fifth generation ; this would not have proved that inbreeding is harmful to rats in general after the fifth generation. Wisemann, experimenting with mice, also noticed degeneration, but not until after the thirtieth generation. We may be sure that, sooner or later, deterioration will occur among inbred domestic stock, but we do not know that such is due to inbreeding in every case. The question depends on how far debilitated

inbred stock can be revived by the admission of new blood? There is little certain evidence on this question to help us. On the other hand, certain evidence is afforded by the Australian rabbits. Somehow or other those animals overcame the ill effect of inbreeding, supposing that they ever felt it. Their case is not unique. Darwin relates of a litter of tame rabbits, born on a ship, which were released on a certain island. The descendants of that single litter became so numerous as to cause the inhabitants to abandon the island. It is also certain that a pair of rats are able to give rise to a group of many million descendants. Is it correct to say that the members of such a group would still be practising inbreeding after the twentieth generation? In the ordinary sense, many of them would be distantly related to one another.

PHYSIOLOGICAL SPECIES

All bisexual animals which are mutually fertile and produce fertile offspring, taken together, constitute a physiological species. At present we do not know why one pair of animals is fertile and another sterile. We know, in a general way, that those which are nearly alike are as a rule mutually fertile, while those which are unlike are mutually sterile; but the likelihood of a pair being fertile is not directly proportional to their resemblance, in a strict sense. For most naturalists the test of a species lies in the mutual fertility of its members and their sterility with all other animals. A group which complies with this test is in the usual sense a real species. It is well known, however, that such groups are composite as a rule in respect to their characters. We never know

if a species, defined in the conventional manner, is compliant with the test or not, for we are never able to apply it strictly, *i.e.* in respect to every individual of the species.

Not until we know completely the physiological basis of fertility and sterility in the individual shall we know the origin of species in a strict sense, nor will our knowledge of the subject be complete until we can render a pair of fertile animals sterile with all others. But this question is apart from that of the early origin of morphological groups which is the subject of this book. It seems obvious that no group can remain distinct as a group, for any length of time, unless the members of it are sterile with all animals outside the group. In other words, the unknown facts of sterility would, if known, account for the permanence of groups, more than for their origin.

Only permanent groups should be considered worthy of a specific name. Discussions concerning the distribution of species are futile unless the term is restricted to groups that are permanent through countless generations. It will be seen that this book scarcely deals with the origin of species, using the term in this restricted sense.

THE TRUTH OF THE SELECTION THEORY

All biologists are agreed that the selection theory is broadly true; that gaps have appeared between the various forms of animal life because of the extinction of the intermediate forms which filled those gaps. For the moment, the origin of species may be regarded as the origin of the gaps between them, just as the origin of the hills in a range is the origin of the gaps between the

hills, the range having been formed from a continuous ridge. Species, as estimated by naturalists of the old school or by sportsmen, are things separated by wide gaps which are for the most part the results of the elimination of intermediate forms which occupied them, but the gaps between our modern species are very much narrower. We all see an immense difference between a horse and a fish, a great difference between a horse and a dog, a smaller but obvious difference between a horse and an ass, there is a difference between a chestnut and a black horse, and an appreciable difference even between two black horses. We speak of these degrees of difference as gaps of varying width, they are obvious to every one, although we cannot devise a unit to measure their width. In the wide gaps are placed many intermediate forms, some living, others extinct, but in the narrowest gaps there is no room for intermediates. We may be sure that there were extinct forms between the horse and ass, but we may well doubt that black and chestnut horses stand out as groups because of the elimination of intermediates.

The selection theory in its various aspects includes a number of propositions, some of which may be called broad statements, others narrow or precise. The following statements are examples of the two kinds :—

1 (broad). The gaps between the various forms of organism are the result of the elimination or extinction of intermediate forms.

2 (precise). Groups of like organisms cannot come into being as groups in any other manner than by the elimination of intermediate forms.

The first of these propositions is not in dispute, it is

accepted as true by almost all naturalists, but opinion concerning the second proposition is sharply divided. To return to our former illustration, the horse and ass are, broadly speaking, animal forms, the great gap between them was no doubt occupied by intermediate forms which have become extinct; this is not in dispute, but within the group indicated by the word horse, colour is a group distinction. There are chestnut and black horses, and each kind constitutes a group of like animals. It may well be doubted whether such groups are the result of the elimination of intermediate coloured forms. In fact the second or precise proposition appears to be untrue.

This particular example is used here merely to illustrate the grounds of dispute. The distinction between black and chestnut horses is scarcely typical of those distinctions which are commonly used to separate our modern species, by many it would be called a varietal distinction. However, when searching among the multitude of animal forms it is impossible at present to say with confidence that one is a variety, another a species. Whether there is an essential difference between them or not, each species or variety is a group of like organisms, and we wish to know how they become groups.

It is indisputable that the conception of a species has changed considerably of late years. It often happens that a species defined fifty years ago is redefined as a genus containing a large number of species. The horse and ass are typical of the older idea of species, but not of our modern species, the gaps between the latter are much smaller. Frequently they are inappreciable by the casual observer, often they are so small that it is difficult to imagine that they were ever occupied by

intermediate forms. Moreover, these small gaps remind us strongly of those larger differences which may occasionally be seen when comparing offspring and parent or when examining a sport along with the putative parental species.

In concluding this introduction two points will be emphasized. If it is true that our conception of a species has changed it is necessary to modify our opinion as to the origin of a species. Some of those who are interested in the origin of species do not know how small are the differential gaps which separate our modern species.

CHAPTER II

THE evidence described in the next few chapters is not the result of a definite inquiry into the manner of evolution. It arose unexpectedly in the course of another investigation, which was undertaken in order to increase our knowledge of the disease called plague. This disease, which has caused such wide-spread distress in India, is baneful alike to men and rats. It is known that the *bacillus*, the cause of the disease, thrives within the body of a warm-blooded animal. Apart from the body of an animal it is short-lived, except under artificial conditions. In this respect it differs from many other organisms of infectious disease. Because of the peculiar delicacy of the *Bacillus pestis*, it happens that rats and mice play an important part in the maintenance and dissemination of the disease. In order to complete this brief description of the natural history of plague, it may be mentioned that the *bacillus* is commonly conveyed from rat to rat and in all probability from rat to man by a blood-sucking insect—the flea. But this is a digression.

As soon as these facts were made known, principally by the work of the Plague Commission in India, it became evident that if the number of house rats were to be lessened, the disease would be reduced to some extent. Consequently, a campaign against these animals was

organized, under the direction of the Government of India. This was conducted not only in plague-stricken towns and villages, but also in places which had been free from the disease since the commencement of the present epidemic. These measures were of course in addition to the more usual methods of combating disease, such as segregation, quarantine and inoculation. Apart from their influence on the epidemic, the measures against rats afforded an opportunity of examining these animals from a general point of view. Any kind of knowledge was required, which might help in determining the best means of combating the disease. The writer was appointed for a period of eighteen months to carry on a general inquiry of this kind. The detailed results of the inquiry have already been published in the records of the Indian Museum. In their bearing upon sanitation they are not of direct importance, but during the progress of the inquiry some useful information concerning evolution was gained. It is necessary to emphasize that the investigation was made primarily in order to increase our knowledge of the natural history of disease. The facts relating to evolution forced themselves into notice in spite of this. The facts appear to be few and incomplete, but similar ones will seldom be obtained. The opportunity afforded by the exterminatory measures was most unusual, since these were conducted on a very large scale. It is unlikely that such action would have been taken solely in the interests of biology. Although the facts to be related have had a decided influence on my own opinion, yet they are too incomplete to be put forward as proving anything, *i.e.* supposing that propositions relating to evolution are capable of proof. Many doubtful

points would have been examined more carefully, if from the first the work had been undertaken in order to throw light on the manner of evolution.

The destruction of rats was carried out in many of the large towns throughout India and Burmah on a large scale, according as funds and other local conditions allowed. To mention one case; in Rangoon the municipal authorities paid daily to the townsfolk sums of money varying from £25 to £30, as rewards for the bodies of rats; indeed the whole system of extermination in that town must have cost the municipality as much as £40 daily, and this rate of expenditure was continued for over a year.

In July 1907, the Government of India issued a circular to the local sanitary departments in India and Burmah, inviting them to co-operate with the Indian Museum in studying the varieties and habits of the common rats of those countries, with a view to obtaining knowledge that might be useful against plague. This circular met with considerable response, so that within the following year over two thousand rats were received by the Museum from different places. It was my duty to examine these specimens at the Museum, and also to visit the towns of Rangoon, Madras, Bombay, Poona, Belgaum, where it was possible to examine the animals in large numbers and to inquire into their circumstances. It was while visiting these places that the more interesting information was obtained.

At the commencement of the investigation, when large numbers of specimens were being collected at the Museum from various places, the question arose as to how they should be classified. The great majority of

them were of the type which has long been known by the name *Mus rattus*. Rats of this kind form a heterogeneous group rather than a species in the modern sense. The term *Mus rattus* is applied to the house rats of India collectively, just as the term *Homo sapiens* is applied to Man.

Although obvious differences were to be seen, especially in coat colour, among the members of the collection, yet it was not possible to divide the group into distinct local races, except in the case of those received from hilly districts. For example, the rats received from both Sirinagar and Darjiling, which are among mountains, have each their own special peculiarities by which they could be recognized. The distinguishing features, however, do not extend to fundamental structures such as the skull. If a few skulls of rats from Darjiling or Sirinagar were to be mingled with a number of skulls of rats caught in any of the large towns of India, it would be impossible for any one to sort them out.

The question as to how the collection should be treated presented some difficulty. In accordance, with the usual procedure the specimens were to be classified according to their resemblances and differences, and the classes were to receive special names, old or new. This method of nomenclature could be applied to the few isolated groups. The rats of Sirinagar had already received the name of *Mus vicerex*, similarly the Darjiling rats might be called *Mus nitidus*. The difficulty arose in considering how the rats of the plains should be treated, since some of them exhibited peculiarities of colour as visible as those which are the marks of identification of certain species. If the name *Mus rattus* was

to be applied to the house rats of the plains as a whole, all these peculiarities ought to be disregarded, as being "individual variation." But these peculiarities had not that indefinite aspect which is commonly associated with the term "individual variation," on the contrary they were definite in appearance and sporadic in their mode of occurrence; that is to say, abnormal rats showing the same kind of abnormality were obtained along with normal ones from widely separate localities.

It is necessary to describe the species briefly, in a general manner, as follows :—

Mus rattus (in India).—The mean length of the head and body combined is about 175 mm., but it varies within wide limits from 150 to 200 mm. The mean length is of course not quite the same in every district. The length of the tail is greater than that of the head and body, being usually about 120 per cent. of the latter and varying from 110 per cent. to 130 per cent.

The appearance of the head is characteristic, because of the large size of the eyes and ears. The ears are so large that when laid forward they wholly or partially cover the eyes. The sole of the hind foot is decorated with six heart-shaped pads, arranged as in the diagram (Fig. 1). The form of the skull affords perhaps the most certain means of identification. If the skull of an English *rattus* be mingled with a number of skulls taken from Indian rats of the same species, it would not be possible to distinguish it from them by any natural mark. The same may be said of many local races of the *rattus* group which have received specific names, although those species have been defined as possessing types of skull peculiarly their own. It is true that many

local groups of rats have slight peculiarities of skull formation, such as are seen especially in the nasal bones, the form of the tympanic bones or the length of the series of molar teeth: but even as few as one hundred skulls of rats caught in different parts of a large town will usually comprise individuals showing similar peculiarities.

The species is most variable in colour. Three of the principal types are shown in the frontispiece, which was executed by an Indian artist from animals captured in Calcutta. That on the left of the plate, which will be referred to as the whole-coloured brown type, is by far the commonest kind in India. It is, however, by no means a pure type. In some individuals red predominates. Others are yellowish. Some are darker than others.

The white-bellied type, shown in the middle of the plate, is comparatively common; its distribution in India will be described later on. The black type, seen on the right, occurs but rarely in the interior of India, although it is not rare in seaports and is common on ships.

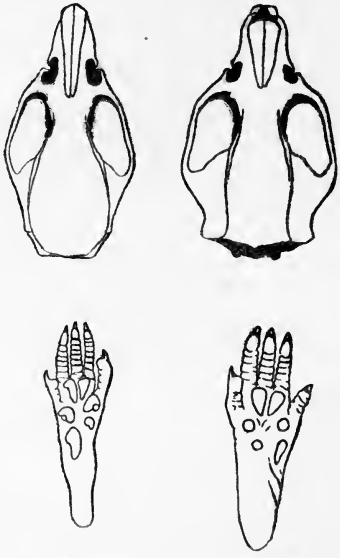
According to report the black type was common in England until displaced by the short-tailed *Mus norvegicus*, but it is difficult to obtain satisfactory information on the subject. At all events the species which is commonly referred to as the black rat seems to be uncommon in England at the present day. Of two specimens in the Indian Museum, which were sent from the British Museum for comparison, one was black, the other sombre brown, somewhat darker than the common Indian type, otherwise these English specimens were indistinguishable

from our Indian ones. Some rats received from an Australian port were also of the same type. The species we are dealing with seems to be of world-wide distribution: its three most striking varieties are shown in the frontispiece. It is an interesting fact that on one occasion all three varieties were found together on a ship. Since the mass of parasitic rodents in India are of this kind, the term house rat will be applied to them.

The species *Mus norvegicus*, which is better known as a house rat in Europe, is common also in some Indian seaports, but it was not met with in the interior. It was found to be constant in type, and therefore it will not be further dealt with here; the following class must, however, be mentioned:—

Gunomys sp.—There is in India a kind of rodent, which is found burrowing in fields after the manner of a mole, and is consequently known as the mole-rat. In taxonomic literature this animal has appeared under various names. In the "Fauna of British India" it is named *Nesokia bengalensis*. Since 1907, however, it appears in the genus *Gunomys*, which was defined by Mr. Oldfield Thomas. From the taxonomist's point of view, *Gunomys* is a composite group which has not yet been resolved into its component species. Although mole-rats are rodents of about the same size as *M. rattus*, they differ from that species as regards all their superficial characteristics. The length of the head and body, in the class, varies from about 150 to 220 mm. The length of the tail is much less than that of the head and body, being on the average about 80 per cent. of it. The mole-rat is more stoutly built than the house rat. Certain differences between

the two are satisfactorily shown in Fig. 1, which represents the upper surface of the skull and the sole of the hind foot of both kinds. The fur is bristly and dark greyish brown in colour, it is less variable in respect of the latter



M. rattus. *G. bengalensis.*

FIG. 1. × 0.

quality than is the fur of the house rat. Albino, yellow, and black varieties were found but rarely. White-bellied ones were never found.

The zoologist wishes to answer the following questions. What is the present state of the animal kingdom or any defined portion of it at the moment? How did the whole

or part come to be in that state? We are dealing here with the portion or group defined as *Mus rattus*, and we must inquire as to the state of the group at the moment.

First, let us consider a few numerical facts. The number of house rats in India may be estimated roughly at a thousand millions. It could not be less than this. The estimation is based on the assumption that on the average there are about three rats for every person in any house or village. The house rats of India, then, are a large number at the moment, and if we wish to know how many have been born in the last ten years we must multiply that number by fifty or more. But what is our warrant for knowing the state of this mass at the moment or during the last ten years? It is this. Before the year 1905, perhaps as many as a thousand Indian house rats had been examined in the scientific manner. Since that year, several thousands have been examined or looked at; but it is evident that the amount of our observation upon the mass is trivial in comparison with the size of the mass. The amount observed is a minute fraction of that not observed, but yet we must form a mental picture of the mass as it is, before we inquire how it came to be.

A specific name for the mass and for every member of it is merely a restful convention. In applying the term *rattus* to the Indian house rat as a whole, we make the tacit assumption that all of them are alike, or rather, we agree to ignore any points of difference that may be detected among them. For certain purposes this convention is sufficient and useful, but it is not a true description of what can be seen.

We may examine the mass either by taking a few individuals from many widely separate places or by examining a large number of them at a few places; but, however we proceed, we shall find that its component members are not all alike. They present certain differences. These differences are not "in all directions," supposing for the moment that we can imagine things chaotic. On the contrary, they are in certain directions, the same kind of difference being found again and again in various places.

We will now discuss and enumerate the differences. They are of colour for the most part. This is because such differences are easily appreciated by our senses. We cannot say with confidence of any two rats—"the one is brown and the other black, and this is the only difference between them." The one may be fertile, pugnacious, and healthy; the other sterile, timid, and susceptible to certain diseases. We choose characteristics of colour for discussion because they happen to be obvious to our senses, but such characters are merely a few chosen from among a large and unknown number. We cannot measure the importance of characters by their degree of visibility. Even if we confine our attention to colour, the same point of view should be taken. An observer may say of three rats, "This one is black, but those two are alike, they are both brown." But a second observer may say of the same three animals, "To be sure, the black one is quite different from the others, but the brown ones are not alike, there is a reddish tint in the fur of one which is absent in the other; moreover it is possible to see the same characteristic both in the parents and offspring of that individual." The contention of the

second more attentive observer may be indisputable. Let us suppose that he is right. The red tint must be due to some material influence which is present in every hair of the animal. One cannot say more or less than this in regard to the blackness, although the latter constitutes a difference which is obvious at a glance. One cannot say that the redness is trivial and of no importance, but it is permissible to choose deliberately the black element for observation and to neglect others that are less visible. This is the method adopted here. The characters selected because of their visibility are as follows :—

(1) *Melanism.*—Some rats are black, like the one shown in the frontispiece. Among these, some have no trace of brown in their fur ; black, iridescent green, and slaty blue being the only appreciable tints. Others, though appearing to be black, yet have a few brownish hairs in their fur. Two litters raised in captivity from a cross between a thorough black and a brown were of this mixed kind. Both the pure and mixed black are found wild.

(2) *Albiventralism.*—Some rats are pure white on the ventral surface. In the great majority the fur of the under side is coloured, each hair being grey with a light brown tip. Rats were received however, from many parts of India, in which the fur covering the belly, breast, throat and lower jaw, as well as the inner side of the limbs, was pure white. This whiteness was sharply marked off from the coloured sides, as in many other vertebrate animals.

(3) *Caudal albinism.*—As a rule the tail of a rat is deeply pigmented from base to tip. The pigment, which

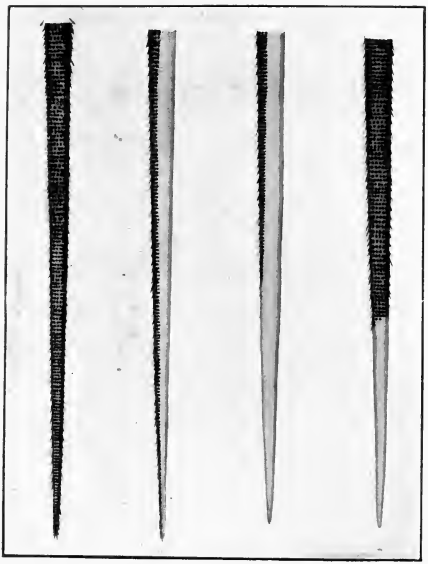


FIG. 2. × ½: side view.

[*To face p.* 47.

is situated in the tile-like scales and short hairs, is present equally both on the upper and lower surfaces, Fig. 2*a*. Departures in more than one direction from this type, however, occur; they will be described as follows :—

(*a*) (*Bi-coloured type, Fig.* 2*b*).—Pigment may be absent from the lower half of the circumference of the tail, from base to apex. This condition is found in several established species of *Mus* which inhabit mountainous districts. The only well-known example in India is the rat of Kashmir, *Mus vicerex*. The character also occurs in smaller local groups in other parts of the Himalayas. Judging from the distribution of this character, we may be sure that every rat possessing it is not of one and the same line of descent. The character has appeared on many occasions, almost always in mountainous regions. A single individual with this peculiarity was, however, found in Rangoon.

(*b*) (*Semi-bicoloured type, Fig.* 2*c*).—In this type, as in the last, there is no pigment in the lower surface, but the pigmentation of the upper surface does not extend the whole length of the tail; it comes to an end at a point somewhere about the middle. Rats possessing this peculiarity have been recorded from places so far removed from one another as Tenasserim, Manipur, and Naini Tal. Wherever found they have been called *Mus berdmorei;* although, in my opinion, there is no reason for supposing that their direct relationship is any closer than in the case of others, which do not possess a special feature in common.

(*c*) (*The parti-coloured type, Fig.* 2*d*).—In this type there is total loss of pigment in the whole circumference

of the tail in the terminal portion of its length. This is seen in a species which occurs in the hills of Madras (*M. blanfordi*), and also in other species outside India. In *blanfordi* the caudal albinism is accompanied by a considerable lengthening of the hair of the tail so that it forms a terminal brush-like tuft. Rats showing this particular type of caudal albinism were found near Amritzar in the Punjab and near Bombay. In those from the former place there is no lengthening of the caudal hair, while in that from the latter place this peculiarity is well marked. It is almost certain that these peculiar rats were not part of an established species, but belonged to a limited family group; in the case at Amritzar this was satisfactorily proved, as will be explained later on.

Another type may be mentioned in which the tip of the tail is white. This is found in rats of various species. Sometimes it is common among rats caught in a particular area.

(4) *The breast stripe.*—Rats are occasionally found which differ from the normal whole-coloured ones in possessing a short white line in the middle of the breast between the forelegs. There is another kind of breast stripe which in a sense is the converse of the last mentioned, some white-bellied rats have a median coloured stripe in the same position.

(5) *The forehead star.*—This peculiarity, well known among mammals, has not been used, to my knowledge, as a specific mark in the genus Mus. It was found on three occasions among house rats in India, in a mature pair caught together in the Amritzar district, in a group of four which were caught together in

the same trap at Calicut and in a single individual at Poona.

This short list of peculiarities includes the most obvious ones which were met with. They have been selected because they have a certain resemblance to those which are the distinguishing marks of certain established species, known to taxonomists.

Other peculiarities were noticed, but as they are less like specific marks, they are of less importance for the argument. Among these may be mentioned, white rats and true albinos, yellow rats and tailless ones, such as would be under an obvious disadvantage. Having mentioned that rats showing these peculiarities are to be found in India—they doubtless occur in other countries —we must now consider how they are distributed among the common kind.

The representation is as follows :—Rats with these peculiarities are distributed in groups of varying magnitude among the normal mass; by magnitude is meant numerical size, which may vary from two or three to fifty or more. These groups will be referred to as family groups. In regard to them the following, more or less assumptive, statements will be made :—

(1) All the members of a group possess some obvious feature or features which mark them off sharply from their neighbours, the normal multitude.

