V.—DISCONTINUITY IN EVOLUTION.

By Francis Galton.

Students of the laws of variation need not be disheartened by the apparent impossibility of learning the details by which particular variations are occasioned. We may take it for granted that the offspring are as little likely to be identical with their parents as it is for a colony to strictly resemble its parent state. The forms of living beings are presumably governed by laws as rigid as those of crystallisation, but the complexity of circumstance under which each germinal element is placed, and the multitude of interacting elements, make the effect of the forces that shape each living creature incomparably more various than those that shape the crystal. It is therefore not to be expected that offspring should exactly resemble their parents; it would, on the contrary, be very wonderful if they did so. The difficulty of being unable to account distinctly for the cause of any particular variation, may then be set aside by those who study the degree and the character of variation generally, as well as the circumstances under which a new variation may have become an established breed.

The amount of information collected by Darwin on these points in his *Plants and Animals under Domestication* is marvellously great; but as Mr. Bateson justly insists in his recent work,[1] to which attention will be drawn later on, there has as yet been no serious attempt on a large scale to add to his array of facts. It is time indeed to do so, and then to discuss them in the light of the larger knowledge and with the wider views which we have gained through Darwin's unflagging industry, and his fertile and powerful brain.

Use will be made in the following remarks of the two words *race* and *type*, or of some expression dependent on the latter, such as *typical centre;* consequently the sense in which they will be used must be defined. A race is taken to mean a large body of more or less similar and related individuals, who are separated from analogous bodies by the rarity of transitional forms, and not by any sharp boundary. This characteristic peculiarity of a race may be likened to

[1] *Materials for the Study of Variation, treated with especial regard to discontinuity in the origin of species.* Macmillan, 1894. Pp. xv., 598.

that of nebulæ, which are conspicuous through the brightness of their cores, and fade away into nothingness at a short distance from them, and therefore have no outline. The type, or typical centre of a race, corresponds in the above metaphor to the innermost core of the nebula. It is to be defined as an ideal form, whose qualities are those of the average of all the members of the race, or, what statistically speaking is the same thing, the average of any large and hap-hazard collection of them. Every race contains numerous individuals who differ very slightly and perhaps inappreciably from the central type, but it is scarcely conceivable that any individual should exist who is a mathematically exact representation of that purely ideal form. The number of individuals who deviate more or less markedly from the central type diminishes rapidly as the divergence increases, according to some law peculiar to each race and quality, but which approximates, in some instances very closely, to the theoretical law of frequency of error. The law in all cases must involve a constant, whose function is to express the degree in which the system is spread out, while preserving its relative internal proportions as defined by the law. The value of the constant is given by the distance, D, from the typical centre, within which a certain proportion, say one-half, of all the individuals, or of any large and hap-hazard collection of them, are found to be comprised. Knowing position of the typical centre, the law of frequency, and the constant applicable to the character in question, the race is defined so far as that characteristic is concerned, for we can easily calculate from those materials the number of individuals who are comprised within the distances 2 D, 3 D, &c., respectively from the core. Thus we see that the idea to be attached to the word race has three distinct and definite elements, any of which may be separately discussed in respect to any of its characteristics. They are (1) the typical centre, (2) the law of distribution about it, (3) the constant involved in that law.

The first of these chiefly interests us now; for we have to consider the ways in which the position of the typical centre of a race may become changed. At a certain period its position was A; at a second and long subsequent period it was B; by what steps did A change into B? Was it necessarily through the accumulation of a long succession of alterations, individually so small as to be almost imperceptible, though large and conspicuous in the aggregate, or could there ever have been abrupt changes?