(2) The members of the group occupy a circumscribed area, in the case of rats they occupy a single house or a group of adjacent houses or a particular set of burrows in a field.

(3) A family group may contain from two to fifty or more members, but we cannot draw a line between a

single individual and the small family group it may establish, nor can we draw a line between a large family group and a small race.

(4) Such groups are common ; they are mostly unknown to us as such, because they can only be appreciated by examining a large proportion of the animals belonging to one species spread over a large area. Whenever, as frequently happens, members of such groups come into the collectors' hands they go to swell the number of the so-called rare species, such as are found once, but never again.

(5) The peculiarity which marks off a group from the normal multitude is not always special to that group. The same peculiarity may appear in various groups which are found in separate areas : this is true, however, only in the case of groups whose peculiarity appears as a single character unit. Those groups whose peculiarity is made up of several uncorrelated characters arise on one occasion only. This statement is assumptive, the facts relating to it will be found in Chapter V.

(6) Family groups arise from individuals by in-breeding.

The representation of Indian house rats is then, a multitude of normal animals spread over the continent, amongst which such family groups are disseminated. Since at one time we may find a group containing perhaps five members, a second group containing ten, a third with fifty or more, we shall understand that groups containing a large number of individuals must have passed through stages exemplified by the smaller groups, in other words they must have grown from unity.

If this representation is a true picture of the present state of the house rats as a whole, it indicates the method of the origin of a race. For example, it is evident that a race of black rats may arise among a multitude of the brown kind without the destruction of a large number of brownish black intermediates.

Before going further we must define certain terms. The words normal and abnormal are used here to mean that which is in the majority and that which is in the minority. It seems that the Mutation Theory invites us to reconstruct our ideas concerning normality and abnormality. If we regard an organism as a complex of character units we can no longer regard the normal rat or the normal man as a reality in the sense of Quetelet.

The terms normal and abnormal have a definite meaning in relation to a single measurable character of an individual, but no meaning in relation to the individual as a whole. An organism possessing a character A, in quantity equal to the mean value of A, as determined in the species collectively, is normal as regards that character. All other members of the species, which possess A in greater or less measure, are abnormal in that respect. But if in addition to determining A, we ascertain the mean values of other characters B, C, D, in the same species, and with them compare those particular characters as they are to be seen in a single individual, we shall find that whereas that individual may be normal as regards A it is more or less abnormal in B, C, D. In other words we shall not be able to find a normal individual.

Not long ago, I wrote as follows: "We can only

speak of a typical individual when we are dealing with one measurable feature at a time." Professor Karl Pearson, commenting adversely on these words, upheld, as I thought, the contrary opinion, that the normal man is a reality.* Either I misunderstood him or he has since changed his view, for he has recently given his opinion as follows: ". . . to define the normal person. There is no such person."† It seems that the "reality" of the normal man is no longer defensible.

Being much impressed with the truth of the Mutation Theory I regard a species as a number of animals each possessing characters A, B, C, etc. Any animal having these characters is a normal member of the species. An individual who has one or more characters in addition to A, B, C, etc., may be called an abnormal member of that species, but only for convenience in order to express the fact that it is in the minority.

Further, it seems that every extensive group of living things appears to be polymorphic, if it be examined closely enough. That is to say, some members of the group will possess characters which are absent from others. We can see dimly that this is true even of mankind. The chief quality of man, that makes him man, so to speak, is his mind; but this attribute does not appear to be one thing, present in varying degrees. On the contrary, the mental endowment of an individual appears to be a number of attributes, some of which, being present in almost all, may be called normal, others, being present in few, are abnormal. Though the human mind in general is so complex that any representation of it must be dim, yet it seems possible to perceive that

* "Biometrica," 1911. † *Daily Mail*, May 20, 1912.

certain mental qualities are present in some individuals but not in others. Such peculiarities, considered for the moment as entities apart from the individuals which contain them, appear to arise spontaneously or inexplicably on various occasions, that is to say in the manner implied in the words " by mutation."

CHAPTER III

In the last chapter the house rats of India were represented as a multitude of normal animals among which were distributed small groups of abnormal animals, which probably arose through inbreeding, and it was pointed out that if these family groups were of variable numerical size, containing from two to fifty or more members, we should have reason to believe that the larger groups had once been as the smaller groups in number. It now remains to describe some of the family groups met with.

The first example is of a group of melanotic rats of the species *Gunomys bengalensis* found at Rangoon. The members of this group were not only peculiar in their black colour but also in being of large size and in possessing a peculiar form of skull.

It is necessary to describe how the rats were obtained in Rangoon. At the time of my visit about 4000 rodents were being caught daily. Most of them were brought in by the townsfolk for the sake of a reward. They were delivered dead at certain collecting stations, twice a day, at specified times.

At Rangoon five established species of rodents are common in houses. These may be enumerated as follows :—

1. *Mus concolor* is the commonest and may be described briefly and adequately as a miniature *rattus*. It

is like the larger and better known species in every respect, except that the mean length of the head and body is only 120 mm.

2. A white-bellied race of the *rattus* type which cannot be placed appropriately under any of the established specific names. It is unnecessary for our present purpose to give it a name.

3. *Mus norvegicus*, the common grey rat of England is found here as in most ports.

4. *Gunomys bengalensis*, as found in Bengal and described on page 42.

5. *Gunomys varius*, considerably larger than any of the foregoing; a bristly, formidable animal, called bandicoot locally. These animals would be placed in the genus *Bandicoota* if they happened to possess the large feet peculiar to that genus, but they agree very closely with the definition of *Gunomys varius* given by Mr. Thomas. This question will be referred to later on, when describing a certain sport derived from this race which happened to possess large feet and therefore appeared to be a bandicoot.

The different kinds were delivered in a promiscuous manner, but as a rule those brought by one man were all of one species. By questioning the collectors it was possible to ascertain generally the kind of environment which each species preferred to live in, but it was often difficult to obtain satisfactory information concerning the circumstances of any particular batch of rats which might be brought to the collecting stations. In spite of the unreliability of the information it was evident that any one collector usually brought rats of one particular species, because, as a rule, they had all been captured in

the same building. This being so, one would expect to observe some interesting facts when the attention was concentrated upon the members of one species in particular, for similarly we should expect to find that those lesser peculiarities, commonly spoken of as variation within the species, would also be perceptible in particular groups which were living and breeding together. This was undoubtedly the case, as may be illustrated by the following example. Melanotic specimens of *Mus concolor* are not common. It was possible to examine a thousand or more animals of this species without finding one black one among them; however, on the few occasions when black rats of this species were to be seen at the collecting stations, they were brought, two or three at a time, by one and the same collector. Observations of this kind were frequently made. It was evidently a common occurrence for rats of a particular house to possess certain special features, such as uncommon size, unusual length of ear, of foot, tail, or whisker, a reddish or yellowish tint in the fur, a tint which may be spoken of as darker than usual, or to be pure black. This state of things might be expected in any community where inbreeding was rife. Observations of this kind, though interesting at the moment, are not of value for our demonstration, which is to show how a new group of animals each possessing some obvious and special characters of their own can arise among an old group of animals which did not possess those characters. In order to demonstrate this, it is necessary to know the history of certain groups in detail. An opportunity for making precise observations of this kind arose in the following manner. Although most of the rats were

obtained from the townsfolk, a small proportion of them, between one and two hundred daily, were obtained in a more systematic manner for the sake of sanitary research. A number of men were employed by the municipality to set baited traps in special houses overnight. The traps were examined next morning and those containing rats, usually to the number of a hundred or more, were brought to the collecting stations. Each trap was labelled with the address of the shop or house from which it had been taken. One morning I noticed that some of these traps contained rats of a kind which I had not seen before, either in Rangoon or in any other place; they were pure black specimens of the mole-rat *Gunomys bengalensis*. Among those persons who had been employed at the collecting stations for many months and who had witnessed during that time the arrival of many thousands of rats from all parts of the city were some who said that they had seen similar animals on rare occasions, but nevertheless the occurrence was regarded by all present with that interest which is aroused by an uncommon event. On the first occasion, five of these peculiar rats were brought in, all had been captured in traps set on the ground floor of a certain shop (No. 65, Maung Khyine Street). The attention of the trappers was especially directed towards that neighbourhood. Traps were set in the adjoining houses, with the result that on the following night one more of these peculiar rats was obtained from the house No. 65, and three precisely similar ones from the next house, No. 66. Again on the third night one more was caught at No. 66. After this no more were caught, although trapping was continued. Ten rats in all were caught in those two adjoining houses.

All of them possessed the same peculiarities. No other kind of rat entered the traps set in those particular houses during the time. There can be no doubt that the ten rats represented only a part of the total number which infested those houses. It is scarcely possible to clear a building by trapping. In any house several rats may be caught on the first night and perhaps one or two subsequently, but after such losses their comrades become wary and avoid the traps, at least for a time. In all probability there were as many as thirty rats in those two houses. The fact that the ten were remarkably alike, each possessing definite peculiarities by which it could be recognized at a glance from the other rats of the town makes it almost certain that if the whole group had been captured and examined all would have been found alike. The subsequent capture some months later of two more of these peculiar animals, both showing the same special characters, confirmed this opinion.

Although it may seem unfair to assume that this family group contained as many as thirty members judging solely from the fact that ten of them were captured, yet those who know the extent to which houses in tropical cities are infested with rodents will consider that the assumption is moderate. The particular houses in question were of a kind very favourable for rats. The partition walls were composed of double layers of wooden planking separated by a space two or three inches wide. The floors were of broad tiles laid upon the earth. Circumstances of this kind are very suitable for rats, especially for those of a species which is well known for its burrowing powers.

There is supporting evidence in the fact that those

which were captured were all mature or nearly so. Rats breed all the year round, producing about four generations in that time. Consequently we may be sure that there were a number of young ones of various ages remaining in the houses.

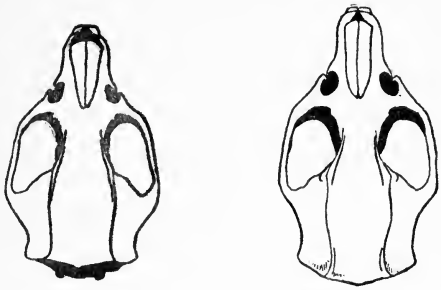
We have now arrived at the following conclusion. There were living at the time of the inquiry (1908) in two adjoining houses at Rangoon, a considerable number of rats possessing certain special characters by which they could be recognized unmistakably from other rats; that is to say, as a group they fulfilled the conditions which are commonly required of a "species," and it is highly probable that if four or five of them had been sent together to a museum they would have been regarded as a new species or variety. Indeed this group has a better claim to be commemorated by a name than some of the species which have been defined in recent years.

The special characters common to the members of this group, are as follows. In the first place, melanism. By this peculiarity alone they could be distinguished at a glance from the common mole-rats, which are greyish brown. The two kinds appear strikingly different even at a distance of several yards. The group was melanic in the strictest sense, not a brown hair was visible in any of them. Black and slatey blue were the only colours appreciable.

If it had not been for their colour, doubtless their other peculiarities would have been overlooked, since they are less noticeable, being, so to speak, within the range of variability of the species (*i.e.* of the established or parent species). These peculiarities are as follows. The skull is narrower and the nasal bones are

longer than usual. The skull of a member of this family group is shown in Fig. 3, together with a skull chosen as representing the type of the parent species.

This cranial type, however, was not an exclusive possession of the group. The same type was occasionally found in other places. It appears to be correlated with large size. Whenever the head and body of a mole-rat measures 200 mm. or more the skull is narrow in proportion to its length, this is true without exception of



G. bengalensis. Melanic sport.

FIG. 3.

specimens collected from various parts of India. A large and relatively broad skull was never found. The adult members of the group were of uncommon size, being in that respect near to the highest limit found in the species at large, hence, as usual, their skulls were narrow.

Let us re-state the case. I assume that there were a group of at least thirty rats living in two adjacent houses and possessing certain special characters, namely melanism and a narrow type of skull, which seems to be associated with bodily largeness.

The assumption is based on the following observed facts. Ten rats of this special kind were captured in the same place at one time. The ten were pure black in colour. The skulls of four of them were examined and found to be of the special type. The circumstances did not allow of a fuller investigation than this, but the assumption became justified later on. It was desirable to collect more facts about the group, consequently it was arranged that any further specimens obtained should be sent to me. Although the same rate of destruction was maintained, it was not until nearly six months later that two others were obtained. They were caught in the neighbourhood of Maung Khyine Street, but the actual house from which they were taken was unknown. Both showed clearly the characters which are the peculiar property of the Maung Khyine group. Hence we may be sure that they were derivatives of that group. If, as might have been the case, they had been black, but not large and with a narrow skull, they would not have been derivatives of the Maung Khyine group, but of some other group of independent origin.

How, now, did this family group arise ? The progenitors of the group were either born in Rangoon of normal brown parents or they are migrants from without. In my opinion there is no other explanation, for we cannot suppose that effectual selection for melanosis occurred in and around two houses only, among all the houses of Rangoon. Hence those who believe in gradual evolution must suppose that the progenitors of the group were migrants from without. Their position is unassailable, for it can never be proved that there is no such race in any part of the world, from which migration by some

means or other might not have occurred. But it is most improbable that the progenitors of the colony arrived from without. They could not have arrived on a ship from a foreign port. Rats of the species *G. bengalensis* have long been known to be common in the fields throughout India. In 1907 it was pointed out that these animals might also be found in houses, and that they were common in some of the larger towns. But the mole-rat is a burrowing animal, and has never been found on a ship. The presence of a group of melanic animals of the species *rattus* or *norvegicus* in Rangoon might be accounted for by supposing that they arose from ship-borne emigrants, and in such a case if we wished to inquire as to the whereabouts of the main body from which the emigrants were derived, we should have any part of the world to choose from. But since in this case we are able to exclude the possibility of marine transit the problem of the origin of the group becomes narrower. The progenitors of the group were either born of the common brown mole-rats in Rangoon or they arrived there from some inland part of Burmah. But it seems impossible that there should be, unknown to us, an established race of black mole-rats in Burmah, for since plague broke out the attention of many persons has been directed towards rodents, and the particular animals referred to are of a most noticeable kind.

Now let us consider the probability of the group having been derived suddenly from brown parents in Rangoon. There is a great difficulty in all such cases. We know that races or groups arise in some way but we cannot know of them until they have arisen, and when they have arisen it is already too late to observe how they

arose. But we know of certain events which will help us to form an opinion. It is known that a pair of normally coloured panthers may produce normal offspring and black offspring in the same litter. The black sheep in every fold is proverbial. Many other animals are known to produce black offspring in this sudden manner, and it is also well known that such sports when mated together produce offspring of their own kind.

My argument is this. Black animals arise in some way from brown animals. The method of their origin has been observed in certain cases. Whenever it has been observed, the whole difference between brown and black has appeared in a single step. The parent has been brown, the offspring has been black. Hence we actually know that black animals take origin suddenly. Why, then, is it necessary to believe that black animals usually take origin by another method? It is unwise to look at the same kind of event on many occasions, to perceive that the method of it is the same on each occasion, and yet to believe that the method observed does not indicate the true method. Why should we think that visible events are exceptional and unreal and prefer to believe in the reality of some process which has never been seen?

I imagine that the history of the group was as follows. A large black mole-rat was born of grey-brown parents of normal size. Even though it mated with one of the common kind we know, from Mendel's discovery, that some of its grand-offspring would resemble their grand-parent in body and in germ. These, by inbreeding, founded the colony. Such a colony could not become established unless it were isolated by some means, either

by migration or by the fact that its members were sterile with all animals that were not of the colony. There is, however, no reason why they should not become a species in a certain sense, for if the whole ten had been sent to a museum with a note to the effect that they had been caught in such and such a district, they would have been called a species, sub-species, variety, or race according to choice, even though there were no others like them in the world.

Melanism is sometimes regarded as a peculiar phenomenon, as different from other heritable variation, but it seems that the difference lies only in the fact that melanism is very conspicuous in our eyes, hence it has often been noticed. The following chapters will show that other conspicuous colour characters occur in the same way as melanism.

Before leaving the subject of melanism I will quote a passage from a paper by Messrs. Clarke and Hamilton (*The Zoologist*, 1891), which has an interesting bearing on the question.

"The following note by the Rev. P. A. Keating of Athlone appeared in the *Field* in 1893: it gives a very good account of a peculiar and as yet unexplained phenomenon in the history of the Irish black rat.

"'Black Rat in County Wexford.

"'During a short visit to Co. Wexford early last October I was informed that a large colony of black rats suddenly put in an appearance in that county. I repaired with a friend to the locality with no small degree of curiosity. We quietly entered a field of oaten stubble,

in which some stacks of corn were yet standing. About fifteen yards from these stacks ran a dyke separating the field from the adjoining farm. In this dyke, which was regularly honeycombed with them, the strange visitors had taken up their abode. In less than an hour I counted over forty of them running out and in from burrow to stubble. Where they came from so suddenly, and in such numbers, is a mystery to me, as I have been intimately acquainted with the place for over thirty years, and a black rat was never heard of in the locality, in fact the country folk viewed them with serious apprehension of some impending calamity.' "

Messrs. Clarke and Hamilton then continue. " We cannot offer any explanation of the above phenomenon, but we may state that similar occurrences have been reported from other localities. . . . On the continent of Europe the only instance of the occurrence of black varieties of *M. decumanus* known to us is one recorded by Milne Edwards (Ann. Sc. Nat. 1871), a large group of black *decumanus* have been known in the menagerie of the Museum."

Groups of melanic rats, then, are not very rare. The accounts of them, however, are likely to be disregarded, because the attendant circumstances are, in most cases, vaguely known. They produce wonder rather than conviction. If in any place there is a firm local belief that the rats of the locality are brown it will cause local astonishment when a group of black ones appears in the district, but the occurrence will not cause conviction in one who has not the local belief.

From a general point of view local hearsay is not to be trusted. It should be noticed that in the case of the

group at Rangoon I have not put faith in hearsay. The circumstances of the normal mole-rats of the district were definitely known owing to the fact that about four thousand rodents of all kinds were being captured daily. The abnormal group stands out distinctly among the normal ones. The value of the case lies wholly in the fact that the condition of the normal mass was actually known.

CHAPTER IV

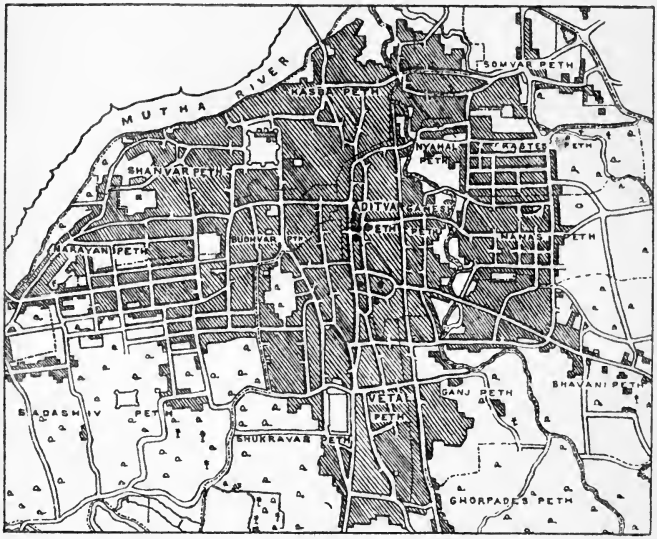
THE next example to be presented is of a group of rats occupying six adjacent houses in Poona city which were obviously different from the common rats of that place. The case is interesting because the fate of the group was ascertained. In Rangoon we were dealing with at least five distinct species of rodents, but in Poona there is only one, the common *Mus rattus*. In Rangoon, rats were being obtained in greater numbers than at Poona, but at the latter place their capture was accomplished in a systematic manner, under the direction of a member of the Plague Commission. The chief purpose was to obtain knowledge of plague, whereas in Rangoon the aim was to lessen or stamp out the disease by the destruction of as many rats as possible. However, the experiments in Poona were on no small scale, for the records of the Commission show that in the interval between the 26th May, 1908 and the 22nd May of the following year, 45,487 rats were captured in that city. They were caught in the following manner: Every night a large number of traps were set in certain houses of the town, each trap was labelled with the address of the house in which it was placed. Next morning the traps were examined and those containing rats, in all to the number of a hundred or more, were taken to the laboratory. Each rat then became the subject of various observations, which were recorded in

a serial register. The points observed were those that might help the Commission in its task, such as the number of fleas on each rat, the pathological condition, the state of pregnancy, etc. For the biologist, it is fortunate that any peculiarity of the outward appearance of the rats, as well as the place of residence of each of them, were included among the records. It is evident that the peculiarities which would be noted under these conditions would be of an obvious kind. Minute details of proportion and tint, such as are often the subject of inquiry among zoologists, would in all probability be overlooked by observers whose chief interest lay in other directions. The peculiarities which were noticed and recorded were indeed of a kind such as could be seen at a glance and from a considerable distance. This is well, since the common conception of a species is of something distinct at a glance.

The house rats of Poona as a class have no special peculiarities. They are, on the average, slightly smaller and darker than the rats of Bombay and other cities. They are of the whole-coloured brown type, and are less variable than those of Bombay city, where the black and the white-bellied varieties are comparatively common. At Poona, among all the rats caught during the year, there was not a single black one, and there were only nineteen of the white-bellied variety. Of these nineteen, three were caught while I was at Poona. This was not due to chance, but to the fact that traps were specially set at my request in those houses from which white-bellied rats had previously been captured.

The white-bellied rats of Poona are exactly like the common ones, except for the one peculiar character

which renders them conspicuous. So conspicuous are they among the others that it was the custom of the Plague Commissioners to refer to them by the name of *Mus alexandrinus*, as a nominal distinction from the common *Mus rattus*. This is in accordance with the



MAP OF POONA CITY.

The group of houses mentioned in the text is indicated by dots in the centre of the map.

modern use of the word—"species," two animals which appear different from one another at a glance being usually considered as of different species. The name *M. alexandrinus*, as used in this particular case, might, however, be regarded by a systematist as incorrect; it

is, therefore, preferable to use the term white-bellied kind. The map on page 69 shows how far this variety was established in the city of Poona; it must be remembered that forty-five thousand whole-coloured rats have been taken from the city at large, and that there is scarcely a house which has not contributed to the total. The nineteen white-bellied rats were caught in nine houses; four of these are contiguous and two others are separated from them by the width of a street. Six of the houses therefore form a distinct focus of habitation for rats of this special variety. The other three houses form another centre, perhaps more than one, which is situated about 250 yards further south.

The following table is taken from the register, and shows the order in which the rats were captured. No steps were taken to ascertain the distribution of the white-bellied rats, but nevertheless the facts concerning their whereabouts came to light of their own accord, for the register shows that whenever during the year a white-bellied rat was captured it was out of a house belonging to the group indicated on the map.

	House.	White-bellied rats.
Raiwar	380	3
„	338	2
„	963	2
„	1146	1
„	1191	2
„	553	2
„	381	2
„	382	3
„	379	2
		19

The focus which is represented by six houses, 379–382, 553, and 963, Raiwar, contributed fourteen rats, but, as

mentioned before in dealing with the other group, we cannot, from this number, estimate the numerical strength of the colony, although we may feel sure that it contained considerably more than fourteen.

In the case of the group described in the last chapter it was almost certain that the members of it held the two houses for themselves. If ten rats of a particular kind are captured during a few successive days in certain houses, none of any other kind being caught at the same time, the probability is great that no other kinds were present in those houses. But at Poona the white-bellied rats did not occupy the houses to the exclusion of the common kind, but the fact that there was a group of them in those houses is clear.

We must now consider how this state of affairs came about. In my opinion there are only two possibilities. The progenitors of the colony were born in the city of normal parents, that is to say as mutants, or they are migrants of another race which arrived there from without. A third view might perhaps be held, although few will consider it tenable, namely, that this small group of white-bellied rats was derived from the common rats in the city of Poona by Natural Selection working upon rats possessing various degrees of ventral coloration, this selective process having occurred in and around the place where the white-bellied rats were discovered in Poona. This view may be disregarded, however, since the more plausible explanation of the facts from the Darwinian standpoint would be that the group was established by migrants from some other place.

The alternative explanations are, therefore, as follows :—

1. A race of white-bellied rats became established in some place by selection from among rats possessing all degrees of ventral coloration. This selection came about because, under the conditions appertaining to that place, white-bellied rats had a better chance of life than others, better even than those with very light grey or almost white under-parts. Under these peculiar conditions, the difference between white and very nearly white under-parts must have been of life-saving value to the rats. From out of a race established in this manner at least one pair wandered, or was conveyed, into the heart of Poona city, where it founded a colony.

2. A pair of whole-coloured rats living in Poona produced one or more white-bellied offspring, which were the progenitors of the colony.

One feels convinced that the group arose in the manner described in paragraph 2. An expression of conviction is not a proof, but proof in any case seems to be impossible. Even if the birth of every member of the colony and of their progenitors had been witnessed and every step in their descent traced back to the whole-coloured ancestors, the experiment could not be repeated for a proof. The purpose of exposition is to convince other people, this can only be accomplished fairly by setting forth for the reader's approval the various steps which led to the conclusion.

It is highly improbable that the progenitors of the colony should have arrived in the city from without, even if white-bellied rats were common in the country around Poona. It is clear from the evidence that the houses of the town are infested with whole-coloured rats. It is difficult to imagine the supposed intruders passing

house after house until they came to rest in the centre of the city and commenced to breed. If we supposed that they arrived there in a corn sack, or by any other means of transport, the difficulty is not lessened, for we do not know whencesoever they can have come originally. The relative distribution of the whole-coloured and white-bellied kinds is known in India and lends no support whatever to the theory of migration. If we knew that there was one extensive area inhabited by the white-bellied kind we could imagine that an occasional isolated body of them, occurring elsewhere among the whole-coloured multitude, was a result of migration. But, on the contrary, it is known that the common rat in India is whole coloured and that the white-bellied variety occurs in groups here and there. This was found by Captain Davys, I.M.S., in the Amritzar district, in which over twenty-two thousand rats were captured in sixty-nine villages. Of these villages, sixty-six contained whole-coloured rats only, while three villages contained rats of both kinds. About ten per cent. of those caught in each of the three villages were of the white-bellied kind. The three villages were not situated close to one another. If we consider this case alone there is perhaps small reason to believe that the origin of the three groups was independent in each case, but when considered in conjunction with the case at Poona and with the facts now to be related concerning the distribution of white-bellied rats throughout India in general, it seems certain that the various groups, must have arisen independently, on separate occasions, in different places.

The facts concerning the distribution of the white-bellied rats throughout India were obtained in the

following manner. Numerous persons in India were requested to send specimens of the common rats of their district. It is evident that such a request may be answered in different ways. One correspondent, living in a town where rats are not being destroyed as a sanitary precaution, will probably set a few traps and send the first two or three animals that are caught in them. If these should happen to be of the white-bellied variety, we cannot assume that all the rats of the district are like them. Another correspondent in a plague-stricken district, seeing hundreds of rats daily, will probably make a selection to illustrate the colour varieties known to him, but the relative numbers of the kinds sent will not necessarily be in proportion to the numbers of the kinds actually present in the district. These facts should be borne in mind in perusing the following table. In most cases when the number sent is more than ten, the rats are from a place where there was opportunity for selection from among large numbers. The records from Belgaum, Poona, and Dacca were furnished by members of the Plague Commission.

	Total sent.	Whole-coloured.	White-bellied.
Rawalpindi	14	12	2
Allahabad	182	169	13
Cawnpore	28	19	9
Hardwar	50	50	0
Ballia	1	1	0
Gonda	16	16	0
Ghazipur	7	5	2
Azamgahr	4	4	0
Saharanpur	2	2	0
Moradabad	4	4	0
Bulandshir	5	5	0
Dehra Dun	4	4	0
Lucknow	10	10	0
Rai Barelli	14	14	0

	Total sent.	Whole-coloured.	White-bellied.
Nowgong	81	81	0
Buldani	22	22	0
Rewar, Barwani, and Dhar	21	21	0
Neemuch	4	4	0
Sambalpur	2	0	2
Belgaum	39,460	39,460	0
Poona	45,487	45,469	18
Dacca	5704	5109	595
Madras City	600	600	0
Chingleput	10	10	0
Salem	4	4	0
Gogalpore	1	0	1
Bellary	2	2	0
Ootacamund	1	0	1
Trivandrum	7	0	7
Cochin	4	4	0
Tellicherri	48	10	38
Mangalore	13	11	2
Chitur	14	14	0

In considering these figures attention should be paid to the relative position of the places. For example, Mangalore, Tellicherri, Cochin, and Trivandrum all lie on the west coast of Madras, the first named being the farthest north and the others lying further and further to the south in the order named. Both kinds were obtained from Mangalore, the whole-coloured being in excess, both kinds were also obtained from Tellicherri, the white-bellied being in excess. From Cochin come only the whole-coloured and from Trivandrum only the white-bellied.

The figures are deceptive in one way, they do not illustrate sufficiently the very large preponderance of the whole-coloured kind throughout India in general. The experience of the observer at Belgaum was the common experience in many other places. Several persons engaged in the extermination did not know that white-bellied rats were to be found in India, although in their

particular districts many thousands of rodents had come under their notice. In the above list only those places

Reference Map of India.

where reliable observations were made, have been mentioned; it must be admitted that in proportion to the whole of India they represent a small quantity, but the

fact is plain that the white-bellied kind are scattered among the others; they have, so to speak, no head quarters.

Let us now inquire into the experience of others who have examined large numbers of Indian rats. Several independent observers have recently described what they saw among these animals, while examining them in the routine of plague investigation. Their opinions are of particular value in this discussion, for they are unbiassed. The question of the origin of races did not concern them. It was sufficient for their purpose to distinguish the true physiological species; but, fortunately, the most obvious peculiarities of the animals within these groups were also mentioned. The experience of the Plague Commissioners in Bombay, which is published in the *Journal of Hygiene* for 1907, was based on the examination of several hundreds of animals brought from all parts of the city daily during more than a year. The two principal species are the well-known *rattus* and *decumanus*, which were obtained in the proportion of about seven of the former to three of the latter. In regard to the former we read in the above-mentioned journal, "*Mus rattus* is frequently called the black rat, but the commonest type in Bombay is a brown variety. The black variety is somewhat rarely seen in rats taken from the city, though it occurs more commonly amongst rats trapped from ships in Bombay harbour. The belly is invariably of a lighter colour than the back. Rarely the belly is unusually light coloured; rats with this peculiarity are considered by some workers as a variety (*Mus alexandrinus*) but no distinction was made by us on this account." Judging from the words, "rarely the belly is unusually light coloured, etc.," it is evident

that the writer found these unusually light-bellied animals distinguishable from the others as a class. It was unnecessary for the purposes of plague investigation to take into account the varieties of *Mus rattus*, consequently the proportionate numbers of the unusually light-bellied ones to the common kind was not recorded. During my visit to Bombay I was able to satisfy myself that those spoken of as " unusually light bellied " were white bellied when cleaned, and that they comprised less than one per cent. of the rodent population. The number of rats which were being caught at the time was about five hundred daily. Although some days might pass without the capture of a single white-bellied rat, whenever animals of this kind were obtained they were brought in, two or three at a time from one and the same house. This peculiarity in their occurrence was noticed again and again, in various parts of India.

Now let us see what the Commission found in the Punjaub. I have already mentioned the experience of Captain Davys in the Amritzar district, his observations were made independently of the Commission, at a later date. The Commission selected two villages, named Dhand and Kassel, for their investigations. During a year they obtained 7525 rats which are described as follows: " The rats taken in the Punjaub villages were all of one species named *Mus rattus* . . . the colour of the dorsal fur is usually brown, while the belly is greyish, dirty yellow, or occasionally quite white." Here also is clearly expressed the opinion that the white-bellied ones are a recognizable class distinct from the common ones. In Bombay some of the rats were described as unusually light bellied, while in the Punjaub they are

said to be quite white in this respect. The smoke grime, which clings everywhere in a large manufacturing town such as Bombay, explains this slight discrepancy.

From time to time systematic zoologists have turned their attention towards the rats of India; they are unanimous in their opinion that these animals are difficult to classify. The more experienced workers have generally taken a broad view of the subject, recognizing that the group *Mus rattus* is characterized in particular by the skull, the length of the tail and ears and the pattern on the soles of the feet. Having defined the group by these salient features they have described a great range of variability within the species especially in respect of coat colour. Mr. W. F. Blanford describes *Mus rattus* in "The Fauna of British India" as follows: "Usually brown, more or less rufous or occasionally yellowish brown; more rarely blackish brown or black, below generally white, frequently sullied sometimes brown or grey and occasionally with a white, fulvous, or grey median band."

Any one who has had experience of the subject will know that these words are well chosen to describe the various colours which occur. It was no doubt the purpose of the writer to mention in as few words as possible any variety that might be found. A reader might sum up this description hastily in the following manner. "Roughly speaking, the colour of these animals may be anything." He might suppose that the coat colour of rats afforded an excellent example of variation in all directions, scattering, fortuitous, according as one or other term fitted his conception of the manner of variation. But he would be wrong, I think, in coming to this

conclusion, for the number of distinct colour types is large, but not indefinitely so. The black type, the reddish-brown the yellowish-brown, the pure white-bellied, the white-bellied with a median brown stripe and others are recognizable and may be found again and again in various localities. There is little doubt that by isolating pairs from these several types, we could obtain pure strains from them in many cases, since we often find in nature litters of young rats all of which are of one and the same colour type. In his description of the colour types that may be met with, Blanford is accurate; but in using the words "sometimes," "generally," "occasionally," he shows uncertainty as to the frequency of occurrence of the various types. Thus he writes, "Below generally white . . . sometimes brown or grey," but we have since learnt from the large number of rats which have been caught in India recently, that the white-bellied type is very much less common than the other.

Blanford's account, though partly the result of his own experience, is based on the work of Mr. Oldfield Thomas, which was published in the Proceedings of the Zoological Society for 1881, and is to-day the most adequate account of Indian rats from the taxonomist's point of view. This account was based on a large number of specimens sent from India. Mr. Thomas refers to the common house rats of India by the name *Mus alexandrinus*, and in regard to the use of the term writes as follows: "*M. alexandrinus* would seem to be a more tropical form of *M. rattus*; but as it seems always to have much shorter, coarser, and more rufous hair as compared with the black and shining fur of *M. rattus*, I have provisionally kept them distinct, though I have little doubt that they will

have finally to be considered as but one species." In regard to the colour of the common Indian rat, he says, " Colour varying from dark rufous-grey to bright reddish fulvous, belly sometimes quite white, sometimes no lighter than the back." He makes no statement as to which kind is the commoner.

Hence taxonomists hold that the rodents common in Indian houses constitute a species, the members of which may either be whole-coloured or white-bellied. That is to say, in their opinion either kind may give birth to the other, and they may interbreed.

In some parts of India the white-bellied rats predominate in number over the others. This is the case at Tellicherri. The forty-eight specimens from that place were sent on four occasions, but on each occasion the number of the white-bellied kind was in excess of the other. There can be no doubt that the former predominates in Tellicherri.

So far we have been considering only animals caught in the main body of the Peninsula and have not mentioned those of the Himalayas, the well-known range of mountains which form the northern boundary of India. The rats of several towns situated among these mountains were investigated. Nearly all from Sirinagar, Simla, and Nainital are white bellied. It is necessary to say nearly all, for out of about fifty specimens from each of these places there were exceptions in each case. There is no certain relation between altitude and colour, for the rats of mountainous regions, such as Darjiling, Khatmundu, and Ootacamund are whole coloured; conversely, white-bellied rats are established in some lowland districts, as, for example, in Tellicherri and Rangoon.

This account of the white-bellied rats in India may be closed with a description of the fate of the group in Poona. The facts were ascertained by Captain Kundart, I.M.S., to whom I am much indebted. The existence of the group was made known between the months of May, 1908 and 1909. At the time of my visit to Poona, in February, 1909, the presence of the group in the particular houses was already known; traps were set on that occasion, with the result that three more white-bellied rats were caught in the same houses. In February, 1911, the group was again sought for. Between the 2nd and 11th of that month, two hundred traps were set in the same houses at night; seventy-two rats were caught in all, but only one of them was white bellied, the others were whole coloured. Hence in less than two years the group had almost died out. No doubt this is the usual fate of such groups, unless they are isolated by some means, but it would be a gratuitous assumption to say that it is their invariable fate. The method of origin of the white-bellied kind from the others seems plain enough, and wherever we find this kind persisting, as, for example, at Tellicherri, we should account for their origin by that method rather than by a method which has never been demonstrated.

CHAPTER V

THE next case to be presented is in one respect of less weight than the foregoing, because the number of individuals exemplifying the group is only two, but it is of interest since the peculiarity in virtue of which the two specimens are marked off from the normal multitude appears as a number of separate characters. They have therefore that relationship to one another which is commonly regarded as the relationship between members of a species, and they are related to the rest of the animal kingdom as the members of a species are related to all animals that are not of that species.

The nature of the normal multitude among which the two abnormal ones were found is satisfactorily known. Reference has already been made to investigations which were carried out in the Amritzar district of the Punjaub. There the character of the rat population was ascertained in sixty-two villages. In only three of these villages were white-bellied rats found, and at one of them, Nowshera Dhala by name, nine hundred rats in all were captured, including the two sports now to be described.

They both possess the following peculiarities. The complete character of albiventralism, *i.e.* the lower jaw, throat, breast, belly, and the inside of the limbs is covered with pure white fur, which is sharply marked off from the coloured fur of the sides. Nowshera Dhala is

one of the three villages where white-bellied rats were obtained; but these two specimens have, in addition to albiventralism, other peculiar characters. They both have a white patch or "star" in the middle of the forehead and the terminal third of the length of the tail is pure white. Besides this, the general colour of the fur is of a peculiar light hue which is perhaps identical with that known as "silver fawn" in mice. Wild rats of this peculiar tint are rare, but they have been seen on other occasions. This remarkable pair, for they are male and female, are nearly of the same size. In this respect both are well below the mean body weight as calculated in the rats of the district. They weighed 85 and 95 grams, the mean being 140 grams. The measurements which provide us with this information were made immediately after the death of the animals, as a routine, by one who did not know why the measurements were required.

The interest of the case lies wholly in the similarity between the two specimens. It is fortunate that two of them were captured. If only one had been obtained we could have learnt nothing from it, it must have been regarded as a freak and passed over. But the fact that there were two of them, as like one another as it is possible for two organisms to be, arouses the attention and suggests that they are part of a species. On sending them, their finder gave it as his opinion that they were a new species, and suggested the name *Mus brahminicus*, on account of the white mark on the forehead. If a number of them had been sent together to a museum, unaccompanied by information as to their circumstances, they would have been regarded doubtfully as a new species, but if others had been sent at a later date the

doubt would have been removed. However, their isolated position among the rats of the district is against their being commemorated by a name.

As already mentioned, these sports were captured in the village of Nowshera Dhala, where nine hundred other rats were obtained and twenty-two thousand normal rats were caught in the surrounding villages. There may be a few more of the kind still living in the neighbourhood, but as the couple stand out alone among so many of the normal kind we may feel sure that they do not belong to an established species but to a family group such as we have seen at Rangoon and Poona.

We know the nature of the rodent population of the district and we know that the couple were caught together in a house of a certain village but we have no other information; we do not know if rats of the common kind were also living in that house.

The pair were male and female, the latter being pregnant and bearing four early embryos. It is probable that the four young ones, if they had been born, would have resembled their parents.

Special stress has been laid on the fact that these two sports each possess a particular combination of four distinct characters which marks them off from the normal multitude. It may be asked, what are your reasons for speaking of the characters as things distinct in the constitution of the animals? To this the reply is—Because those characters are found separately in other sports which have been captured in other places. For example, Firstly, albiventralism appears frequently as a single character among Indian rats, a fact which has been dealt with at length in the last chapter. Secondly, the

forehead star was found on two other occasions, in a single sport caught at Poona and in four young ones which were captured together in a trap at Calicut. None of these were white bellied. The first mentioned had a special peculiarity in possessing white feet, and in the other four the extreme tip of the tail was white, but this latter peculiarity is quite common among otherwise normal rats. Thirdly, a sport was captured near Bombay in which the last two inches of the tail were white; a similar, though not identically similar, character has been used as a specific mark among various rodents and other mammals. Fourthly, the peculiar general colour of the couple was seen on several other occasions, but it is perhaps less distinctive than the other three characters.

These are reasons for believing that the four characters are independent things in the constitution of rats, but how came it that all four were combined in both of these peculiar individuals. The reason seems obvious. They were of one line of descent in the narrowest sense, being in all probability offspring of the same parents. There is then evidence to show that these four characters are appearing separately on many occasions in separate places, but there is no evidence to show that the four characters are appearing, in association, in separate places. It may be said that the four characters which are special to these two sports are not such as are used by taxonomists to distinguish species. But it happens that two of them have been used for this very purpose. Thus albiventralism is characteristic of several defined species of the *rattus* group which have certain other special characters besides. Loss of pigment in the whole circumference of tail in the terminal portion of its length

is also characteristic of several species. There is a group of such rats in the Philippine and neighbouring islands, which is spoken of collectively as the *Xanthurus* group. They have received the specific names, *xanthurus*, *everetti*, *meyeri*, *celebensis*, *macleari*, and *luzonicus*. If the tail of one of the sports shown in Fig. 2 *d* be compared with that of *Mus macleari* (illustrated in the proceedings of the Zoological Society for 1887), little difference will be found between them. In both cases there is complete loss of pigment from the whole circumference of the tail in the terminal quarter of its length and the change from the coloured to the colourless portions is abrupt, so that whereas one of the tile-like scales may contain the full amount of pigment, another in contact with it may be devoid of pigment. If we are to believe that the parti-coloured tail which is characteristic of the *Xanthurus* group is a definite character, being represented both in the germ and in the body of all animals of the group, we can believe no less in the case of the two sports from Amritzar. I have had the opportunity of comparing the sports side by side with specimens of *M. macleari*. It cannot be said that the character is exactly the same in both. Thus, in the sports the white area invades the pigmented area in an irregular manner on the ventral surface but not on the dorsal surface or on the sides; but in *macleari* the change is sudden round the whole circumference of the tail. Although, as regards this character, the sports are slightly different from the species (*macleari*) yet both the sports are alike; the white area invades the pigmented area in an irregular manner on the ventral surface in both of them. We may say, therefore, that the sports do not possess exactly the

same character of caudal pigmentation as the species, but this is not a proof that the character in the sports is different in nature from a specific character, for we can find another established species with a parti-coloured tail in which the pigment is distributed in a manner unlike that seen either in *macleari* or in the sports. Take, for example, *Mus blanfordi*, which occurs among the hills of Southern India. This species is closely allied to the common *rattus*. Certain minor points have been described as characteristic of its skull, but in specimens of the species which are available for examination in the Indian Museum, these points are not constant. *M. blanfordi* is a white-bellied rat with white feet, its principal mark of identification lies in the tail, which is white as regards the terminal half in all its circumference. The tail of *blanfordi* is peculiar, the tip being tufted with hair, but if we overlook this additional character for the moment and regard the pigmentation only, we shall see that it is different from either of the others, for in this species the colourless portion invades the coloured portion in an irregular manner *on all sides*, so that small areas of pigmented scales are completely isolated from the main pigmented area.

Let us summarize the facts.

(1) Two sports were found together, each possessed the same four peculiar characters. All four characters are known to occur separately in other sports. Therefore they are not under any compulsion to appear in association with one another.

(2) The fact that all four appear together in a single individual sport may therefore be spoken of as accidental, the cause being unknown. But the fact that all four

characters appear in two individuals is not due to accident but to community of descent. This can hardly be disputed.

(3) The four characters are similar to the marks of species. Two of them have been used as marks of identification, in certain cases.

From the view point of the Mutation Theory we regard an animal as a combination of characters, and the species question may be stated thus—how is it that a number of animals have the same combination of characters ? This aspect of the problem is well illustrated by the following words—

" Every taxonomic description testifies to the fact that a certain set of characteristics is usually found associated in each species or variety. The prevailing theory has been that this association is a necessary one, maintained because all the characters are necessary to the success of the species in its relation to the external environment, or else that they were physiologically dependent. Modern work in hybridizing is establishing the fact that few of the specific characters are interdependent. Their association is, so far as interaction goes, mostly accidental. Thus, in my experiments with poultry, I have reached the same conclusions as have been reached by Johannsen, De Vries, and indeed by all recent workers. I find, namely, that of the scores of evident external characteristics of poultry that are inherited in alternative fashion scarcely two can be found that are always associated. The most striking exception is the association of high nostril and absence of single comb. (C.B. Davenport, " The Inheritance of Poultry.")

Many other examples of correlation are known. To mention one recently described, Mr. H. M. Leake working

among Indian cottons found that the size and colour of the petals were correlated. The petals were either large or small, they were either yellow or white. The large petals were yellow, the small ones were white. In over a hundred thousand plants there was no exception to this rule. A small yellow or a large white flower could not be found.