A specious and it may be a very misleading argument in

favour of the steps being always small, is derived from the observed fact that specimens can usually be found ranging between A and B, each differing from its predecessor in only a slight degree. The inference is that the course of evolution followed those steps. But there is nothing to show that the specimens were typical forms of the race at the time when they were alive. Two that approach each other closely in appearance may be fundamentally different in nature, the one being a variant of A in the direction of B, and the other a variant of B in the direction of A. Though alike outwardly they differ inwardly, as shown by their offspring, which will 'regress' towards the A and B types respectively. The offspring of the variant of A do not deviate *on the average* so widely from the typical centre of A as their parents did. Some may deviate more, but the majority will deviate less. Similarly as regards the variant of B. So, although the pairs of parents may be outwardly alike, the successive generations of their offspring will differ increasingly, and their separation into representatives of A and B respectively will very soon become obvious. There can be no doubt as to the reality of regression. I have not only proved its existence in certain cases and measured its amount, but have shown that no race could continue constant in its characteristics unless regression existed. And, again, the observed and the theoretical details of the process were found to strictly concur. Therefore, although a museum may contain a full series of intermediate forms between A and B it does not in the least follow that the course of development passed through those forms.

The causes why the A and B races are such definite entities may be various. In the first place each race has a solidarity due to common ancestors and frequent interbreeding. Secondly, it may be thought by some, though not by myself, to have been pruned into permanent shape by the long-continued action of natural selection. But, in addition to these, I have for some years past maintained that a third cause exists more potent than the other two, and sufficient by itself to mould a race, namely that of definite positions of organic stability. The type A is stable, and so is the type B, but intermediate positions are less stable; therefore I conceive the position of maximum stability to be the essential as well as the most potent agent in forming a typical centre, from which the individuals of the race may diverge and towards which their offspring tend on the whole to regress.

Let us take some instances from Darwin's *Plants and Animals under Domestication* to serve as examples of what I call positions of organic stability. The Peacock, as he tells us, has hardly varied under domestication otherwise than being sometimes white or piebald, except in the following rare and curious particular, namely, the occasional appearance in England of the "japanned" or black-shouldered kind. It was considered by Mr. Sclater to be a distinct species, and named by him *Pavo nigripennis*. Its males differ conspicuously and in many respects from those of the common bird, while the females differ through being much paler. These japanned birds appear unexpectedly from eggs laid by the common kind, nevertheless they propagate their breed quite truly. Seven well-authenticated cases are given of their abrupt appearance in the broods of ordinary peafowl. In two of them, the black-shouldered kind, though it is a smaller and weaker bird, increased "to the extinction of the previously existing breed". Darwin concludes his remarks upon the large body of evidence that he adduces about them, by saying "these facts seem to me to indicate that the japanned peacock is a strongly marked variety or 'sport,' which tends at all times and in many places to reappear". As to the Peach, no less than six named and several unnamed varieties of the peach have suddenly produced several varieties of nectarine. The evidence of this is superabundant. There are in addition a few records of trees producing fruit which were individually half a pure peach and half a pure nectarine, or on which some of the fruit were pure peaches and the rest pure nectarines.

Many, if not most breeds, have had their origin in sports. A famous horticulturist, Vilmorin, quoted by Darwin, emphatically states that when any particular variation is desired, the first step is to get the plant to vary in any manner whatever, and to go on selecting the most variable individuals, even though they vary in the wrong direction; for the fixed character of the species being once broken, the desired variation will appear sooner or later. Horticulturists seem generally to agree with the view that the chief difficulty in producing new varieties is to break through the original form and colour of the species. There is nothing to be seen in the plant to show that the stability of its organisation is lessening; the fact is known only by its consequences.

Lastly, I will quote Darwin's important generalisation, that though the numerous animals and plants which have given rise to sports are known to have been separated from any common progenitor by a vast number of generations,

and though they have been reared under diversified conditions, the varieties they have severally yielded are closely analogous. In other words, the competing positions of organic stability are well defined and few in number.