But, as Davenport points out, this correlation of characters is exceptional, whereas we see the association of uncorrelated characters in every part of the animal and vegetable kingdom. But how can we explain this association? The case afforded by these two sports is very simple. The occurrence of the four characters in both must have been due to their community of descent from an individual, and this explanation may be extended to larger groups which are marked by a number of associated but uncorrelated characters and to groups such as are often called species.

It may be said that this is building on slender foundations; but the key to complex problems is often found in simple instances.

There seems to be an important difference between the occurrence of many-charactered groups, such as the one described in this chapter, and single-charactered groups such as were mentioned in the last chapter. The former seem to arise on one occasion only, whereas the latter arise on many occasions. For example, if other individuals, having exactly the same four characters as the two sports under discussion, were to be found in the Amritzar district, we might be sure that they were derivatives of the same family group as the others, but we can never be sure of the place of origin of a sport

whose peculiarity consists of only one character, such as albiventralism.

Species, recognized by the taxonomist, are, as a rule, marked by the possession of many characters; hence the taxonomists' belief, that each species has arisen on one occasion only, appears to be well founded.

CHAPTER VI

THE cases described hitherto show that small groups of abnormal animals or sports occur in nature. Although the members of a group are few as a rule, *the groups are many*. This important fact is likely to be overlooked, unless one realizes the smallness of our observations in proportion to the immensity of the thing to be observed. We must imagine the frequency of these groups in relation to the mass of rodents in India as a whole. The portion of the mass examined is very small, but a considerable number of such groups were found in it. We may, therefore, be sure that although family groups are small in membership and usually temporary in duration, yet they are of frequent occurrence.

The ideal method of research would have been to have determined the number of abnormal family groups per million of the rodent population. This, however, was not possible; records such as were kept at Poona were exceptional. But it is probable that the number of groups per million would be not less than ten, and we must multiply the number by a thousand if we wish to estimate the number of such groups in India at the moment and by a much larger number to find how many have been in existence, on and off, during the last century.

But we do not know the actual frequency of these family groups, and it would hardly be possible to estimate

it, for the following reason : Some groups, such as those mentioned, are marked off by decided characters, but others are less clearly marked, so that the question would often arise in reference to a particular group, " Ought this to be included in our estimate ? " We might indeed make the number appear large or small according to our personal inclination. The same difficulty is often felt by the taxonomist in regard to certain groups. The same question arises, " Ought they to be regarded as species or overlooked ? "

Since the object of this book is to suggest rather than to prove, little is to be gained by describing many cases, but three others will be mentioned in this chapter as illustrating further the occurrence of characters and groups.

The first of these illustrates the occurrence of a distinct character which was often found singly, and on one occasion in a group.

The presence of a white line on the breast between the fore-legs is not uncommon in rats and other mammals. The fact first came to notice in Madras city, where on one occasion, two out of six hundred house rats were marked in this manner. The mark was not quite alike in both, being longer in one, so as to extend to the abdomen. There was no reason for supposing that both had been taken from the same house, although they might have been. We may be sure that they, or a near ancestor marked in a similar fashion, were born from normal parents. No other explanation of the occurrence seems possible. There is no doubt that the animals marked in this fashion arose in some way from the normal stock, since they differ only in the one character.

There is no reason to believe that the change came gradually. Suppose, for example, that there are a thousand white hairs in the breast patch of such a marked animal, there is no reason to believe that the number became increased by small increments from one to a thousand in a successive series of ancestors. The breast mark is usually about an inch long and a quarter as broad in the adult and occupies the middle line of the breast between the fore-legs, not always symmetrically. Rats marked in this manner were found in various parts of India, for example, Madras, Calcutta, Bombay, Poona, Nagpur.

The mark may be smaller or larger than usual, it may be a small spot or a long line two inches in length. If we could obtain some hundreds of individuals marked in this manner we should find, no doubt, that the mark showed fluctuating variation as regards size; but it is evident that the character is appearing independently in places remote from one another and when it appears it is more likely to appear in what may be called the usual amount, than in any other amount greater or less.

When the occurrence of this breast mark was first noticed, the fact did not seem to be of much interest. The mark evidently appeared on various occasions, but there was no evidence to show that it was transmissible in full measure from parent to offspring; nor did it seem likely that such a character would appear in every member of a large group so as to constitute a racial mark.

Later on, however, evidence was obtained that rats breed true in this respect, and it appears that the mark was made use of as a specific character in the case of the Irish rat *Mus hibernicus*.

At Poona, four young rats were caught together in a

trap. All, being in the same stage of adolescence, were doubtless of the same litter. Each was marked with a white line on the breast. This shows that all the young produced at a birth may be marked in this manner. Such a litter, if placed upon an island, would probably stock it with their descendants, all possessing the mark. We know, however, that it is scarcely possible for such a group to establish itself in India, since none was found, whereas single sports bearing the mark are quite common. At least one per thousand of Indian rats are marked in this manner, that is to say about a million of them are born yearly in various places, but yet they have not become established in any place so far as we know.

In the Proceedings of the Zoological Society for 1837 Thompson described the species *M. hibernicus*. The chief characters chosen by him as the marks of this species are as follows : short tail and ears, black coat colour with a white breast mark. There is a drawing of an animal of this species in the *Zoologist*, September, 1889, which shows plainly that the breast mark is like that noticed among the sports in India.

Messrs. Clarke and Hamilton have since shown that *M. hibernicus* is a melanotic variety of the common *M. norvegicus*. They examined fifty-six specimens from various parts of Ireland and found that the breast mark was present in thirteen only of them, hence it is not a constant specific mark. There is evidently no correlation between melanism and the breast mark. The first alone or both may occur in addition to the other characters of *M. norvegicus* in Ireland.

We will now pass on to another case.

During my visit to Rangoon I heard of some peculiar

rats which had been seen occasionally at one of the collecting stations. They were said to be very large and yellowish in colour. A brood of five such animals had been brought in recently by a rat catcher. As it was the custom at the collecting stations to reject white rats and other fancy kinds which might have been bred for the sake of the reward, these five had not been purchased for destruction, although their captor declared that they had been found wild in the usual manner. The man, who was well known at the station, was questioned as to their disposal. Fortunately two of them were still in his possession. Being reared artificially from an early age they were quite tame: thus they came into my hands. They were quite young, being as was subsequently ascertained less than a half their mature size. At first sight it was difficult to know what they were. They evidently belonged to one of the larger species, for their heads were already as large as that of a mature *rattus*. The tail was shorter than the head and body, so evidently they were of the species *Mus decumanus* or one of the species of *Gunomys*, since these were the only short-tailed kinds known in Rangoon. Both showed two striking peculiarities, namely yellowish coloured fur and large hind feet. The fur covering the whole body was much the colour of tow. The hind feet were of remarkable size, it was this fact that made their relationship so doubtful. The feet of many mammals are proportionately larger in adolescence than in maturity, but although the couple were evidently immature their feet were already as large as those of any species of rodent known in Rangoon.

A few days later, a large yellowish rat was brought in dead to that collecting station at which the other two had

been received. The circumstances of its capture were not ascertained. The length of the hind foot of this animal recorded at the time was 49 mm., *i.e.* about a centimetre longer than the mean foot length of any other species of rodents known in Rangoon. Except for its colour and the disproportionate size of its feet it resembled the larger species of mole-rat, *G. varius*, which was common in the district. It resembles that species in total size, in tail length, and in the form of its skull. There is little doubt

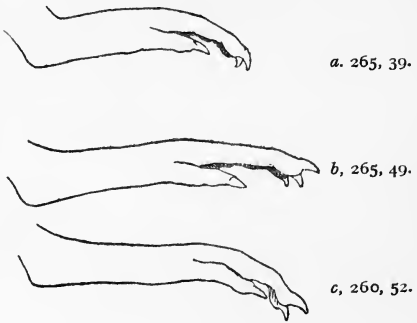


FIG. 4. × 0.

that there was a group of these curious animals in that part of the town, nor is there much doubt that their progenitors were born as sports from normal members of the species *Gunomys varius*. Unfortunately the group was not located.

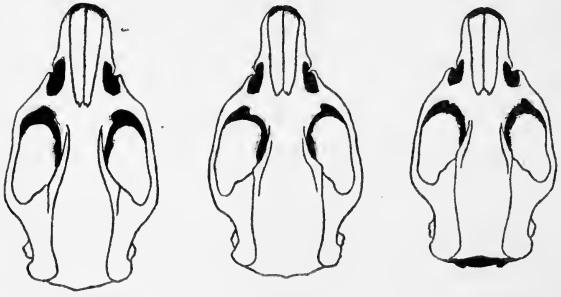
The large size of the feet is interesting, for in this respect the sports resemble the allied genus *Bandicota*. In Fig. 4 are seen drawings of a hind foot of (*a*) a normal *Gunomys varius* of Rangoon, (*b*) of the sport under discussion, and (*c*) a *Bandicota nemorivaga* caught in Calcutta.

The drawings are of the natural size but were made from dried specimens. The accompanying figures represent the measurements of the length of the head and body together and of the hind foot in the same individuals.

It is interesting to note from the taxonomic point of view that the single character of foot length seems to be enough to justify the placing of these sports in the genus *Bandicota*. Both *Gunomys varius* and *Bandicota nemorivaga* are large bristly rats of much the same size and appearance, having much the same proportionate length of tail and quality of fur. The proportionate size of the hind feet is the most striking difference between them, but in this respect the sports resemble *Bandicota*. They could not have been derived from that genus, for it does not occur in Rangoon. Moreover, the skull type of the sports is nearer to *Gunomys* than to *Bandicota*. This can be seen in the illustration, Fig. 5, which shows the three types. An important point of resemblance between the sport and *Gunomys*, in which they both differ from *Bandicota* is the distance between two boney points projecting from the junction of the front and inner margin of the orbits. In *Bandicota* these two points are much closer together. As regards the hinder part of the skull, however, the sport is more like *Bandicota*, since in both the occipital surface is vertical, so that the condyles cannot be seen when the skull is viewed directly from above. It may seem to those unacquainted with taxonomic zoology that such points are too small to be of much significance, but it is just such points as these that are used by taxonomists as generic and specific characters, and we wish to know how it is that such

insignificant features are constant in immense numbers of animals, for so they are often found to be.

The skull of the sport is therefore unlike that of either species, but of the two it is more like *Gunomys*, from which no doubt it was derived. Unfortunately we cannot speak of the skull type of the sports, since only one skull was examined. The peculiar coat colour and the large feet were present in each of the three available individuals, and there is little doubt that the skull type, if examined in



B. nemorivaga. Sport. *G. varius.*

FIG. 5. × ⅔.

the three, would also have been found constant, as in the case of the group at Maung Khyine Street.

The two young rats belonging to the litter of five were kept in captivity in Rangoon in order that they might grow up. Unfortunately they met with an accident, so that their measurements in maturity were not obtained. It was of course important to compare their adult foot length with that of the other mature specimen obtained. There is, however, little doubt that they were of the same lineage as this other. The disproportionate size of their

feet was more striking than their unusual colour, for the same tint may be seen occasionally in other sports and species.

Appearances are more or less surprising according as they are more or less rare. This particular colour is not very rare, it has been seen occasionally among rats of other species. The following cases may be mentioned. Two mole-rats (*G. bengalensis*) captured together in Calcutta—a *M. rattus* from Dacca and another from Calcutta; also some specimens of *M. concolor* in Rangoon. The colour in question is seen in many other animals, notably in the commonest variety of ferret. Yellow is perhaps the technical term for it, but tow-coloured is more descriptive.

Cases of wide variation in foot length are also occasionally met with in other species. Twenty-two mole-rats were obtained from one field in Purneah, two of them will be referred to. As regards length of head and body they are very nearly alike. The hind foot of one, an old male, is 35 mm. in length; that of the other, a more lightly built female, is only 26 mm. The difference in outward appearance is most striking; the females of the species are not small footed as a rule. There is of course no evidence to show that either animal was the founder of a large or a small-footed group, the case is only mentioned in order to illustrate the great differences in foot length that may occur in the same community of rats.

In reviewing this case we are again face to face with the question—how is it that each of the three individuals combines in its person the two separate peculiarities of colour and foot length? Again there appears to be only the one answer, because they are of the same lineage

in the strictest sense, *i.e.* they are descendants of an individual that happened to possess these peculiarities and arose as a sport from the common stock.

We will now pass to another case which illustrates the occurrence of minor structural characters.

Hitherto it has been assumed that each of the well-known species has its own type skull, but this is true only in a broad sense. The common *rattus*, for example, undoubtedly has its own type of skull, which is recognizable in animals captured in various parts of the world. But any large collection of these skulls will show minor differences. Strictly speaking these should not be called individual differences, since they only appear as such if the collection is taken haphazard from all parts of a town, moreover the same kind of peculiarity is found again and again in animals taken from separate places. We may recognize certain definite types even among these minor peculiarities. Such peculiarities are seen in the shape of the nasal bones, of the supra temporal ridges, of the plate which bounds the infra orbital foramen, the degree of inflation of the tympanic bone, and other points. It is sometimes possible to observe that minor points such as these occur in every member of a family group. In the case now to be related it happens that these minor peculiarities are so well marked that they may be spoken of with confidence and illustrated graphically.

In Fig. 6 are seen drawings of the skulls of three mole-rats (referred to as A, B, and C) which were found burrowing in fields near Amritzar within a circle of radius a few miles. The dorsal, the ventral, and the posterior aspects of each are shown in succession from above downwards.

The most noticeable differences among the three are to be seen on the ventral aspect and are exhibited by the palatine foramina and the molar teeth, which are both shown white in the illustration. In the skull C the palatine foramen extends up to an imaginary line joining the first molar teeth. In A and B it falls far short of such a line. This is an important difference, since it is constant: any mole-rat is in one state or the other as regards this foramen. It is the custom to regard this character as generic. A and B would be placed by taxonomists in the genus *Nesokia*, C in the genus *Gunomys*. This is not the only differential character between the genera, but it is the most noticeable one. It may be mentioned in passing that the distinction between the two groups has long been recognized and is undisputed. The two kinds occupy for the most part separate areas. *Nesokia* is found in the north-western parts of India, *Gunomys* in the southern and eastern parts. It is unusual to find the two living side by side, as at Amritzar. The size of the teeth, as seen especially in the breadth and solidity of the molars, is also considered to be generic, for it is the rule that rats with short palatine foramina have large teeth. The skull B is, however, an exception to this rule: it has the foramen of a *Nesokia* and the molars of a *Gunomys*.

Let us now examine the upper surfaces of the skulls. A differs from the other two in being smaller as a whole, in being broader in proportion to its length, and also in the fact that the occipital condyles are invisible, owing to the vertical position of the posterior surface of the skull. A and B are much alike and differ from C in respect to the proportionate length of the nasal bones (shortness of

the nasal bones is one of the generic characters of *Nesokia*). As regards the ridges which mark the upper limits of

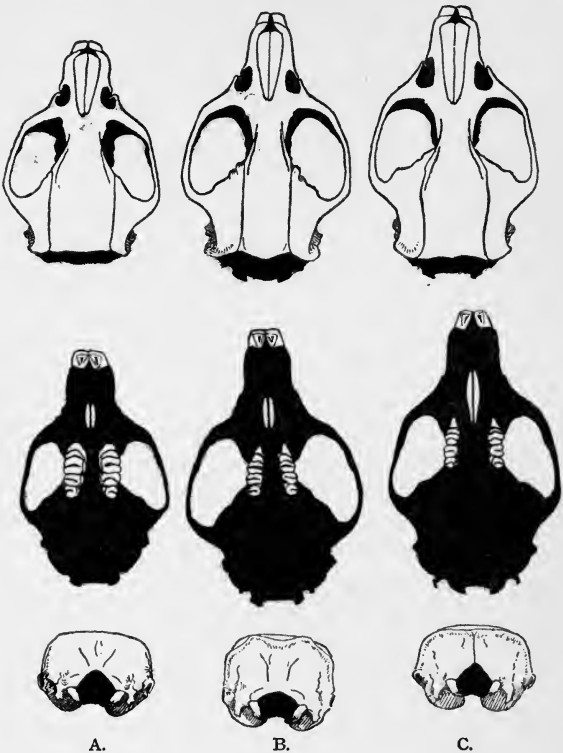


FIG. 6. × 0.

the temporal muscles, the three skulls are all different from one another. In A and B the ridges are straight, in C they are curved; but A and B are very different, for

although B is, on the whole, considerably larger than A, the distance between the ridges is greater in A than B, in the proportion of three to two. This of course is due to the fact that B has larger temporal muscles than A.

Let us now examine the posterior surfaces of the three skulls. In A and C the foramen magnum (shown black) is of the same type, somewhat like a gothic arch. This type of foramen is constant in both genera. The skull C is the only one out of some hundreds taken from various districts that departs from this type, and it departs very far from it, as shown in the diagram.

The skull C is typical of the genus *Gunomys* as found in the district. Eighty of the kind were examined and the illustration would almost serve for each of them. But skulls A and B, although belonging to the genus *Nesokia* since they possess the most important character of that genus, are not typical of *Nesokia* in the district. The skull A is however typical of a local group of rats (*Nesokia*) taken from a particular system of burrows near a village named Atari. The members of this group were distinguishable from the others of the district in being unusually small and in possessing a skull type of their own. The skull designated A was taken from a member of this group, and the illustration would serve equally well for any one of the four adult members of the group which were examined.

The skull B is also of an uncommon type. But only one specimen was obtained. There is, however, no reason why it should not have been typical of a local group.

The foregoing descriptions may perhaps appear purposeless, but they serve to illustrate somewhat the problems before systematic zoologists, and to explain why

the Mutation Theory as defined by its author (page 177) is valuable to him.

The local group found near Atari shows that peculiarities of cranial structure may occur in all the members of a family group, as do the peculiarities of coat colour, previously described. Our attention is more often drawn to coat colour than to cranial or other structural peculiarities, since the former are more conspicuous than the latter.

Further, the Atari group affords a good example of what often comes to be known as a rare species, and therefore proves the unwisdom of commemorating such by names. The group is represented by four adults and a number of young ones taken from a particular field. All have the same cranial type in the narrowest sense, a type that is recognizable from other types known in the district. But there is no evidence that the members of the group are isolated from their neighbours by infertility. They may be thinned by disease and weakened by the admission of other types, or the whole group may be drowned in a year when the monsoon is heavier than usual. The rise and fall of such a group may be accomplished within ten generations, that is within about three years. Why, then, should such a group receive a name ? A single individual showing a peculiar character or two is often considered a sufficient excuse for setting up a new name, and four like individuals taken from the same district would generally be regarded as a sufficient reason for so doing.

The cranial characters just described though apparently small are important from a taxonomic point of view. For example, the several points of difference between

skulls A and B are as a whole more conspicuous than the cranial differences which separate the well-known *M. rattus* and *M. norvegicus*. These two common species, although living side by side in many parts of the world, preserve their identity because they are mutually infertile. Similarly, types A or B would no doubt become permanently established, if they happened to be infertile with all other types.

CHAPTER VII

THE next case to be described is of a somewhat different order. The groups mentioned hitherto are numerically small and differ from the normal multitude in one or a few obvious characters. But in the case to be described now, we cannot speak with decision about a normal multitude and an abnormal group which is among it. When we find, within a limited area, a number of animals, some of which have a certain characteristic while others have not that characteristic, we may divide them into two groups, but we cannot say that the one group is normal and the other abnormal, when the numbers of each kind present in the area are about equal. The rats of Nainital were found to be in this condition.

Nainital is a small town, lying at an altitude of about five thousand feet among the hills on the southern flank of the great Himalayan range. The compact portion of the town occupies a small area, but the outlying houses are scattered at a distance from one another, so that the cantonment as a whole covers an area of about a square mile. The town is isolated in position, since although it is within thirty miles of the plains yet it is not on the way to any other places. It lies wholly within a crater-like depression, at the bottom of which is a lake. The summits of the surrounding hills stand at about a thousand feet above this lake. The compact portions of the town lie

at the northern and southern ends of the lake ; the outlying houses are situated upon the sides of the crater, but all the buildings lie within the boundary afforded by the summits of the surrounding hills. The crater lies open to the south, in which direction the lake drains away to the plains in a small stream. The town communicates with the plains by a cart road, which takes much the same direction as the stream. There are a few small hamlets scattered at wide intervals among the surrounding hills, but it is evident that Nainital is more isolated than towns lying on the plains of India. The isolation spoken of here is, of course, in special reference to the rats living within the area. It must be noted that the whole town of Nainital has arisen within the last century. It cannot, of course, be said that during that time no rats have entered the crater from outside, but from the circumstances of the case it is evident that the rodents now living in Nainital are mainly the descendants of those living on the spot before the town began to expand. It is necessary to keep this in mind when considering how these animals came to be as they are.

Let us now consider their condition at the present day. At Nainital, rats were not being destroyed as a sanitary precaution, consequently the number of animals dealt with is small, being only about fifty. It may be mentioned in passing that it is not an easy matter to obtain rats in large numbers. Any one who wishes to investigate them unaided, must be content to proceed slowly. The chance of a trap, set over night, containing a rat next morning is about one to four ; it varies of course according to the type of trap and the place in which it is set. The large numbers obtained in other places were due to the

concerted efforts of many persons, aided by considerable sums of money. However, the fifty obtained are a fair sample of the community. All of them belong, broadly speaking, to the species *Mus rattus*; that is to say, their skulls are indistinguishable from those of the common lowland rats, but they possess certain superficial features of their own. The tail is, on the average, scarcely longer than the combined length of the head and body, the ventral surface is white, though in a few the roots of the ventral hairs are light grey; the fur generally is longer and more plentiful than in the lowland rats.

Having enumerated the points of distinction between the Naini rats and their lowland relatives, we must now consider how the former differ amongst themselves. They fall at once into two groups, the members of which are recognizable from one another at a glance. The tails of some are pigmented in their whole length and to an equal depth on both upper and lower surfaces, just as are the tails of lowland rats. Others have lost part of their caudal pigment. In all, the pigment has quite disappeared from the lower half of the circumference of the tail, the junction between the black upper surface and the white lower surface being sharply marked in a line along either side. There are no differential features, other than the caudal pigmentation, by which the separation into groups could be effected. If the tails of the fifty animals were to be cut off and the bodies alone presented to an observer he would not be able to sort them into two groups. But the amputated tails could be sorted without difficulty.

We now come to the most interesting feature of the case. These two kinds, the black-tailed and the

white-tailed, were found living apart from one another. The principal masses of buildings, the bazaars situated at either end of the lake and certain large buildings close by, contain only the black-tailed kind; but of eight isolated houses lying at some distance from and above the level of the lake which were investigated, six contained only white-tailed rats, the numbers captured being 4, 5, 7, 3, 3, 1. The seventh house contained four white-tailed and two black-tailed rats. In the eighth isolated house one black-tail was captured. These facts are sufficient to show that the two kinds although distinguishable by such a trivial character are segregative, that is to say on the whole they are living apart from one another.

It is evident that we do not know how this state of affairs came about, for no one observed the vast number of little events which led up to it; but if we wish to guess at the method, we must be guided by events that we know to have occurred in other places. Hence I arrive at the following conclusion. The progenitors of the white-tailed rats were born from the common black-tailed race. They arose from one or more than one family group, comparable to those found at Rangoon and Poona, the members of which were distinguishable, even at first, from the common race. It was their custom, we know not why, to live apart from the common kind and to breed among themselves. The chief masses of buildings were strongly occupied by the common kind, consequently they found a place in the outlying buildings.

The two kinds, the black-tailed and the white-tailed, are quite distinct from one another, no one would hesitate in sorting them. But neither kind can be regarded strictly as a pure race. After examining all the members

of one or other class, a careful observer would not say that all possessed the same number and the same kinds of characters. They differ from one another in small but appreciable points, in the length and tint of the fur, in total size or in the proportional size of any part such as the ears or the feet. We may feel sure that these small points are hereditable, since they may be characteristic of a few animals caught together. It seems that a race is never pure in a strict sense, but that the term pure race should only be used in a conventional manner, to indicate a group of animals all possessing some one or more obvious characters which are special to them. When the experimental biologist uses the term pure race he means, perhaps, that the race is pure as regards those characters which he is studying, at all events this is the sense in which the term is used here. I purposely ignore the small, less appreciable characters and pay attention for the time only to the pigmentation of the tail for that is the obvious character which is the mark of the classes under discussion. The black-tailed rats of Nainital may, therefore, be considered as a pure race in regard to their caudal pigmentation. The white-tailed race, however, cannot be regarded as pure in the same sense, for there are among them two distinct varieties.

The inquiry at Nainital had a strong influence on my opinions, I will therefore relate the order in which the facts came before me, so that the reader may ask himself how he would have reconciled them with certain of the accepted opinions about species, their origin and distribution. At first I examined about twenty rats which had been caught in various houses in the bazaar, *i.e.* the more central or compact part of the town. I regarded

them as a fair sample of the rodent population which seemed to be as constant in type as that of most lowland towns. They presented the peculiarities already mentioned, in comparison with lowland rats. They were somewhat grayer in colour with white under-parts, their tails were relatively short and their fur dense. On the first occasion of setting traps in an outlying house, two animals were captured which caused great perplexity. They were captured side by side in a small cupboard, both were adolescent and in the same stage of adolescence. The probability that they were of the same litter was therefore great. The tail of one was bicoloured in its whole length, that is to say the upper surface was black as far as the tip, the lower surface being devoid of pigment. In the other, the dorsal pigmentation did not extend so far as the tip of the tail, but came to an end a little beyond the middle of its length. This is the type called semi-bicoloured, Fig. 2 *c*.

Except for the colour of their tails they were as alike as two young rats of the same litter usually are. Moreover, except by their tails, they could not have been distinguished from many of the adolescent rats which were being caught elsewhere. They were compared with three young black-tailed rats caught in the bazaar. If the tails of all five had been cut off there would have been no obvious means of distinguishing the tailless bodies from one another.

The interest of the case is this. Both types of caudal pigmentation exhibited by this remarkable pair have been known for a long time as specific characters, which in the first instance were defined by taxonomists and have since been used on several occasions as marks of identification.

Since in all probability the couple were of the same litter, it was evident that they could not be regarded as belonging to different species, although they exhibited a specific difference.

Let us now consider some of the established species which are known to systematists as having bicoloured tails and semi-bicoloured tails.

A completely bicoloured tail is a special characteristic of *Mus vicerex*. A rat of this species is a short-tailed, thick-furred, white-bellied animal, with a skull of the *rattus* type. The species was first described from eleven specimens caught in Simla in 1903, it was afterwards shown that the common rats of Kashmir resembled them so closely as to be considered of the same species. It is uncertain whether the kind persists at Simla at the present day, since of fifty-eight rats caught there in 1908, not one was of this type; all of them had black tails. There is, however, no doubt about the Kashmir rats. I have seen over thirty of these animals, obtained from two different sources, and in all the tail was completely bicoloured. *Mus niveiventer* is a species which was first recorded from Khatmandu in 1836, it has also been recorded from Simla. It is a thick-furred, white-bellied animal, with a short bicoloured tail. How it differs from *vicerex* is not clear. Besides these two species, others characterized by a bicoloured tail have been recorded from the Malay States, usually from highland districts.

Now let us turn to the other type, in which the pigment of the tail is confined to the dorsal surface, but only reaches halfway along it, so that the terminal portion of the tail is pure white in its whole circumference. Rats with tails of this kind have been recorded on more than

one occasion from the Oriental region. When found the name *M. berdmorei* has always been applied to them. In regard to this species we read in " The Fauna of British India "—" tail bicoloured, the upper surface of the basal half brown, the lower surface of the basal and the whole of the terminal half pale with white or whitish hairs, the type comes from Mergui, specimens have since been obtained from Moulmein, Manipur, and the Khasi hills." There is no doubt that the species has been recognized when met with by the peculiar colouring of the tail, for there is no other definite character by which it could be recognized. Hence it has been the custom to regard all rats with tails in this condition as descendants of a common stock, wherever they may have been found. If we are to carry on this custom we ought to regard the individual under discussion and others like it from Nainital as of the same stock.

For similar reasons, its companion ought to be regarded as *Mus vicerex*, for when compared side by side with a Kashmiri rat the only appreciable difference lies in the general colour of the fur, which is slightly redder than in the rats of Nainital. The short bicoloured tail, the white belly and the dense fur are characters common to both which give them a remarkable degree of resemblance. It is probable that if a pair of rats with completely bicoloured tails had been captured in Nainital and sent to a museum in Europe for identification they would have been placed either in the species *vicerex* or *niveiventer*, if this last is still a recognized species. Similarly, if a pair with semi-bicoloured tails had been sent they would have been called *berdmorei*. In accordance with the rules of systematic zoology they ought to be treated in this manner.

So from one point of view they are separate species, but yet no one who had taken them from the traps could have regarded them otherwise than as belonging to the same family circle. They must have been more nearly related to the black-tailed rats of the Nainital bazaar, than to animals living in such distant places as Mergui or Manipur, Khatmandu or Kashmir.

Some interesting problems therefore arose out of the capture of this couple. Accordingly steps were taken to obtain others of the same group. Six more were obtained from the same house and the adjoining outhouses. All were deficient in caudal pigmentation. Three were strictly in the condition of *berdmorei* as regards caudal pigmentation, that is to say, the dorsal pigmentation extended up to the middle of the length of the tail or slightly beyond it, but in the other three it did not extend so far. Later on, seventeen more white-tailed rats were caught in other isolated houses. In sixteen of them, the pigmentation reached a variable distance along the tail but fell short of the tip. In one other it reached as far as the tip (bicoloured type) ; this was the second specimen obtained from Nainital, which, from the taxonomist's point of view, ought to be called *vicerex*. The two were captured in separate houses situated about a mile apart, in both cases they were found living along with rats having tails of the semi-bicoloured type. It is probable that if a pair of rats with completely bicoloured tails had been selected artificially for breeding, they would have bred true to their type and given rise to offspring of the type *vicerex*. However, it is evident that at the present day the *berdmorei* type is the commonest among the rats living in the outlying houses of Nainital, and that the type

vicerex exists there as an abnormality, being in the minority. It has been fully ascertained that the type *vicerex* is in the majority in Kashmir, it is the normal house rat of that country. It is likely that the type *berdmorei* occurs as an abnormality in Kashmir.

CHAPTER VIII

In the opening sentence of his great work, De Vries writes as follows: "By the Mutation Theory I mean the proposition that the attributes of organisms consist of distinct, separate, and independent units. These units can be associated in groups and we find in allied species the same units and groups of units." This is to me the Mutation Theory that has helped so much towards any understanding of the living kingdom that I may have. It is the second step towards understanding living things, the general recognition of the process of evolution being the first step. The Mutation Theory was based upon observations in the vegetable kingdom, but in the words of its author, "It is perhaps unnecessary to remark that these generalities refer to the animal as well as to the vegetable kingdom."

When speaking with enthusiasm of this theory I am thinking of the above statement of it as made by its author. It is perhaps more usual to think of the Mutation Theory as any proposition contained in the book bearing that title: such as, that species are indivisible or real, that species and varieties are ordinally distinct, and that mutation is a periodic process. These propositions have given rise to much discussion, they have seldom found favour with zoologists. Animal species appear to be conventional rather than real, so also appears

the distinction between species and varieties. Sports, common enough in the animal world, are not quite like the mutants of Œnothera. Any class of animals examined over a wide area is seen to contain sports and groups of sports at all times. There is little evidence of periodic mutation among animals. A single case suffices to illustrate this. How is it that Lingula has been exempt from mutation, if that process is periodic? Thus the zoologist may criticise De Vries' opinions, and has done so in certain instances without remarking, perhaps even without noticing, that his criticism is not directed against the Mutation Theory, as defined by its author.

The teachings of De Vries concerning the elementary nature of species, the distinction he draws between species and varieties as well as the idea of periodic mutation seem to be distinct from the Mutation Theory as defined by him. In that theory he has pointed out to us that living things possess many of their attributes as separate entities. Since this fact has been pointed out, some of us can see it plainly; so plainly that we feel it must be for long a guiding principle in biology and even in daily life.

Let us pass now to another subject. It is difficult to compare the higher animals and plants, since the latter are comparatively simple in structure, and do not move from place to place, hence it will be more satisfactory to compare what we have seen among the rats with similar observations made in other parts of the animal kingdom. Few persons have attempted to make a general survey of a generic group of animals as it lies spread over a wide area. Fortunately, one such attempt has been made with great success. I refer to Mr. W. L. Tower's work

on the potato beetles of America. In comparing my results with his, I am fully conscious that as regards completeness no comparison can be made. Even as a survey my work is less complete than his, but the survey of the beetles was merely preliminary to the far more important breeding experiments which were carried out. In the case of the rats no experiments, worth mentioning, were made. However, the results of the survey of the rats are in many ways like those of the more complete survey of the beetles. In both cases sports and groups of sports were found. The sports were of a limited number of kinds, the same kind appearing in widely separate places, some more often than others, for unknown reasons.

Mr. Tower's memoir was published in 1906 * and contains the results of eleven years' work, it may well serve as a guide to any one who wishes to inquire into the method of evolution in a direct manner. A problem offered by plants and animals is this—how have they come to be as they are? The systematic zoologist who determines how the animals are, their appearance at the moment, does not as a rule inquire into this problem. On the other hand, some of those who are particularly interested in the origin of species, and endeavour to explain how animals reached their present condition are not so well acquainted with that actual condition. Mr. Tower, however, commenced his work among the beetles with a survey of the group, he ascertained the areas of distribution of the various species and sub-species. Some of these areas were of great extent, comprising more than the eastern half of the United States; others were

* Publications of the Carnegie Institute of Washington.

small, a few square miles. Sports, when met with, were described. Having observed and described the present state of the group, he next endeavoured to ascertain the steps by which the group came to be in that state. Although this seems to be the only way of reading the past history of a group, *i.e.* of ascertaining the method of evolution, few persons have as yet followed it.

Let us now see what Tower found. He examined 207,891 specimens of the commonest species, *Leptinotarsa decemlineata*, which were taken during nine years from seven widely separate parts of the United States. Among them he found 118 sports of nine different kinds. Some kinds were more common than others. Thus the kind *pallida* was found on sixty-three occasions, the kind *melanicum* thirty-one times, the other seven kinds were found only once or twice; a striking form called *tortuosa* was found three times. Some of these sports are shown on Plate II.

The idea of species is not confined to naturalists, every one has his own idea as to what constitutes a kind of animal. Nearly every one will agree that the beetles shown on Plate II. are of different kinds. If a person be questioned as to what he understands by a kind of animal, he will perhaps select an animal and state his belief that the ancestors of that animal always were, and its descendants always will be, like it. But the evidence is perfectly clear to the contrary, one kind may give birth to another occasionally, and the new kind may henceforth produce descendants like itself. It will be long before it is widely known that an animal of one kind may give birth to an animal of another, that is to say of a kind slightly but obviously different. Eventually it must

MULTITÆNIATA.

RUBICUNDA.

MELANOTHORAX.

DECEMLINEATA.

PALLIDA.

TORTUOSA.

PLATE II: SPORTS AND MUTANTS OF LEPTINOTARSA.

become common knowledge, for the evidence is clear and direct, more so even than the theory of descent in general. The beetles illustrated herein are each of different kinds in the usual sense. As a rule each kind breeds true to its own kind, but occasionally some kinds give birth to others. Similar events seem to occur among the rats, and doubtless among the animal kingdom at large.

Let us now continue our summary of Tower's researches. The sports were found singly among swarms of the normal or common *decemlineata*, sometimes they were found in twos or threes, and on one occasion six of the kind *pallida* were found in the same neighbourhood. These six were kept for experiment: they hibernated as usual. One female and two males survived the winter and became sexually mature in the following spring. From a pair of them, descendants were raised for seven generations, in nearly every generation some hundreds of beetles were raised, large numbers had to be destroyed in order to keep the experiment within bounds. All, without exception, were of the *pallida* type. No mention is made of any ill effects arising from inbreeding. This experiment shows conclusively that if a pair of *pallida* were to be isolated physiologically or geographically in a suitable place, they would give rise to a species.

A second experiment shows, that *pallida* may persist when mingled with *decemlineata*, in spite of the fact that the sport is not sterile with *decemlineata* and sometimes breeds with it in nature. *Pallida* does not then become swamped by crossing. In this experiment twenty-two *pallida* and thirty *decemlineata* were confined together and allowed to breed freely, in each group about half were

males and half females. The beetles sought one another for breeding. In most cases each kind united with its own kind; but there were exceptions, for every seven normal unions there was one cross. It is evident that each kind preferred to unite with its own kind, although it was able and somewhat willing to unite with the other. This preferential mating must have been an important factor in the survival of both the kinds. The mixture was allowed to breed together for five generations; there was considerable mortality among both during hibernation. In the fifth generation, there were 420 *decemlineata* and 1049 *pallida*, although when the experiment started the former were in the majority. The whole were returned to nature in a plot under control. Many of both kinds were killed during hibernation. In the spring, sixteen *decemlineata* and twenty-nine *pallida* emerged and commenced to breed, giving rise to some hundreds of offspring, each to its own kind. Hence *pallida* was on the way to becoming an established group or species. Indeed the probability of its establishment was so great that all the beetles had to be killed, otherwise they might have spread over the country as a pest. It was known that *decemlineata* had spread over most of the United States in less than fifty years. This mixture of the two kinds might also have spread far and done much damage. So far as the experiment went, *pallida* gained over the common kind. In the first generation it was in the minority, but in the sixth generation, when the experiment was stopped, it was in the majority. Hence the origin of a species in nature was actually observed. It may, of course, be argued that they were not a natural species. Being under man's observation they were more or less domestic,

so it may be said; but if this is a sound argument we shall never observe the origin of a natural species. It is true that man had a hand in starting the group, but only by bringing together a single pair of sports. We know that a group of *pallida* is not easily established in nature since the event has not yet been witnessed. But thousands of the kind are being born as sports every year, since sixty-two of them were found among less than a quarter of a million of the normal kind; and time is going on. However unlikely the establishment may be, it must eventually happen, unless of course *decemlineata* itself becomes extinct or ceases to give off sports. However unlikely any particular combination of events may be, given sufficient time it will occur, provided that it continues to be possible. The possibility in this case was demonstrated.

If *pallida* had been found established in nature we should of course have known nothing about it as a sport, it would have been a species. But this is a digression.

We will now consider the most important part of Tower's researches. The potato beetle lays five batches of eggs at intervals of a few days. The second batch does not begin to develop in the ovary until after the first is laid. The third batch not until the second is laid, and so on. This peculiarity in the life history of the beetles afforded a valuable opportunity which many would have overlooked. Fortunately the opportunity was used with good effect, in the following manner. Certain beetles of the species *decemlineata* were placed under unusual physical conditions, great heat and dryness, during the time when their first three batches of eggs were being formed. The eggs when laid were set aside

(lot A). But during the formation of the fourth and fifth batches of eggs, the beetles were kept under normal conditions, and the eggs were also set aside (lot B). Out of lot B, sixty-one beetles were raised, all being normal. From lot A, ninety-six beetles were raised, but only fourteen of them were like their parents, *i.e.* normal *decemlineata*. Of the others no less than eighty-two were *pallida*, and two were of another form called *immaculothorax*. Both the *pallida* and the *immaculothorax* were exactly the same as sports found in nature. We have seen that in nature *pallida* was much more likely to appear than any of the other kinds of sports, so also in experiment. This is not merely coincidence, it seems significant, but we know not of what. The artificial sports were in every way the same as those found in nature. Among themselves they bred perfectly true. When crossed with *decemlineata* they gave a simple Mendelian result, that is to say, in the second generation a quarter of the offspring were of the kind *pallida*, in body and germ.

The species *decemlineata* has a wide distribution over more than the eastern half of the United States. Another species named *multitæniata* has a smaller area of distribution which is quite apart from that occupied by *decemlineata*. The area occupied by *decemlineata* at present is north of the parallel of latitude 29° N. lying roughly speaking between that line and the great lakes. The species *multitæniata* lives in Southern Mexico, its area of distribution not being continuous with that of *decemlineata*.

Multitæniata gives rise to two other kinds, *rubicunda* and *melanothorax*. It is difficult to decide whether these forms should be called sports or species, since both have

been observed as the offspring of *multitæniata*, and both have been found in groups. In regard to *melanothorax*, Tower says, "I have reared this form from eggs laid by *multitæniata*. It is an elementary species which appears at various localities at frequent intervals, but which has not yet been able to become established as a member of the fauna of the habitat into which it is born."

The question—when is a species a member of the fauna of the habitat into which it is born, and when is it not a member?—seems to be unanswerable, since there is no fixed convention to appeal to. We can describe animals as we see them and inquire how they came to be in that state. What more can we do? However, even if we do not take this view, if we know what a species is, and are sure that small groups exemplified by the *melanothorax* beetles or by the white-bellied rats, appearing temporarily here and there, are not really species; yet we cannot deny that such small groups occur. It has been the custom to regard sports as negligible, must we also regard small groups of them as negligible?

The form *rubicunda* is perhaps a species in the usual sense. In the year 1903 it was found over an area of seven or eight square miles at Toluca in Mexico, three years later the area of its distribution was found to be about ten acres, hence the species is waning or may even now be extinct, but in 1903 it was a large group.

A number of *multitæniata*, collected at Guadeloupe in Mexico, were taken to Chicago and allowed to breed in captivity under conditions which were nearly normal. The great majority of the offspring raised were of the kind *multitæniata*, like their parents, but both *rubicunda* and *melanothorax* were born in the same line as sports.

Three of the former and one of the latter appeared as offspring of *multitæniata*. Both kinds were isolated and were found to breed true, each to its own kind, for several generations, without any tendency to revert to the type of the parent species. A pure line of *melanothorax* was raised from a single male specimen, by crossing with the normal *multitæniata*, the second generation contained a certain number of the kind *melanothorax* of both sexes, in accordance with the Mendelian expectation.

The line of *rubicunda*, which was raised directly, both male and female sports of this kind being obtained, was found to be " almost completely sterile " with the parent species, but quite fertile with specimens of *rubicunda* found at Toluca in natural circumstances.

In another series of experiments, a number of *multitæniata* were subjected to unusual heat and moisture during the formation of some of the eggs, and to normal conditions during the formation of the remainder. As in the similar experiments with *decemlineata*, the first batch of eggs gave rise to many sports, a few of the kind *rubicunda* and many of the kind *melanothorax*. The second batch of eggs gave rise to offspring of the parental kind. It is then a property of *multitæniata* to give rise to *rubicunda* and *melanothorax*, just as it is a property of *decemlineata* to produce the forms *pallida*, *melanicum*, etc. We may expect such events to occur, just as we may expect any of the well-known reactions between chemical substances. There is, of course, no similarity between animal mutation and chemical reaction. But it is evident that mutation like chemical reaction is a phenomenon which has been observed.

At present these two classes of events are not looked

at from the same point of view. Chemical reaction is commonly regarded as a natural process, but mutation is often spoken of as though it were supernatural. Unsupported bodies fall to the ground, chemical substances unite in various ways, animals and plants produce mutants. All are events which have been observed; why, then, must we think that some of them are more supernatural than others? There is no doubt that to some biologists mutants have a supernatural appearance. The Mutation Theory brings back to their minds what they call the "doctrine of special creation," and is repugnant for that reason. But none of the older doctrines of creation could be expressed in terms of perceptible events, whereas the Mutation Theory is a description of events which have been observed.

It is perhaps this feeling that there is something supernatural about mutants which leads Mr. Tower to the opinion that they play a minor *rôle* in evolution. Like Darwin, he is unable to believe that they play no part at all.

When describing his own observations Mr. Tower appears to be almost convinced that mutants are of importance; thus on page 259 of the Memoir we read—

"The work of De Vries, however, shows conclusively that in plants the rapid development of new forms occurs also in nature, and it is now an established fact that it is not confined to domestic races, but is common to both wild and cultivated plants."

And on page 282 we read, in reference to such forms as *pallida*, "As far as I can judge, my variations from *decemlineata* are as pure and strong as De Vries' mutants

from *Œnothera*, holding their own even in crossing with the parent species. The experiments with *pallida* are strong evidence in favour of the origin of species by rapid change, better perhaps, than that afforded by De Vries' plants, because *pallida*, given the necessary start, needs neither selection nor attention to take care of itself, and my cultures would, I have no doubt, have spread widely in nature, as they began to do, had I allowed them to continue."

After this the summing up on the last page comes as a surprise. " This view differs, therefore, from that of De Vries, who sees in mutants the origin of species ; the real test is, as De Vries clearly sees, the fate of these mutants in nature. This I have been able in some measure to test, and at present there seem to be insuperable difficulties in the path of all observed mutants in *Leptinotarsa*. I therefore regard mutations as prophetic variations indicating what may perhaps be the next species in the evolution of the race."

No doubt there is truth in the belief that mutants are " prophetic variations," but what will be the manner of the fulfilment of that which they prophesy? They prophesy the arrival of a new race of their own kind. Why should we assume that they will arrive by the process indicated in the theory of Natural Selection, a process which has not been observed when the actual method of arrival was observed in one case to be growth from unity?