Notwithstanding a multitude of striking cases of the above description collected by Darwin, the most marked impression left on his mind by the sum of all his investigations was the paramount effect of the accumulation of a succession of petty differences through the influence of natural selection. This is certainly the prevalent idea among his successors at the present day, with the corollary that the Evolution of races and species has always been an enormously protracted process. I have myself written many times during the last few years in an opposite sense to this, more especially in three works: *Natural Inheritance*, 1889, in *Finger Prints*, 1892, and in the preface to a reprint of *Hereditary Genius*, 1892, and will briefly recapitulate part of what was then more fully expressed. Mention was made in *Finger Prints* of the existence of certain definite forms, few in number, which appear again and again in the majority of men and women. They are the curious patterns formed by the papillary ridges on the bulbs of the fingers. It was shown on ample evidence that they are the most persistent of all the external characters that have yet been noted, and are consequently not unimportant in spite of their minute character. (We know nothing by observation about the persistence of any internal character, because it is not feasible to dissect a man in his boyhood, and a second or third time in his after life, whereas finger prints can be taken as often as is desired.) It was also shown that notwithstanding the early appearance of the patterns in fœtal life and their apparent importance, they are totally independent of any quality upon which either natural selection or marriage selection can be conceived to depend. For example, I find the same general run of patterns in English, Welsh, Jews, Basques, Hindoos, Negroes, men of culture, farm labourers, criminals, and idiots. I have failed to observe the slightest correlation between the patterns and any single personal quality whether physical or mental. They are therefore to be looked upon as purely local peculiarities, with a slight tendency towards transmission by inheritance. Yet notwithstanding their immunity from the influence of selection, they fall into three definite and widely different classes, each of which is a true race in the sense in which that word was defined, transitional forms between them being rare and the typical forms being frequent, while the frequency of devia-

tions from the several typical centres in those respects in which measurement could be applied, correspond approximately with the normal law of frequency. I therefore insisted that the continual appearance of these well-marked and very distinct patterns proved the reality of the alleged positions of organic stability, and that the latter were competent to mould races without any help whatever from the process of selection, whether natural or sexual.

A single fresh case shall be now introduced, merely for the purpose of varying the character of the evidence of sports, namely, that of Inaudi, the mental arithmetician. He has been tested very thoroughly in France, and been the subject of an extremely interesting report. I also had the pleasure of seeing him in England, and of testing his powers quietly in the company of a few friends. It appears that he had a passion for performing simple sums when his condition was no higher than that of an illiterate Piedmontese peasant boy. He gained *sous* by showing his arithmetical powers at *cabarets* before he had learnt even to read, an accomplishment which was deferred until opportunity for acquiring it arose in his youth (he being still a young man). So he had not even the advantage afforded by a visual memory of picturing a black-board in his imagination, upon which the sums could be mentally seen. I think that this limitation of his mental equipment, which makes his achievements still more extraordinary, was sufficiently proved by the following experiment. Two rows of figures, one of 18 and the other of 17 in number, were read out to him, and he was to subtract the latter from the former. (I have not access at this moment to my notes, and writing from memory it is possible that in the fear of overestimating I may have a little understated the number of figures.) He repeated them in order to make sure that he rightly understood what had been said, then he subtracted the one row from the other, mentally. After a little conversation and testing in other ways, we returned to the same figures, and he was asked to recall and repeat the whole sum backwards; this he did correctly but slowly. Then, after another interlude of conversation, he was requested to repeat the figures in columns. This also he did correctly, but much more slowly than before. The conclusion was that he did *not* see the figures mentally as written, say, in chalk on a black-board. Had he done so, it would have been equally easy to him to read them off in any order we asked for, whether forwards, backwards, or in columns. His parents had no such power; his own remarkable gifts were therefore a

sport, and let it be remembered that mental sports of this kind, however large, are none the less heritable. As we are speaking of the faculty of mental arithmetic, it is better to keep to it for illustration; so I will adduce in evidence of its hereditary persistence the well-known case of Mr. Bidder, the "calculating boy" of a past generation, whose son, the present Q.C., and many of whose grandchildren exhibit strong powers of the same kind.

What has been said about this particular gift of mental arithmetic is equally applicable to every other faculty, such as music and scholarship. Can anybody believe that the modern appearance in a family of a great musician is other than a sport? Is it conceivable that Sebastian Bach derived his musical gifts by atavism, and therefore ultimately from an anthropoid ape? The question is too absurd to answer.