Of the many facts leading to the conviction that groups arise from mutants, those observed in the case of *rubicunda* are perhaps the most convincing, but scarcely less so is the fact that in the case of *pallida* steps had to

be taken in order to prevent a mutant from growing into a species.

Tower states his objection to the origin of species from mutants plainly. He does not believe that mutants would get a start in nature, since wherever he looked the commonest mutant, *pallida*, was not becoming established. Hence he concluded that it was impossible for it and all other mutants to become established. But it seems to me that this conclusion is unjustifiable. It seems ungenerous to speak of Mr. Tower's observations as small, since in comparison with other human observations they are great indeed, but we cannot overlook the fact that the species *decemlineata*, as it lies spread over more than half of the United States, is an immense thing of which a small fraction was seen. The area of distribution of the species is certainly large, and the beetle is very common in some places ; a fact that is illustrated by the following quotation from the letter of a farmer in Kansas, written in 1862, and quoted by Tower. "It is no exaggeration when I tell you that we have often in a very short time gathered as many as two bushels of them." If a cubic inch contains ten beetles, a bushel will contain over twenty thousand. It seems evident then that the population of potato beetles in the United States, like the house rats in India, must be recognized in thousands of millions. But the actual number of beetles examined for sports was less than a quarter of a million, that is a small fraction of the whole. Is it then justifiable to conclude that a group of *pallida* could not get a start either now or within the next few hundred years? is it a sound deduction from the observations made in the particular case, let alone the living kingdom in general? The reader must form his own opinion.

The question is, can these groups get a start, can a group containing fifty or so individuals arise from the one or two? If there are fifty of them in nature they have started and the rest may be easy. The observations among the beetles show conclusively that, given a start, a small group may become a species under natural conditions. They do not perhaps show so clearly that a mutant may get a start, but the possibility of its doing so was first clearly demonstrated by Mendel's experiments.

Among the rats it was the small group of mutants which were particularly in evidence. The groups found at Rangoon and Poona had, so to speak, started, this was certain, and they must, I think, have arisen from one or a few mutants. My own strong belief in the efficacy of mutants arose perhaps in the following manner. I had the benefit of Mr. Tower's observation that given a start the mutant might become a species, and I had also seen among the rats that such startings actually occurred, although the great majority of them come to nothing.

We are told that there were insuperable difficulties in the path of all the observed mutants of *Leptinotarsa*. But what if there had been no difficulties? In that case the sports would not have come before us as such, they would have been species. They appeared as sports because they could not overcome the difficulties. *Pallida* was only found as a sport, but *rubicunda* was found both as a sport and as a species; at Toluca it had overcome the difficulties.

Different minds pronounce different judgments. Men see alike, but they judge differently concerning what they see. Some, like myself, judge that groups arise from mutants, that is to say they believe that mutants occasionally grow into groups. Such occasions must be rare, for

if a thousandth part of the mutants which appear were to form groups, the profusion of groups would be much greater even than it is.

Mr. Tower has other judgment, leading him to the opposite conclusion. He has placed on record some most valuable facts, and has summed them up according to it. Let us bring both judgments to bear on the same facts in succession. In order to make such a comparison it is necessary to quote again from the memoir. The argument against the efficacy of mutants, opens as usual with the declaration that they are too few in number to be able to make any headway against the preponderating normal multitude. The question of numbers is important and as a result of much labour Tower has shown exactly how rare (or how common if we choose to express it so) mutants are among the potato beetles. He then states his belief that they are too rare to be effectual, in the following words :—

" From these records in nature we see the great rarity of these forms, and taking all the counts, out of 207,891 beetles examined there were 118 sports, or in the ratio of 1761 to 1. This high ratio, however, is due to the lot from Cabin John Bridge, where there were an enormous number of sports, due to most unusual conditions of environment." (The number was 82 sports of four kinds among 11,792 beetles collected in one field.) " If we remove these the remaining 196,099 show 36, or in the ratio of 5447 to 1. Taking all the available data gathered from 1894 to 1904, I find that on the average about one beetle in six thousand is of the class which is designated as sports, discontinuous variations, or mutants." A commentator with other judgment might object to the Cabin

John Bridge figures being excluded in this manner in order that mutants should appear rare. He might prefer to exclude the United States in general and consider only the figures from Cabin John Bridge on grounds which would appear to him reasonable, that a species does not arise all over a wide area but within narrow limits. Thus he would find that mutants occurred in the proportion of 1 to 143, and would have succeeded to his own satisfaction in showing that they were not rare but common. The reader must settle the question for himself according to his judgment.

On page 277 of the Memoir we find an explanation of the fact that *pallida* has not as yet (and therefore cannot) become established. "*Pallida*, however, is the most common one, occurring once in every five thousand cases. Suppose one to arise in nature, I have shown that there is close selective mating in *decemlineata*, and that in confinement the chances are seven to one against *pallida* mating with *decemlineata*. When we add to these conditions the great mortality during hibernation, which is especially fatal to extremes of variability, the probability of a single variation of this class being able to propagate itself is so remote as to become a real impossibility."

Let us examine this passage also from the opposite point of view. Selective mating, that is to say the well-known preference of like for like, appears to Mr. Tower as a factor which must hinder the establishment of a group of mutants. But, from the other point of view, it appears as a powerful factor in helping the establishment of such a group. From the result of the experiment, in which a number of *decemlineata* and *pallida* of both sexes were confined together (pages 121–2), Tower draws the

deduction that the chances against one kind mating with the other are seven to one. The experiments certainly show that the sexual attraction, *decemlineata* to *pallida*, is weak, and that the attraction, *decemlineata* to *decemlineata*, is strong. But they also show that the attraction *pallida* to *pallida* is strong, and this last will be an important factor in causing the establishment of a group, when there are two like sports of opposite sexes on the same plant or on neighbouring plants. Tower relates that he found two or three sports together at various times.

Let us consider also the case of the single sport alone among an unfriendly multitude of *decemlineata*, what chance has it of finding a mate? It is said that the chances are seven to one against it. If we express this statement in terms of events we shall see more clearly that out of every eight isolated mutants, occurring in separate places, one will unite with a *decemlineata* and seven will die unmated. But if this is true it follows that some hundreds of isolated sports will find a mate in every year in the United States for the mutant occurs in the proportion of at least two hundred to a million of the normal kind, and we also know that a quarter of the number of their grand-offspring from a hybrid union will resemble them in body and germ.

According to Tower the three factors which combine to make it impossible for mutants to give rise to a species in nature are as follows: firstly, the rarity of the mutants; secondly, the difficulty they would experience in finding mates; and thirdly, their delicacy. We have discussed the first two factors, and the third is not valid in the particular case of *pallida*, since when this mutant was

reared in nature together with the parent species it was not delicate, indeed it endured the frosts of winter rather better than the parent species. Although, as shown, we may explain away each of these preventive factors or even invert them in order to support the opposite thesis, there is no gainsaying the fact that, so far as it was possible to examine the group *decemlineata*, in nature, the mutant *pallida* was not meeting with success among it.

It is evident, therefore, that argument is not of much use in deciding the question of the efficacy of sports in general. It can only be decided by observation. But Mr. Tower's declaration that it is "a real impossibility" for a mutant to become established cannot justly be made, I think, as a result of his observations upon *decemlineata*. He observed a quarter of a million of the beetles of a certain species, that is to say less than a thousandth part of the number born even in one year, and from his observations draws a conclusion not only concerning the other 999 thousandth parts, but also regarding the much vaster mass that will no doubt be born within the next few centuries. Real impossibilities stand for all time.

Let us keep in mind the fact that among the quarter of a million examined, there was no established group of the kind *pallida;* but let us suppose that such a group had been found, a group that covered seven or eight square miles of country and comprised perhaps a million individuals; would this have been considered as evidence by Mr. Tower that groups arise from mutants? Evidently not, for he had evidence of this kind in the case of the *rubicunda* group at Toluca, but was not satisfied with it. The mutant *rubicunda* was born of *multitæniata* in Chicago, and at the same time there was a group of

rubicunda occupying an area of seven or eight square miles at Toluca. Can we escape from the conclusion that that group arose from mutants? Having seen the method of origin of a single *rubicunda*, why is it necessary to assume that there is another method?

Beetles of the kind *rubicunda* come into the world in one of two ways, ordinarily from an egg laid by a beetle of the kind *rubicunda* or extraordinarily from the egg of one of the kind *multitæniata*. This we know, and we had better hold fast to this knowledge and assume that these are the only methods of arrival, until some one can describe to us a third method in terms that we can understand. It may be said that we do not understand the arrival of *rubicunda* from *multitæniata*, but do we understand the common event *rubicunda* from *rubicunda?* We do not understand either of them, but we know that they occur, and that is what we require from a scientific point of view.

The facts relating to these two kinds of beetles will suggest to any one that species arise from mutants, though we know they may not convince him. But supposing that conclusion is objectionable, where lies the way of escape from it? The readiest way is to argue that the group of *rubicunda* found at Toluca was not really a species; that is to say, not sufficiently established to be a real species. By thus altering our conception of "species" we may subdue the thought that a species had arisen from a mutant. Before we can decide the question in a scientific manner we must therefore lay down a numerical limit for species in general. If we say that a group is not a species until it contains a million individuals, we shall then be able to know in any case when a species

has arrived. Until such a definition is laid down we shall never actually know whether a species can grow from an individual or not. I do not, of course, offer this as a serious suggestion; as such it is worthless, but it may serve to illustrate the value of the conception embraced by the word species.

While some are surprised at Mr. Tower's conclusions as to the inefficacy of sport, others agree with him. Nothing is to be gained by declaring that those who do not agree with us are mistaken. Those who come after will settle the dispute, and in their turn hand on other disputes for settlement.

Many will think that I am belittling a great work in pointing out that a quarter of a million is a small fraction of the mass of a species which extends over a large part of the United States. It seems to me, however, that it is Mr. Tower himself who has belittled his own work. In my opinion he has been the first to describe the origin of a species of animals as he himself saw it. No such description is to be found in the writings of Darwin, who described what he conceived to be the method of origin.

The species *pallida* became established. Every step in the process leading to its establishment was observed. It grew from a pair of mutants. It had to be exterminated. Tower regards this event almost as though it had never occurred, since he himself had a hand in it. Is the gust of wind that might bring two beetles together something essentially different from the hands of the man which caused the union? is the first in nature and the second outside nature? Where is this limit between nature and not nature? It is enough to know that the event occurred, and the manner of its occurrence.

CHAPTER IX

In the foregoing pages we have seen examples of animals which are abnormal in possessing one or more distinct characters, each affecting a particular part or system of the body. Hence from a certain point of view these characters are varietal and not specific. According to De Vries mutants differ from one another and from the parent species in every part of their constitution in all their systems, although to a slight extent. The specific character of a mutant or true species is a single indivisible attribute, affecting the whole constitution of an organism, having no parts such as might appear separately in different individuals. In the words of De Vries, the specific quality "must be regarded as the expresssion of a single character, of a single unit, which arose as such and as such can be lost."

Varietal characters, on the other hand, affect only a single system or part of the organism. The specific character remains as a whole and is still recognizable as such after a varietal character is added or subtracted. This conception of specific and varietal characters as things of a different order was a result of observations made upon flowering plants. It is difficult to form an opinion as to whether this distinction is recognizable among vertebrate animals. It is not easy to see. Were it obvious, there would be less difficulty in classification.

We should be able to recognize our species clearly, each species containing a number of varieties, each variety marked by its particular varietal character or characters, which would not obscure the underlying specific facies. We cannot always draw a clear distinction between species and varieties. But yet we cannot be quite sure that there is no distinction between a specific and a varietal character. The matter is well illustrated by rats. *Mus rattus* (the common or brown variety) and *Mus norvegicus* are so alike that an unobservant person might overlook the difference unless it was pointed out to him, that the ears and tail of the latter were relatively shorter. But yet it is true to say that these two species differ from one another as regards their whole constitution. Their skulls and even corresponding portions of them are distinguishable from one another, yet the differences are so small that many people would overlook them; but when once they are appreciated and remembered the observer will make no mistakes in identification. A number of skulls of the two kinds mixed together can be sorted without hesitation and without mistake. The same is true of other detached and corresponding parts of the body such as the ears, tail, or feet. Hence the relation between these two species appears to be that of mutants in the strict sense of De Vries. Although both are so alike we cannot say that one kind was derived from the other. We can find nothing in their appearance leading us to suppose that one was the original and the other a derived species. But, on the other hand, there are many so-called species of *Mus* which are obviously derivatives of *Mus rattus*. They possess what may be called the specific facies of *rattus* with one or more additional

characters imposed upon it, but not masking it. The facts may be illustrated by reference to certain parts of the body, such as the skull and the hind foot. The drawings shown at Fig. 1 would serve not only as illustrations of the common *Mus rattus*, but also of a number of so-called species belonging to the *rattus* group, but any one acquainted with the subject would recognize that they were not representations of *Mus norvegicus*. Thus there seems to be reason for believing that the distinction between specific and varietal characters which was recognized by De Vries in plants, is also recognizable among the higher animals.

But difficulties arise in many cases, for we cannot always say of groups—this is a species—that is a variety. Let us take a few examples. The black and the white-bellied rats shown in the frontispiece might well be regarded as varieties of *Mus rattus*, having a single additional character. The miniature *M. concolor* of Burmah might also be regarded as having the single character of smallness. But *M. vicerex* has at least three peculiar characters, *M. blanfordi* has at least five, *M. jerdoni* has six or seven, and others have so many that they obscure the underlying specific facies more or less. When is the specific character obscured, and when is it not quite obscured? These are questions which arise in many cases and cannot be answered.

There is no certainty in the matter at present. It seems, then, that we ought not to use the term mutant, in the strict sense, when speaking of the higher animals. By mutant in the strict sense I mean, as exemplified by those of *Œnothera*. We cannot believe in mutants, in this sense, among the higher animals until some one has

observed an offspring of vertebrate stock differing from its parents in all parts of its constitution, that is to say having a different specific facies from its parents. We may have to wait long before such an observation is made. In the mean time our attention may be turned to those characters affecting a particular portion of the body, the so-called varietal characters, for we know, without doubt, that offspring and parent may differ from one another in respect to them. Moreover, it is evident that many of our so-called species differ from one another in respect to such characters.

The following case of a polymorphic species of fish is of interest in this connection, since we can see it in an example of a character which affects the whole outward appearance of the animal to a remarkable extent, and is therefore more like the specific character as conceived by De Vries.

Malthopsis is a peculiar genus of fish which has been obtained four times in the trawl of the Survey Ship *Investigator* from considerable depths near the Andaman Islands. A single specimen was also obtained by the German research vessel *Valdivia* off the East African coast. The genus has been recorded from near the Hawaian Islands, but as described and depicted the specimens from this place differ in certain respects from those we are about to consider; at all events, they are not of the same species. *Malthopsis* is common locally, but not generally, in the seas round India. Thus it was obtained on four out of twenty-one occasions when the trawl was used near the Andamans in depths greater than a hundred fathoms. The four stations are not very close together, nearly two hundred miles intervene between

those that are farthest apart. *Malthopsis* has not been found elsewhere, although we are comparatively well acquainted with the inhabitants of the deep seas on either side of the Indian Peninsula. The numbers of individuals caught at the four stations are 10, 7, 4, and 5. Of the first group I have examined only six, so that my conclusions are based upon an examination of twenty-two individuals in all. The reader will no doubt notice that the conclusions are concerning a species; that is, many millions of individuals, out of which number twenty-two representatives are taken at random for consideration. Further, it will be obvious that we know nothing concerning the parentage of one of them.

It is necessary to describe briefly some of the external features of *Malthopsis*. The body consists of a flattened portion or disc which is triangular or heart-shaped, the mouth being placed at the corner corresponding to the apex of the heart. The paired fins are placed as in the diagrams, Fig. 7. The skin is set with bony plates. There are three prominent spines, one at each of the three corners of the disc. The foremost or nasal spine is placed just above the mouth. The others, which may be called the lateral spines, project one on either side from the outermost parts of the body.

There is great variation among this collection of fish, affecting two of their attributes, which appear to be quite independent of one another. These attributes are the shape of the disc and the arrangement of the dermal plates. Regarding the former, we see that there are three types; a narrow, a medium, and a broad type of disc. But if we disregard this and examine only the arrangement of the dermal plates, we find that there are

two distinct types which may be called the orderly type, and the disorderly type. Let us consider how these attributes are present among the fish.

The first is best studied by measurement. If we determine the value $\frac{\text{breadth}}{\text{length}} \times 100$ in a large number of any kind of flat fish, such for example as the skate, we shall find that the range of variation is less than ten as a rule. But in the twenty-one measured specimens of *Malthopsis*, the range of variation is as much as forty-four. The actual values arranged in series are as follows—49, 50, 51, 53, 55,—67, 72, 72, 73, 73, 74, 74, 74, 74, 75, 76, 77, 80, 85,—92, 93.

Although the individuals are unfortunately few, yet there are enough to show at least three types, in which the mean of the values expressing the breadth of the disc are approximately 51, 74, and 92. No one who has examined the collection would deny that the group is trimorphic as regards the breadth of the disc. The individual with the value eighty-five which is intermediate between the medium and broad is a curious unsymmetrical specimen which appears to be of the medium type as regards one half of the body and of the broad type as regards the other.

Now let us consider the dermal plates. As regards them, the fish appear in two states, some in one state and others in another. We may speak of these states as characters without making any assumption as to the nature of a character. The general nature of the armature is alike in all, that is to say it consists of soft skin in which are embedded conical limpet-like plates of bone, and, as mentioned already, there are three prominent

spines, one nasal and two lateral, which are part of the dermal armature. The length of the nasal spine bears a constant relation to the rest of the body. In all the specimens it is nearly equal to half the diameter of the eye. But the lateral spines are quite different in the two types. The two characters may be described thus—

The Character of Order.

The bony plates covering the whole body are arranged symmetrically in a pattern. Upon the ventral surface, this pattern is simple and easy to describe. In the centre of the space which lies between the bases of the paired fins is a conspicuous hexagonal plate which is surrounded and touched by six other plates. Between the bases of the smaller or pelvic fins are one or two pairs of plates, in front of this are a few others. Close behind the mouth is a curved row of four small plates. The lateral spines—but not the nasal—must be considered along with the plates, since every individual with plates arranged according to the above pattern is also provided with spines of the following type. These spines are large relatively to the size of the body, and are armed with four conspicuous spinelets.

The Character of Disorder.

The plates on the ventral side surface are very numerous and small and are scattered separately upon the skin according to no discernible pattern. The lateral spine is very small, and is covered with numerous inconspicuous points.

In fifteen of the collection the plates are arranged in the orderly manner : in six the arrangement is disorderly.

The arrangement of the dermal plates appear to be quite independent of the shape of the disc. Therefore, since

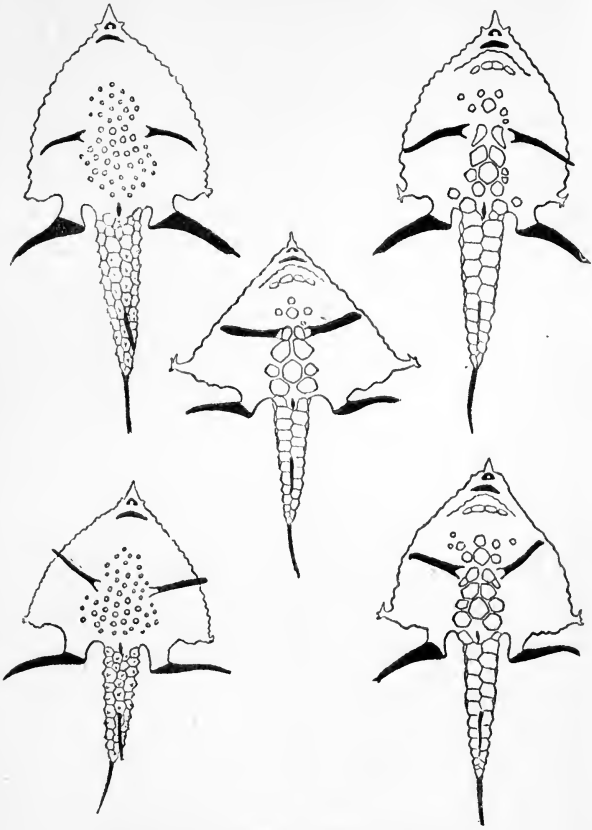


FIG. 7.—FIVE TYPES OF MALTHOPSIS LUTEA.
Ventral surface, about natural size.

there are two types of dermal pattern and three types of shape, six types of fish are possible, of these five are represented in this small collection. Of the fifteen possessing the orderly pattern, one is narrow, twelve are medium, and two are broad; and of the six which are disorderly in the arrangement of the dermal plates four are narrow and two are medium. The broad irregular type which probably exists is not represented in the collection. The five types were found mingled together in the following manner at the four stations or places of capture.

	Order narrow.	Order medium.	Order broad.	Disorder narrow.	Disorder medium.
Station 1	1	2	—	2	—
Station 2	—	5 (1)	—	1	—
Station 3	—	1	—	1	2
Station 4	—	3	2	—	—

As shown in the diagrams, the five types are quite distinct; no one would hesitate in assigning any particular fish to its type. There is, however, one of the regular pattern which cannot be classed either with the medium or with the broad, since it is nearly halfway between the two. No doubt if a large number of individuals had been captured other intermediate specimens would have been found among them, but this is no reason for supposing that the types appear as such because large numbers of such intermediates have been eliminated in the past. We cannot imagine any circumstance appertaining to one locality which would eliminate fish of breadth 60 and 80, and leave untouched those of 50, 70, and 90.

Nor can we believe that each type arose in its own separate place of origin, compelled by the conditions of that place. If they arose separately, how or why should the various types become mingled together over a wide area as they were found? At each station, and doubtless over much of the intervening ground, the various types arose from one original type and perhaps from one another. We describe such a group of animals as a polymorphic species, but there is no explanation of such a phenomenon. The conclusion to be drawn from it is expressed in the following words which were used by Darwin in reference to the causes of variation:—

"There are two factors; namely, the nature of the organism and the nature of the conditions. The former seems to be much the more important; for nearly similar variations sometimes arise under, so far as we can judge, dissimilar conditions, and, on the other hand, dissimilar variations arise under conditions which appear to be nearly uniform" ("Origin of Species," 6th ed. p. 6). There are two reasons given here for believing that the nature of the organism is a *much more important factor* than the nature of the conditions, in the change which comes over organisms. The present case and others like it, which might be chosen from all parts of the animal and vegetable kingdoms, illustrate them both. In this case, different kinds of variants occurred at any one station where presumably the conditions were uniform; while at different stations where presumably the conditions might differ the same kind of variant occurred.