The phrase of organic stability must not as yet be taken to connote more than it actually denotes. Thus far it has been merely used to express the well-substantiated fact that a race does sometimes abruptly produce individuals who have a distinctly different typical centre, in the sense in which those words were defined. The inference or connotation is that no variation can establish itself unless it be of the character of a sport, that is, by a leap from one position of organic stability to another, or as we may phrase it, through '*transilient*' variation. If there be no such leap the variation is, so to speak, a mere bend or divergence from the parent form, towards which the offspring in the next generation will tend to regress; it may therefore be called a '*divergent*' variation. Thus the unqualified word variation comprises and confuses what I maintain to be two fundamentally different processes, that of transilience and that of divergence, and its use destroys the possibility of reasoning correctly in not a few important matters. The interval leapt over in a transilience may be at least as large as it has been in any hitherto observed instance, and it may be smaller in any less degree. Still, whether it has been large or small, a leap has taken place into a new position of stability. I am unable to conceive the possibility of evolutionary progress except by transiliences, for, if they were merely divergences, each subsequent generation would tend to regress backwards towards the typical centre, and the advance that had been made would be temporary and could not be maintained. But what is transilience and what is divergence, physiologically speaking? As we know nothing about the arrangements and movements of the ultimate living units of the germs we can only answer by analogies. The exact answer

would require a knowledge of the cause of what, in the nomenclature of Weismann, would be called the architecture of the *id*, and of which he assumes the existence, but does not attempt to account for. We know that the germ contains the seeds of a vast number of ancestral potentialities, only a very few of which can be simultaneously developed, being to a great extent mutually exclusive. It may therefore be inferred with confidence, that organisation is reached through a succession of struggles for place among competing elements, the successful ones owing their success through position, through superiority in vigour, and so on; while these owe their existence in part to a host of what, in popular language, are called accidental causes. However vague such an explanation may be, it is far from being an inefficient one, for it defines the general character of a process though avowedly incapable of dealing with the details. It applies, moreover, to every theory of heredity which is of a "*particulate*" character;—that is to say, wherever the theory is based on the supposition of a vast number of partly independent biological particles, whose mutual attractions or repulsions, as they successively ripen, result in organisation. Theories that have this general idea for their foundation seem to be the only ones that are in any way defensible, and to all of these the idea of positions of organic stability is applicable.

The analogies that I have published in *Natural Inheritance*, in which gatherings of all kinds fall into positions of stability, are striking; but I cannot compress them further and there is not space for their recapitulation. Suffice it to say that they abound, and that the lists I have given of them might be almost indefinitely extended.

These briefly are the views that I have put forward in various publications during recent years, but all along I seemed to have spoken to empty air. I never heard nor have I read any criticism of them, and I believed they had passed unheeded and that my opinion was in a minority of one. It was, therefore, with the utmost pleasure that I read Mr. Bateson's work bearing the happy phrase in its title of 'discontinuous variation,' and rich with many original remarks and not a few trenchant expressions. I do not profess to review the book here; that should be done by others in a cooler and more cautious spirit, perhaps, than I can command, and with vastly more zoological knowledge than I possess, but I will briefly touch on a few salient points.

Mr. Bateson puts the problem clearly as follows: Evolution implies transition from one form to another by means

of a progressive series. If the whole series were before us should we find that this transition had been brought about by very minute and insensible differences between successive terms in the series, or should we find distinct and palpable gaps? In proportion as the transition from term to term is minimal and imperceptible, we may speak of the series as being *continuous*, while in proportion as there appear lacunæ, filled by no transitional form, we may describe it as *discontinuous*.

He shows with force the extreme difficulty of solving the problem by the methods ordinarily relied on. First, he says that the *embryological* evidence of evolution is little more than suggestive. Allowance has to be made when using it "for the omission of stages, for the intercalation of stages, for degeneration, for the presence of organs specially connected with larval life or embryonic life, for the interference of yolk and so forth. But what this allowance should be and in what cases it should be made has never been determined. More than this: closely allied forms often develop on totally different plans, . . . for example, . . . the germinal layers of the Guinea-pig when compared with those of the Rabbit are completely inverted, and so on" (p. 9).

Secondly, he shows with no less force the hopelessness of arriving at sure data, from the facts of *Adaptation*. He explains that large groups of common phenomena exist, for the use of which no one has yet made even a plausible surmise—"the study of adaptation ceases to help us at the exact point at which help is most needed. Darwin and many others have pointed out that the characters which visibly differentiate species are not, as a rule, capital facts in the constitution of vital organs, but more often they are just those features which seem to us useless and trivial. . . . These differences are often complex and are strikingly constant, but their utility is in almost every case problematical. . . . In the early days of the theory of natural selection, it was hoped that with searching the direct utility of such small differences would be found, but time has been running now and the hope is unfulfilled" (p. 11). He shows that the cardinal objection to the method is that, while it is generally possible to suggest *some* way by which any given structure may be of use to the animal who possesses it, it is by no means easy to prove that the structure is *on the whole* useful or harmful. A quantitative estimate of the value of each peculiarity is wanted, which, in the face of the complexity of the relations between an animal and its surroundings, is scarcely possible to be obtained in any single case.

Then he proceeds to show that the study of *Variation* gives us the only apparent chance of advancing our knowledge of the principles of evolution. To do this is the laudable object of Mr. Bateson's volume, from which a few classes of fact will now be selected for illustration.

There are numerous genera of the Lamellicorn family of beetles, in which the males are known as "high" and "low" according to the length of their horns. A careful study was made of 342 specimens, and it was found that the two groups of high and low behaved as the members of two races, each sort having its own typical centre, precisely of the kind I described when defining the word race. These two sorts were separated by no hard and fast line but by an interval of scarcity.

Exactly the same occurred in respect to Earwigs, one form of them having their forceps of much greater length than the other. Out of 583 males, 124 had a forceps of $3\frac{1}{2}$ millimetres long, 88 had one of 7 millimetres, while there were only 7 whose forceps was $4\frac{1}{2}$ millimetres.

Cases of this kind fall under Mr. Bateson's category of substantive variations, or those in which the unit itself varies. The other great class is that of "meristic" variations, in which the unit is unchanged, but the number of units varies. An excellent example of the latter is found in the number of joints in the leg of the Cockroach. About one quarter of these creatures have four joints in the tarsus and the remainder have five joints. The length of the leg may be the same in both; the only material difference being in the number of joints. In either case the leg is perfect of its kind, without any sign of deformity; moreover, in either case there proves to be a typical form from which variants in different degrees occur with a frequency approximately conformable to that of the normal law of error. The book is full of instances of discontinuity. In one class of them the colour of cats is included. These animals are notoriously promiscuous in their interbreeding, yet the result is that they show very distinct types of coloration, pure specimens of tabby, tortoise-shell, black, grey, white, and piebald being frequent.

Mr. Bateson has thus far been more desirous to collect facts than to formulate theories, and is laudably cautious of committing himself too far. The following paragraph is one of the few in which he 'lets himself go,' and far be it from me to do otherwise than agree altogether with it. "The belief that all distinctness is due to natural selection, and the expectation that apart from natural selection there would be a general level of confusion, agrees ill with the facts of

variation. We may doubt indeed whether the ideas associated with that flower of speech, 'Panmixia,' are not as false to the laws of life as the word to the laws of language" (p. 573).

Inquiries are greatly needed into the discontinuous variations of human faculty, a subject untouched upon by Mr. Bateson in the present instalment of his most valuable work. The assurance that sports of considerable magnitude occasionally occur in moral and intellectual gifts, justifies more daring speculations than we are apt to indulge, in respect both to the past and future history of mankind. It does not seem to me by any means so certain as is commonly supposed by the scientific men of the present time, that our evolution from a brute ancestry was through a series of severally imperceptible advances. Neither does it seem by any means certain that humanity must linger for an extremely long time at or about its present unsatisfactory level. As a matter of fact, the Greek race of the classical times have surpassed in natural faculty all other races before or since, and some future race may be at least the equal of the Greek, while it is reasonable to hope that when the power of heredity and the importance of preserving valuable "transiliencies" shall have become generally recognised, effective efforts will be made to preserve them.