It may be mentioned in passing that the physical conditions throughout the area, including the four places of capture, must have been remarkably uniform. All the

fish were caught in considerable depths of the sea (200 to 400 fathoms) from a bottom of green mud.

Nothing is to be gained by discussing the past history of this group, since we know nothing about it. The single specimen obtained by the *Valdivia* from near the east coast of Africa is evidently of the narrow disorderly type, judging from the careful description of it. It is therefore certain that this particular type has been established for a long time, and it may be the primary type; but since only one was obtained we cannot form an opinion on this point, for the species may have been polymorphic in that part of the world also.

The narrow disorderly type was described as the type of the species under the name *M. lutea*. Of the ten specimens caught on the first occasion, from which the genus was defined, five have passed out of the Indian Museum in exchange: it is likely that some of them were of the narrow disorderly type, so that this type may have been the predominant one, on the first occasion of capture, they are certainly larger in size than those of the other type. At any rate, this type was in some way conspicuous since it was regarded as the type of the species. The characteristic of disorder was first only to be seen among small individuals, hence it was regarded as a sign of immaturity. Later on, however, when others of the species were obtained it became evident that this was not the case, for the character was seen to be present in some fullgrown individuals. Hence there is no doubt that the species is polymorphic, the several forms being recognizable. Although the facts of the case cannot be tested experimentally, yet there is reason to believe that the attributes of order and disorder in the arrangement

of the dermal plates are alternative characters which cannot blend. Their chief interest lies in the fact that they influence the entire outward appearance of the fish to a remarkable degree, and hence they are somewhat like the specific character exemplified by *Œnothera.*

CHAPTER X

KNOWLEDGE is becoming more intimately connected with daily life. We do not wish to store it in print for the pleasure of seeing it grow, as the miser gloats over his hoard. As knowledge grows, it becomes more and more a guide for conduct; with its help we predict events and control them for our benefit, as much as possible. In short, man's efforts to obtain knowledge appear as efforts to obtain benefit.

In this chapter I shall endeavour to point out the bearing of the Mutation Theory in practical matters. Other writers have pointed out its value to the breeder of economic plants and domestic animals. We will not pursue the subject in those directions. The Mutation Theory has also a bearing on certain branches of pathology, especially on mental and other congenital diseases and on all those diseases that are due to parasitic organisms of any kind.

Many diseases may be described as one kind of organism invading another. The manifestations of such diseases, are a result of the interaction of two different organisms both liable to vary. In the method of their variation they must obey the unknown laws, which no doubt govern variation among all living things. The method of variation is essentially alike in the vertebrate animals, the insect and the flowering plant, nor can we

believe it to be different among those bacteria and protozoa that are the factors of disease.

The aspect of disease as the interaction of particular organisms is, comparatively speaking, a new idea, scarcely thirty years old, and even now it is usual to regard the various kinds of disease as fixed for all time, just as formerly we regarded the various kinds of animals from the same point of view. If disease is the interaction of particular organisms, there must be an evolution of disease just as there is an evolution of organisms. Although the physician may be willing to admit this in a general way, yet he does not regard it as of any practical importance. Thus he may say to himself—"appreciable evolutionary change can only be accomplished in the course of centuries, enough for me to know of things that can be seen in my lifetime." This seems to be a sound enough conclusion for practical men, but the premise is not sound, for there is no doubt that appreciable change may occur suddenly in the succession of organisms, and the fact should be remembered by the investigators of disease. The sudden appearance in an organism of new habit, which becomes henceforth permanent in its descendants, is not uncommon. Such a habit may or may not affect the lives of other animals or plants. In the well-known case of the parrot *Nestor*, we have an example of a habit which is harmful to another animal and indirectly so to man. *Nestor* is a small parrot, common in the South Island of New Zealand, which became notorious by acquiring the habit of feeding upon the flesh of living sheep. Clinging to the wool of the sheep's back, it bites through the skin and devours enough of the tissues to cause the death of the animal. It is said that it aims,

particularly and successfully, at the kidneys of the sheep; but many people would demand substantial evidence before believing that *Nestor* has an inborn knowledge of mammalian anatomy, especially those of us who know the difficulty of gauging the exact position of internal organs from the outside. But indeed the sceptic might be quite wrong. Thus, he might think, "It is impossible for one of the lower animals to know what man does not know;" but there are many facts to prove the unwisdom of such a thought. It is idle to discuss whether the lower animals have knowledge or not, at any rate they often behave as though they had very exact knowledge. But this is a digression. This peculiar propensity of *Nestor* became so common that rewards were offered for the destruction of the bird. The habit became a custom among the parrots. From my point of view, this fact need not be explained, but it should be remembered. A certain kind of organism suddenly became harmful to another kind. This is the fact, and the inference is that similar events probably occurred in the past and will occur in future.

It may be said that this is all beside the mark, parrots are not a disease of sheep any more than tigers are a disease of man, but the discussion is general and concerns all those organisms that are harmful to others. Parasitic organisms harmful to others are numberless. We do not know how the harmful habits were acquired, their origin is lost history; but it is important to remember that in this one case where the method of origin of a harmful habit was witnessed, that habit was acquired suddenly. Although it is not safe to infer that all parasitism is of sudden occurrence, yet we know that it may

be so. In many other cases it is difficult to imagine how it can have been acquired gradually.

The green-bottle fly is another example of an organism which sometimes acquires a parasitic habit. This fly is well known in many parts of the world; it is shaped much as the common house-fly, and is conspicuous owing to its bright metallic colour. There are several species of green-bottle flies, but, broadly speaking, they form a well-marked generic group named *Lucilia* (using the name in the broad sense to include *Compsomyia*, *Chrysomyia*, and *Pychnosoma*, as the term *Anopheles* is used to include several genera of mosquitoes). *Lucilia* is widely distributed over the world, together with other muscid—flies such as the smaller house-fly, *Musca*, and the larger blue-bottle or blow-fly, *Calliphora*. The three kinds are much alike in their breeding habits, all lay their eggs in putrid matter. Now, there are many recorded cases of flies depositing their eggs on the bodies of living warm-blooded animals, especially in the nasal passages and less frequently in the ears of man. The larvæ hatch out and do great damage to the tissues. Eggs are also deposited upon ulcerated surfaces, but such cases are seldom reported.

The point for emphasis is this. It is not a random habit of flies in general to deposit their eggs within the nostrils of a sleeping man, but a special habit of the green-bottle flies, a habit which breaks out from time to time independently in various parts of the world, for example in America and in India. The result of *Nestor's* vicious habit is hardly worthy of a place in the category of diseases, but the damage done by this fly seems to be more so, since it is sometimes spoken of as a disease

under the name of myiasis. If for the sake of discussion we regard the attacks of the fly as disease, we shall then see in myiasis an example of a disease which may originate sporadically, as the result of a harmless organism becoming vicious, on different occasions, in separate places. The sporadic occurrence of the disease is not due to a particular vicious strain of organisms which is carried about from place to place, as we commonly assume disease to be carried, but to the sporadic appearance of a vicious habit among a widespread group of organisms which is usually harmless. The habit may be regarded as a varietal character of *Lucilia*, which appears occasionally in that genus but not in *Musca* or in *Calliphora*. It is possible that the harmful character of certain other organic agents of disease may arise as a sudden change of habit, in a widespread and harmless organism. From our knowledge of the mutation of organisms in general we might expect similar changes to occur among the protozoa and bacteria; and if such expectations were realized, various anomalies in the mode of occurrence and distribution of certain diseases would be explained. On the other hand, there is of course no doubt that many diseases are due to well-established strains of permanently harmful organisms and when diseases such as these break out in a new spot, we may be sure that the particular organism has been carried to that spot from elsewhere. But with the facts of mutation before us, it is not safe to assume the eternal stability of pathogenic organisms. We can lay down no rule for disease in general, the many kinds must be considered separately.

The foregoing argument depends on the assumption that the habit of depositing eggs in the human nostrils or

ears belongs especially to the green-bottle flies. Descriptions of the fly are not always recorded in cases of myiasis. Within the years 1909–10, ten cases were described or mentioned in the *Indian Medical Gazette*, in three of these cases the fly was hatched out for identification. In two cases the insect was identified by a competent entomologist as a species of *Pycnosoma*, the third case was accompanied by a sketch and description of the insect, which was certainly a green-bottle fly. The three cases are each from a different province of India, being separated from one another by many hundreds of miles. In some other parts of the world, in South America and the southern United States for example, myiasis appears to be more common and is definitely ascribed to a green-bottle fly with three dark lines on the thorax, which is spoken of as *Lucilia* by some writers and *Compsomyia* by others. Myiasis is scarcely known in Europe, but there is an interesting case of *Lucilia* attacking living sheep at Amsterdam. Here there was a considerable outbreak of the disease, if we may so call it, which was explained in the usual manner on the supposition that the hostile organism was imported from a foreign land. In this case the outbreak was evidently due to a vicious strain of green-bottles, possessing a harmful character which appeared suddenly and was transmitted through a number of generations as in the case of *Nestor*.

Judging from these facts we may expect that any cases of myiasis, occurring in future, will be due to green-bottles and not to other kinds of muscid flies such as the common *Musca*, although the latter is ten times more common than the green-bottle and therefore ten times more likely to be the agent of myiasis, if it were not

for the special tendency of the green-bottles to become harmful. Indeed there can be little doubt as to the reality of this tendency. If it be granted, then we may draw the conclusion that the sporadic occurrence of myiasis is not due to the dissemination of a harmful organism but to an occasional and local change in the character of a harmless one. A way of escape from this conclusion may be tried thus. One may say, "I will grant that the green-bottles as a class have the harmful tendency, but the tendency is in all of them at all times. The infrequency of its appearance is explained by the fact that the flies seldom enjoy the opportunity of exercising it." But this argument appears unsound, for if every green-bottle possessed the tendency, myiasis would be far more common than it is. It would be liable to occur in all parts of the world, wherever men lie about in the daytime in a state of deep sleep or intoxication. Moreover there would be far more recorded cases than one of an outbreak among sheep,

In the foregoing only muscid flies are referred to. The *Sarcophagidæ* and especially the *Œstridæ* are better known for their habit of depositing their eggs in living tissues. The habit is established in those families.

It may seem unjustifiable to speak of harmful instincts, such as those of *Nestor* and *Lucilia*, as character-units. It is indeed assumptive since the Mutation Theory has not as yet met with general approval. This theory may be regarded as a statement that every hereditable attribute of every living thing, be it total size, or size of a part in proportion to the whole, be it a peculiarity of form or of colour, a change in the rate of reproduction, or a peculiar mode of action such as we call instinct, that every such

attribute has an individuality in the constitution of the organism. It is idle to discuss whether this generalized statement is true or not, as idle as it would be to discuss whether atoms are substantial things. Truth appears to be a word to express consensus of opinion, hence we must admit that the conception of character-unit is not as yet true. But, disregarding the truth of the conception, let us consider its value. Does it give any result? As an outcome of the atomic theory, man prepared new substances for the first time; these are results. (The question of the utility of the new substances does not come into the argument.) Who, knowing the facts, will deny that the conception of character-units has already given results in agriculture? Just as the conceptual atom has been applied in our dealings with what we call matter, so may the character-unit be applied in dealing with living things, *with result.*

We do not know yet if the theory is true, but we can know if it is effectual. From this point of view result is the test of truth. This view was upheld by Herbert Spencer in the following words: "The fundamental intuitions that are essential to the process of thinking, must be temporarily accepted as unquestionable: leaving the assumption of their unquestionableness to be justified by the result." It appears that the term character-unit embraces a conception already justified by results.

The peculiarities of *Nestor* and *Lucilia* may be called new instincts or untaught modes of action appearing suddenly. A good example of the sudden appearance of such an instinct which behaves as a character-unit was described by Tower. The facts are as follows:— Potato beetles have very constant breeding habits. There

are two generations during each year, one of which hibernates, *i.e.* burrows into the ground to a depth of about two feet and lies dormant for over six months. They retreat in September and emerge in May. It is necessary to describe very briefly the normal cycle, which is very simple. On leaving the ground in May, the beetles are not sexually mature, but they become so in a few days, and deposit their eggs in batches. These hatch out and give rise to adult insects (first generation of the year) in July. The beetles of the first generation soon become mature and deposit their eggs, which give rise to the second generation by about the end of August. Beetles of the second generation postpone their reproductive period until the following summer, they fly about for a time but soon enter the ground to emerge six months later, when they commence to repeat the cycle. The life-history is essentially the same in twenty species of *Leptinotarsa* examined, although of course the times vary a little. Hence we may say that the annual occurrence of two generations, one of which hibernates, is a generic feature of the group, a fact which makes the abnormal line, now to be described, appear in strong contrast.

Tower found among the offspring of some of the mother beetles which had been treated artificially, a number of insects which appeared outwardly to be normal but were abnormal in their life-history, for they hibernated in the fifth generation, not as usual in the second. They behaved in this unusual manner for three successive years, until the whole race was destroyed by an accident. To allow time, so to speak, for the five generations to be produced within the year, the period of hibernation was curtailed. They went to ground a month late and rose

early in January, nearly five months before their proper time. Moreover, as though with a purpose, they spent their winter sleep close to the surface instead of going deep, a circumstance which would have been fatal to them under nature, unless perhaps they had occurred in a humid tropical climate. If such a race had appeared in nature and found itself on an equal footing in other respects, in the rate at which it was being thinned by foes, and in the number of young produced by the females in each generation, it follows that the parent species would have been ousted from its food plant. Since the outward appearance of the new race would be the same as that of the old, an entomologist might wonder how it was that *L. decemlineata* had thus changed its habit in the course of a few years, being unconscious of the fact that *L. decemlineata* was extinct and some variety of it occupying its place.

Let us leave the subject of parasitism and turn to congenital disease. The word disease no longer expresses a single idea. Formerly it meant any disordered state in which a person might appear at birth or into which he might fall during the course of his life, any state of erroneous action of a system or part of the body appearing suddenly or gradually, being terminable or interminable during the period of life and lessening that period or not as the case may be. From this medley of states, which was the category of disease, were separated certain obvious congenital deformities such as hare-lip, club-foot, extra digits, etc. The distinction between such congenital states and disease is usually regarded as evident, although some pathologists have expressed the

opinion that monstrosities and all obvious variations are somewhat akin to disease: just as a naturalist who had studied pathology might express the opinion that the manner of disease was somewhat akin to the manner of variation. There is indeed a certain resemblance. If, for the sake of discussion, we regard each case that might come before the general physician as a separate event, and attempt to classify those various events; we notice at once that some fall naturally into classes, while others do not. Diseases fall into natural classes, but not injuries. We may of course speak of cuts, bruises, or burns, but these classes are only set up for convenience. No two injuries are alike, in each case the destructive agent is different, in kind or degree, in duration or point of application. But, on the other hand, if we see two cases of disease, small-pox, for example, in widely separate parts of the world, they are obviously similar, the separate events called cases of small-pox form a class or species. Moreover a species of disease may be divided naturally into sub-species. The method of this classification is much the same as that which the naturalist applies to organisms. We may call it for the moment natural classification. In many cases the similarity of the method of classification is the outcome of the fact that the disease is the effect of one organism upon another, both organisms being liable to vary in the manner common to living things, that is to say the specific group of the host and the specific group of the parasite are composite, each contains numerous sub-specific groups.

But if now we set aside the parasitic diseases and regard some of those that are not due to parasitism, still we find that even they may be submitted to natural

classification, using the term as opposed to conventional classification. Take, for example, the mental disorder known as *Dementia præcox*, it is regarded by alienists as a definite group, and it contains sub-groups, *Hebephrenia*, *Katatonia*, and *Dementia paranoides*, and no doubt these might be subdivided. The similarity between this method of classification and that used by the naturalist is evident, and this taken in conjunction with the fact that these disorders are inheritable, makes it worth while to examine the assumption that such disorders are essentially similar to the inheritable variation of living things in general.

To regard a demented person as a mutant may not appear useful at first sight. The name is not an explanation, we know very little about mutants, we cannot control their occurrence. But mutation is a process concerning the whole tree of life and eventually we shall know more about it and perhaps even control it to some extent; hence it is worth while to consider the many resemblances between abnormal mental subjects and mutants.

If they are mutants they are unsuccessful ones, unfit, predestined to fail. In the living kingdom the unsuccessful mutant is as evident as the successful one, though less conspicuous. Some cannot reproduce their kind at all, others can do so for a generation or two, they are none the less mutants, *i.e.* distinct kinds thrown off again and again from an established or successful kind.

The Mutation Theory will probably be found useful in psychiatry, the modern study of mental anomalies, at any rate the possibility of its being so should be considered.

Lugaro, the author of "Modern Problems in Psychiatry," closes his introduction to the subject in the following words: "From this rapid summary we see that Psychiatry not only enters into intimate contact with all the fundamental sciences of medicine and biology in general, but also touches upon the social and moral sciences. On all it imposes problems, from all it requests data. And rarely are these sciences sufficiently advanced to solve those problems or to supply those data. There is no doubt that all sciences have a reciprocal connection and each advances by taking advantage of the progress made by others."

In recognizing the characters of living things and the manner of their origin, biology has made permanent progress. We are beginning to recognize that all established and widespread groups, such as are called species in a wide sense, appear polymorphic when examined closely enough. They are not one kind, strictly speaking, but a mixture of kinds. The several mutants of *Œnothera* which form the basis of De Vries' enlightening work would be classed by an unobservant person as one kind under the name of evening primrose. De Vries regarded the events witnessed in *Œnothera* as special to that group because it was in a mutating period, but there is reason to believe that many other widespread groups of organisms will be found to be in a similar state, if examined widely and closely. It seems that the potato-beetles of America and the house-rats of India are in a similar state.

Let us forget for a moment the various conceptions indicated by the words species, variety, character, etc., and describe De Vries' great experiment in general terms. He took a kind of organism *Œnothera lamarckiana* and

raised from it a vast number of individuals through many generations. Contrary to expectation these were not all of one kind. Although there were thousands of instances to confirm the rule that like gives birth to like, yet there were occasional exceptions to that rule. A number of other kinds were given off from time to time from the original parental line, *i.e.* produced from the self-fertilized seed of *O. lamarckiana.* These other kinds were so definite that they could be recognized without doubt. It is important to note that the several kinds had not an equal chance of survival in apparently similar circumstances. Some kinds were able to give rise to long lines of healthy progeny, others, although thrown off from the parental stock, on many occasions were unable to establish themselves. After every occasion of origin they quickly disappeared.

In the words of De Vries, "The causes of such disappearances are mainly three: (1) sterility, or at any rate insufficient fertility; (2) constitutional delicacy; (3) inability to breed true." We are beginning to recognize that the so-called unfit mutant is a common product of the tree of life, and perhaps we shall recognize eventually that those mental disorders that are not the result of an external agent, should be regarded as unfit mutants.

This assumption is based on the fact that such cases of mental disorder may be classified in groups and subgroups, that the same type of disorder may arise again and again on various occasions in separate parts of the world. Though transmissible to some extent, like the unfit mutants of *Œnothera,* they cannot persist for similar reasons.

It is unnecessary to explain the mutants of *Œnothera*

as due to some unknown morbidity nor does it appear necessary to explain congenital mental disorder by any such agency. Whatever the explanation may be, it is probably the same in both cases. We do not know why mutants appear, but if we obtain knowledge in the one case it will be applicable to the other.

The conception of disease as an entity in itself is held by many physicians and alienists. For example, Lugaro writes, " Every individual variety, one may even say every individual, is in itself an exception to the law of heredity, though to a partial and scarcely appreciable extent. Variations which depend on pathological causes are always markedly divergent from the normal."

This passage contains the idea, suggested also by Virchow, that all marked variations, such as hare-lip and extra digits, are pathological in origin, but this suggestion was first made at a time when we hardly knew anything of pathological cause, before it was recognized that the greater part of pathological cause was micro-organism. In the present state of biological knowledge, it is unjustifiable to say that markedly divergent variations depend on pathological cause and that small variations are not so dependent. The variations of organisms cannot be divided into great and small, with a clear line between ; they are of all sizes so to speak.

If the present position of psychiatry is represented by Lugaro in the paragraphs quoted, biology is perhaps able to render it some assistance, though not of a hopeful kind.

The group of mental states gathered together by Kræpelin under the name *Dementia præcox* is illustrative of congenital disorder and is therefore chosen here for

discussion. The term embraces a large part of congenital mental disorder.

The present writer has little practical knowledge of *Dementia præcox*, but it is permissible to mention some of its principal features, as described in any standard work on psychiatry.

Victims of the disorder are usually regarded as normal until they reach early adult life, when certain eccentricities gradually appear in them. One writer mentions a parent who said, in describing its onset in her daughter, that she grew to be "more and more herself," a descriptive and significant statement which might be applied to other types of eccentricity, such as pass for normal.

Dementia præcox is peculiar among diseases in that it appears in the afflicted person when he or she has reached a certain age. There is a definite "age of onset," just as there is an age of onset for the full development of the third molar teeth and also perhaps for the development of what may be called normal mental maturity. In each case the particular attribute is more likely to appear at a certain age in early adult life than at any other age, and is less and less likely to appear as age increases. For example, most afflicted individuals become so at about the age of twenty, fewer at twenty-five, fewer still at thirty, and so on. Kræpelin has shown that this is true of the onset of *Dementia præcox*. It is doubtless true of the development of the third molar teeth, perhaps also it is true of the development of normal mental attributes. It seems that demented persons become endowed with their peculiarity in place of normal mental maturity.

The signs of *Dementia præcox* need not be referred to

at length. Stolidity of conduct, preference for confinement, disordered action are mentioned by all writers on the subject. Some perceive that the victims of the disorder purposely falsify their thoughts. This reminds us that two normal persons occupying diverse points of view may have much the same feeling towards one another as a result of argument, each feeling that the other is not candid.

A well-known alienist relates the following anecdote in reference to what is called "disordered action." A young girl, a victim of this disorder, had the habit of occasionally shrugging one shoulder. When asked why she performed this action, she replied, after a pause, that "underground electricity" caused in her the movement. To explain phenomena by an empty phrase is not in itself a sign of a disordered mind. A moment later it happened that her interrogator, who was seated, crossed one leg over the other in the usual manner. At once the girl asked, "Why did you do that?" Getting no reply she observed that that movement also must have been caused by "underground electricity." To cross the legs is normal action, since many persons do it; to shrug one shoulder or to assume a fixed grimace is abnormal action. We cannot explain abnormal action but neither can we explain normal action.

Dementia præcox is neither fatal in itself nor curable, were it either we could not of course regard it as the expression of a character or group of characters, or compare its victims to mutants.

Another similarity between the origin of these cases and the origin of mutants is seen in the fact that more than one offspring, growing up to be afflicted, are

frequently born to the same parents. "The brothers" are conspicuous in many asylums. In Tanzi's well-known book on mental diseases there is a photograph of three sisters, all victims of the disorder, who are alike even in feature.

Let us now consider some of the opinions held in regard to the nature of *Dementia præcox*. Some writers uphold the atavistic theory and regard persons afflicted with the disorder as reversions. They compare disordered action to "monkey-tricks" and draw special attention to the so-called signs of degeneration, especially to certain characteristics of the ear and hand, which are said to be peculiarly frequent in demented persons. Many of these peculiarities, however, may be seen in persons commonly regarded as normal. The so-called simian hand is said to be common in dementia. This type of hand is described as long and narrow with a thumb less "opposable" than usual. The hands of the many species of apes and monkeys display a great variety of shape, they are not always long and narrow. It is scarcely possible to construct an ideal simian hand from nature, but in some monkeys the plane of the thumb nail is at right angles to the plane of the finger nails when the hand is flat; that is to say it is more, rather than less, "opposable" than in man.

The term reversion which contains, so to speak, the essence of the atavistic explanation of dementia, is of course borrowed from biology, it is well therefore to note that this term is no longer in general use among biologists.

The atavistic theory does not find many supporters. There is one grave objection to it. As Tanzi points out,

the victims of *Dementia præcox* are unadapted to any imaginable environment. Because of their eccentricities, they cannot live unaided under the conditions of modern civilization, and it is much less likely that beings with similar eccentricities would have been able to live under the harsher conditions of prehistoric times. Hence Tanzi arrives at what is perhaps the accepted opinion, that since demented persons are not reversions, they must be suffering from a disease. It seems that in the search for an explanation choice lay between two conceptions, indicated by the words reversion and morbidity. They are not reversions, therefore they are due to an unknown morbidity; this seems to be the present position. But during the last decade the conception embraced by the word mutant has come into use and its claim should be considered by the alienist, bearing in mind that the unfit mutant is just as real a phenomenon of life as the fit mutant.

The assumed similarity between persons afflicted with *Dementia præcox* and the mutants of the animal and vegetable kingdom may appear more credible, if we compare this disorder with another mental disease, the cause of which is actually known.

If we examine the knowledge of the general physician concerning mental disorders one fact stands out. He is usually able to recognize the common disease known as *paralytic dementia*, but beyond that he knows little about the various types of insanity. This is not due to any defect in training, but to the fact that *paralytic dementia* is the most definite of all forms of insanity, and therefore the one that can be recognized with the greatest ease and certainty. It is indeed the only mental

disease that is dealt with in books devoted to general medicine.

Among the many kinds of insanity that are commonly met with, *paralytic dementia* is peculiar in two respects. It is definite in course and symptoms and it is produced by the invasion of a parasite. The disease cannot occur in the absence of the specific organism. If our knowledge of the causation of insanity in general were a mass of conjectures containing a single fact, this would be that fact. There are of course other influences in the causation of this disease, since everybody infected with the parasite does not become paralytic, but that does not lessen the certainty that the essential cause of *paralytic dementia* is the specific organism. It is hardly less certain that the general physician feels on sure ground as regards diagnosis and prognosis when he meets a case of *paralytic dementia*, but not so when confronted with other kinds of mental disorder. All those others are for the alienist to deal with, and sometimes one notices a certain scepticism in the mind of the general physician as to whether the various types described by the alienists are definite enough to be clearly recognizable. But there is of course no justifiable reason for doubting the reality of the types.

This distinction between *paralytic dementia* and the others is probably due to the fact that the former is caused by an invading parasite while the latter are not so caused. How, then, are they caused? asks the alienist, who finds himself face to face with exactly the same problem that confronts the biologist when seeking to know the cause of variation. Let us all admit that the cause is unknown, but let us recognize that the biologist and the alienist are

searching for the same thing and may be able to help one another.

In the last chapter we saw that mutants appeared in response to physical change such as heat and drought. But we cannot feel satisfied that such stimuli are of great importance in the production of the mutants. They are like an impulse that sets something moving, something lying ready to be released. That something is hidden in the nature of the organism.

All we know is that each kind of organism is in the habit of throwing off other kinds from time to time. The beetle *decemlineata* throws off *pallida* and *tortuosa* and others. The evening primrose produces a number of recognizable kinds. It may be assumed that the human race produces these peculiar mental types, such as are recognized by the alienist, in like manner on various occasions.

This chapter is not quite in keeping with the others; its chief purpose is to show that the Mutation Theory is of importance in pathology.

CHAPTER XI

THE opinion that groups arise from mutants is opposed to the theory of Natural Selection. We know this from a passage, already quoted (page 2), in which Darwin states that a species arising from a sport would arise independently of selection. It is sometimes said, that all experienced biologists regard selection as the principal cause of evolution and adaptation, but that all are not yet agreed as to the method of evolution, some believing it to be a gradual process, others that it proceeds by obvious steps. It is evident, however, that if imperceptible gradation is an essential condition of evolution by selection, such a statement is erroneous. If species arise directly from mutants, they arise independently of Natural Selection, as that process was imagined and described by Darwin.

The term Natural Selection does not call to mind the same idea in all persons. It may be used like the word competition, to express the fact that some organisms succeed while others fail. We can often see that the success of one kind of animal is accompanied by the failure of another or of others. We see this so frequently that we may regard success and failure together as one of the primary processes of life, and if we wish, we may speak of this process as Natural Selection. From this point of view selection appears to be one process just as

evolution appears to be another, the two processes being independent of one another. It is evident, however, that many persons believe the two processes to be dependent; by them selection is spoken of as the cause of evolution. But there is some reason to believe that the two processes are independent. Evolution is the arrival of novelties into the world; each extinct species of animal or plant was in its early days a novelty. It seems that this definition of evolution is sound. If so, it follows that selection is not a cause of evolution since one cannot obtain by means of artificial selection any novelty that may be required; moreover, novelties often appear without the help of selection. As investigation goes on, more and more cases are brought forward to exemplify (1) selection in a group without effect; (2) a new group arising from an established group without the help of selection.

However, the belief that selection is the cause of evolution still holds the field. According to this view, the offspring of animals and plants always show some slight points of difference from their parents. These differences or variations are said to be " in all directions." A species does not change so long as its circumstances remain unchanged. The characteristics of a species are always suited, so it is said, to the particular circumstances in which that species finds itself. Hence, if any individuals should deviate from the standard quality of the species they would be at a disadvantage and die out. Because for the most part the aberrant members die without leaving offspring, the species remains pure and constant in quality always suited to its surroundings. But when the circumstances change, the species as a

whole will also change, because the new circumstances will favour certain aberrant members of the group, which will survive and give rise through their descendants to a new species suited to the new circumstances. Variation being in all directions, the aberrant individuals are always present among the species in readiness to replace those that are no longer suited to their circumstances, except, of course, in the special case when the new circumstances are so stringent as to cause the extinction of the whole.

According to this view, change occurs primarily in the environment, secondarily in the species. It takes for granted the environmental change and explains the evolution of living things from it. It is our custom to take some things for granted and to explain others from them; indeed, this is the only way of making explanation. The essence of the theory is found in the words "variation is in all directions," and for many its charm lies in the fact that for them it explains adaptation.

There is much dispute over this question, but it seems to me that the dispute arises out of certain essential differences in the minds of the opponents, and no amount of evidence can alter those differences. It is usual to assert that Natural Selection must be upheld as an explanation of adaptation since it is only the explanation, hitherto suggested, that can be understood. To many persons this seems to be a strong argument for upholding the theory, while to others it does not appear to be an argument at all. The two points of view are distinct, some persons require explanations, others do not. From the point of view of the latter, adaptation appears to be a property of living matter in general, just as gravitation

is a property of all matter. In neither case does it seem necessary to ask why. All we can do is to know such processes more and more intimately. It cannot be said that he who has no craving for explanation is abnormal. It cannot be said with certainty that such persons are in the minority, although they appear to be because of the influence of authoritative teaching.

The student of zoology is first told that an explanation of adaptation is necessary, that all healthy minds require one. Believing this, he becomes willing to accept the Darwinian explanation, because it is the best available. The question as to whether any such explanation is a natural necessity, is never put before him for consideration.

It is, of course, deeply felt by mankind in general, that there must be a cause for the phenomena of life. But no such cause is known until it can be clearly pictured in the mind ; so clearly, that two or more persons working independently of one another would be able to illustrate it with some semblance. In other words, the agent must be more than verbal. In verbal form it may be a source of satisfaction to some persons, but the cause remains unknown to others. It seems that the power, influence, or process called Natural Selection is a verbal agent. Let us put it to the test.

The theory of Natural Selection offers three distinct propositions as follows :—

1. Evolution proceeds gradually, by steps so small as to be imperceptible.

2. It is accompanied by the elimination of those organisms called the less fit.

3. Selection works upon organisms varying " in all directions," and so makes cosmic that which is chaotic.

Of these three propositions, the third one is the essence of the theory, containing, as it does, the whole substance of explanation, and it is also the crux of the theory. The first two are within the sphere of knowledge, but the third one is outside it. We can form a mental picture of the progress of evolution. We can represent it by a series of models or diagrams. We can turn to nature and examine the steps by which it advances. Similarly, we all know what is meant by elimination, a process involving separation and rejection, which can be illustrated figuratively as well as described in words. It is sometimes assumed that the theory of Natural Selection consists wholly of these two propositions, and the hope is held out that, by establishing them, we shall be able to prove the theory, for it is indisputable that both propositions lie within the sphere of knowledge.

But what of the third proposition? which in popular opinion is the essence of Darwin's theory. Variation is chaotic or "in all directions." Is it possible to know whether it is so or not? What does the phrase mean? It cannot be represented by figures or models, which would express the conceptions of several persons. We know that there may be agreement in regard to this matter, but it seems to me that the agreement is merely verbal. That is to say, those who are in agreement find themselves in that state, because each is inclined to employ the same words in order to describe his uncertain idea. Even the verbal agreement is not close, for some prefer to describe variation as scattering or fortuitous; terms more or less synonymous with the expression "in all directions."

The foregoing criticism depends on the assumption

that one cannot imagine things varying in all directions. Let us examine this assumption further by making the attempt. A man is provided with a mass of clay and ordered to break off small pieces of it and set them aside. It is assumed that his mind is free from all constructive ideas. It is certain that no two of the resulting pieces of clay will be exactly alike, therefore they may be considered as things varying "in all directions." But they are not actually so. However many of them were made, they would be varying not in all directions, but in certain directions, determined by the fact that they were the work of man's hands. Among "all" we must include numberless imaginable forms, too delicate to be formed by hand.

This illustration does not help us to understand the meaning of the words "in all directions" as applied to variation, and any such attempt to illustrate their meaning will fail. But this formula is the basis of the selection theory, as commonly understood. Hence it seems that the basis is unreal.

Let us, for the sake of argument, assume that the pieces of clay are things varying in all directions. Though they are not so, yet we can hardly get a clearer notion of such things than is afforded by this illustration. Having made this assumption, let us inquire into the effect of selection upon the pieces of clay. The pieces are very numerous, say a million in number, and no two of them are alike. In order to bring about selection, we must have a selective agent or agents, each with its own special requirement or thing it wishes to select. Let there be two human selectors; the one requiring a thing roughly cup-shaped, the other a thing sufficiently spherical

and of such a size as nearly to fit the bore of a gun. It is evident that their requirements may or may not be satisfied by searching among the pieces of clay, but *it is more likely that one of them would be satisfied than both.* Having obtained what they require, the selectors may think that their wants were supplied by selection from among things varying in all directions.

Let us keep in mind that in the above case it would be more likely for one selector to be satisfied than both if their choice lay among a finite number of variable things, and now let us discuss the supposed influence of selection upon organism in nature.

According to the selection theory there are in nature an infinite variety (number of kinds) of natural selectors, the conditions of life, to which organisms come to be suited by selection. We may speak of the conditions of life as so many separate natural selectors, for they are usually known to us as distinct influences. For example, a race comes under the influence of some disease or predatory enemy, those of the race that are protected by some accidental peculiarity will survive, being, so to speak, selected for survival by the hostile influence. Hence we may speak of the various conditions of life as separate influences and compare them to the artificial selectors in the above illustration. In nature, the number of kinds of natural selectors is infinite, for anything may happen to a race. We cannot think of the possible conditions as limited in number. Each one of this infinite number of selectors has its own special requirement or adapted attribute; just as the artificial selectors required the one a cup, the other a sphere. It cannot be denied that two selectors are less likely to be

satisfied than one, if the field of their choice is limited to a finite number of variable things; it follows from this that as the number of selectors grows larger and larger it becomes less and less likely that all should be satisfied. But according to the selection theory, an infinite number of natural selectors, new conditions of life, are lying in readiness to descend upon the race one or more at a time, and each one of this infinite number must find its requirement among the varying race, otherwise extinction will occur. It seems impossible for an infinite number of selectors to find their requirements among a finite number of variable things, hence it seems impossible for living things to obtain their attributes from their circumstances by means of Natural Selection.

There is a fallacy in the above argument; but if we expose the fallacy we strengthen the argument. The pieces of clay are not things varying in all directions, but in certain directions, determined by the fact that they were hand made. A piece bearing a depression large enough for it to be useful as a cup and another sufficiently spherical for use as a projectile were likely to be formed by the chance movements of fingers. Hence the two selectors were satisfied mainly because their choice lay among things that were not varying in every direction, strictly speaking. Conversely, if their search had been among things varying in every direction, assuming such chaos to be possible, it was even less likely to have been successful.

The essence of the selection theory is found in the following words: "If the race has been long under the same environment it is probable that only periodic selection is at work, maintaining its stability. Change the

environment and a secular change takes place, the deviations from the mode destroyed giving the requisite material." In much the same manner we may summarize our objection to the theory by considering the effect of circumstances upon a stable race as follows. During the time that the race is stable it is always overshadowed by the possibility of a serious change of circumstance. The change may be in any direction, that is to say, the first new circumstance that comes upon the race is merely one of an unlimited number which might have come upon it. Consequently, unless the race is to become extinct it must include amongst its deviations an unlimited number of different kinds of such deviations and presumably more than a few individuals of each kind, but how can a race, a finite number of living things, contain such a profusion of deviations ?

The selection theory was put forward chiefly to explain the adaptation of living things to their surroundings. It therefore rests on the assumption that an explanation of some kind is necessary. It was put forward as an alternative to the older explanation. If Anthropomorphism had never entered the mind of man, some of Darwin's teaching would have been meaningless. The selection theory therefore differs from many other scientific theories which are built upon new ground of fresh material provided by observation. The theory of the earth's rotundity does not depend upon the previous supposition that the earth was flat. But the selection theory depends upon a pre-existing theory, it borrows the idea that adaptation is a thing needing explanation.

Anthropomorphism in its simplest form must be of great antiquity. There must have been a moment long

ago when it first dawned in the mind of man. Probably this moment occurred not long after the first implements were made. The whole idea of purpose and design in nature must have come to man from a consideration of implements, the work of his own hands. Spears were made pointed in order to pierce. An animal's horns were seen to be pointed for a similar purpose. Perhaps the form of the weapon was suggested by that of the natural objects; but let that pass, it is evident that the idea of natural design arose out of artificial design. The weapon is pointed for a purpose, it was designed and made by man. A horn is also pointed for a purpose, therefore it also must have been designed by an agent, man-like in quality, though far more powerful than man, since it was able to create things which he could not hope to imitate. This argument from analogy has satisfied a large part of mankind in the past, and will continue to do so in future, but we may be almost sure that from the first it was not satisfactory to all men. As a result of the great freedom of thought and expression which is the special privilege of our times, it is evident that many writers are dissatisfied with the argument from analogy. Any one who is dissatisfied with the old explanation will adopt one of two attitudes. Either he will seek, like Darwin, for a new explanation, or he will not require an explanation of any kind. But is it possible to assume one or other attitude at will? It seems to me that there is no choice in a matter which depends on the innate quality of the mind. Opinion is to a great extent the expression of mental type, such types are distinctly appreciable, though difficult to define. From a general point of view there is no reason for regarding one type as better than

another. The feeling of rectitude is common to all types, and is present even in those classed as insane. Strength of the feeling of rectitude is not a mark by which we can recognize those types destined to be prevalent in the future. The future will unfold itself, it cannot be foreseen. The prevalent types of the future may be called the surviving types or the fittest types, the name is of no importance.

It seems that Darwin made fitness a primary cause, although it cannot be imagined by every one or seen by any one. By fitness is commonly meant a state of adaptation to the circumstances of life. There is, of course, no such thing as perfect adaptation, we can only use the term in a relative sense. Of two individuals, or of two races we may say, the one is fitter or better adapted to its circumstances than the other. But in either case how do we know which is the fitter of the two? We can only know by watching them in competition with another, there is no other way of deciding the question. The successful one is the fitter, but only for the reason that we apply the name of "the fitter" to the successful one. Fitness is not a reality in itself, it is only a term used to express the fact of survival.

This opinion must be carefully examined, some persons are convinced of its truth, but yet it is not widely accepted. Sociologists, as a rule, do not hold this opinion, and generally we find that the belief in fitness as a reality is widespread. Thus, regarding two classes A and B, sociologists observe that A is increasing and B is diminishing, but yet they speak of B as fitter than A. For example, it is the custom to divide mankind into three classes, upper, middle, and lower. Individuals fall

conventionally into these classes according to the amount of their possessions. The sociologist observes that the lower class, in some parts of the world, is increasing owing to their higher birth rate, while at the same time the middle class is diminishing. He also observes that the middle class is fitter than the lower class. Since he is able to observe the greater fitness of the dwindling class, it is evident that in his eyes fitness is an attribute apart from success and failure, that may be a concomitant of one or the other.

This argument is used here merely for an illustration. I do not believe for an instant that any essential human attribute is more common in the middle class than in any other class. There are different types and degrees of mental qualities, but they are to be seen in every section of society. It seems that the essential human qualities are not altered by nutrition or education, although they are various and changing.

Man has long observed success and failure and tried to explain them. Darwin taught that organisms succeed because of their fitness. But in substituting the word fitness for the older formula, he did not elucidate anything, unless it can be shown that fitness is an attribute in its own right.

The term fitness is used at present to express at least three separate ideas.

1. To express the adaptation of organisms, that is to say, a certain similarity between living things and those works of man that are adapted to a purpose.

2. To express an idea that springs from the egoistic side of the human mind, an idea that is in each mind in regard to itself and is present even in those classed as

insane. Fitness in this sense is implied, when one says that the middle class has more fitness than the lower class, and at the same time points to the latter as the surviving class.

3. As a word to express the fact of survival. From this point of view fitness is survival and nothing more.

The aim of this small book is to lessen the belief in Natural Selection as a creative agency. At one time it seemed that this belief was fast diminishing as a result of biological research. But there is little doubt about its vitality. It is still upheld by our leaders as a profound truth. New books designed for public instruction continue to appear which imply that Natural Selection is a creative agency and reiterate that no experienced naturalist doubts its efficacy, that such doubt would at once stamp the sceptic as inexperienced. These books are misleading, and from a certain point of view harmful.

I have no wish to urge the magnitude of my experience, but I will mention one phase of it. In the year 1892 I first heard of Natural Selection while attending the lectures of the late Professor W. F. R. Weldon. It was he who first pointed out to me that survival is the only test of fitness. From this follows the idea that fitness or adaptation is nothing more than a word to express the fact of survival, so that the phrase "survival of the fittest" becomes "the survival of the survivors" and loses its value as a profound truth. Professor Weldon continued his search after Natural Selection with a rare enthusiasm until almost the day of his death. It was evident to his pupils that he did not consider the theory proved, for his whole energies were directed towards

proving it, but it was no less evident that he, with many others, *wished it to be proved*. This wish accounts for the vitality of the idea called Natural Selection. Partiality or repugnance to it, are innate. It cannot be judged like other theories. Various writers reiterate, " We must not throw it over, what else have we ? " They say this as though it were an argument, whereas it is merely a statement of a certain fondness.

The desire for a philosophy, or explanation of life, cannot of course be decried. The intuitive perception of an Unknown Cause, *i.e.* some influence outside the limit of man's senses is fundamental in the human mind. This perception is common to all. But some persons hold that this influence is utterly incomprehensible. Consequently to them any teaching, that professes to be an explanation of it, is repugnant.

From this view point, both Anthropomorphism and the theory of Natural Selection are attempts to explain the unknowable. Though worthy of respect as being satisfying to sections of humanity, these theories are not universal truths. They are generally considered to be utterly antagonistic to one another, but from this point of view they are essentially alike. Both theories appear to be derived from the same source. Both take their representation of the unknown from human affairs.

Anthropomorphism appears to be constructed from two important human attributes ; namely, ingenuity—the making of appliances adapted to a purpose foreseen, and autocracy, the one person compelling the many.

Let us also inquire into the source of the theory of Natural Selection and the reason for its wonderful influence at the present day. Within the last century human

affairs have changed somewhat. Ingenuity is still among us, stronger than ever. Autocracy also remains, but it is less evident than before. The spectacle of an individual compelling the multitude to do his bidding, without question, is less common now than in the Middle Ages.

But a new influence has appeared which seems to urge and guide our activity. It is our custom to speak of this influence as competition. The events, embraced in the term competition, are not of course new nor are they confined to mankind among living things. But competition has become the mainspring of human activity in many parts of the world. From our first day at school are we not urged to compete? Can we not point to a hundred institutions whose arrival was accompanied by the disappearance of others? Is it not our custom to say that such institutions were brought into being by competition?

To a world permeated with the spirit of competition the theory of Natural Selection appears as an axiom. Natural Selection, the survival of the fittest, the struggle for existence, open competition, all express the same idea which may well be called the spirit of the age, just as the unquestioned domination of individuals was the spirit of the Middle Ages.

Thus the source of the theory of Natural Selection, like the source of Anthropomorphism, seems to be in human affairs. These theories do not teach us anything concerning the Unknown Cause. They appear to be temporary phases in mental evolution rather than final truths.

It may seem that, by my own showing, the theory of Natural Selection should not be opposed. It is

attractive to some though repugnant to others. Why decry it? some may ask. But Natural Selection is more than a mental comfort, it is often used as a justification for conduct. Anthropomorphism has been used in the past much in the same manner. We are familiar in history with the phrase "the divine right of kings," *i.e.* the sacredness of hereditary right which was used as a plea till the seventeenth century. At the present day this plea finds a parallel in what may be called the right of the spirit of competition which is daily invoked in order to smother those altruistic feelings that are an important part of the human mind.

THE END

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