THE INADEQUACY OF "NATURAL SELECTION."

STUDENTS of psychology are familiar with the experiments of Weber on the sense of touch. He found that different parts of the surface differ widely in their ability to give information concerning the things touched. Some parts, which yielded vivid sensations, yielded little or no knowledge of the size or form of the thing exciting it; whereas other parts, from which there came sensations much less acute, furnished clear impressions respecting tangible characters, even of relatively small objects. These unlikenesses of tactual discriminativeness he ingeniously expressed by actual measurements. Taking a pair of compasses, he found that if they were closed so nearly that the points were less than one-twelfth of an inch apart, the end of the forefinger could not perceive that there were two points: the two points seemed one. But when the compasses were opened so that the points were one-twelfth of an inch apart, then the end of the forefinger distinguished the two points. On the other hand, he found that the compasses must be opened to the extent of two and a half inches before the middle of the back could distinguish between two points and one. That is to say, as thus measured, the end of the forefinger has thirty times the tactual discriminativeness which the middle of the back has.

Between these extremes he found gradations. The inner surfaces of the second joints of the fingers can distinguish separateness of positions only half as well as the tip of the forefinger. The innermost joints are still less discriminating, but have a power of discrimation equal to that of the tip of the nose. The end of the great toe, the palm of the hand, and the cheek, have alike one-fifth of the perceptiveness which the tip of the forefinger has; and the lower part of the forehead has but one-half that possessed by the cheek.

The back of the hand and the crown of the head are nearly alike in having but a fourteenth or a fifteenth of the ability to perceive positions as distinct, which is possessed by the finger-end. The thigh, near the knee, has rather less, and the breast less still; so that the compasses must be more than an inch and a half apart before the breast distinguishes the two points from one another.

What is the meaning of these differences? How, in the course of evolution, have they been established? If "natural selection" or survival of the fittest is the assigned cause, then it is required to show in what way each of these degrees of endowment has advantaged the possessor to such extent that not infrequently life has been directly or indirectly preserved by it. We might reasonably assume that in the absence of some differentiating process, all parts of the surface would have like powers of perceiving relative positions. They cannot have become widely unlike in perceptiveness without some cause. And if the cause alleged is natural selection, then it is necessary to show that the greater degree of the power possessed by this part than by that, has not only conduced to the maintenance of life, but has conduced so much that an individual in whom a variation had produced better adjustment to needs, thereby maintained life when some others lost it; and that among the descendants inheriting this variation, there was a derived advantage such as enabled them to multiply more than the descendants of individuals not possessing it. Can this, or anything like this, be shown?

That the superior perceptiveness of the forefinger-tip has thus arisen, might be contended with some apparent reason. Such perceptiveness is an important aid to manipulation, and may have sometimes given a life-saving advantage. In making arrows or fish-hooks, a savage possessing some extra amount of it may have been thereby enabled to get food where another failed. In civilised life, too, a sempstress with well-endowed finger-ends might be expected to gain a better livelihood than one with finger-ends which were obtuse; though this advantage would not be so great as appears. I have found that two ladies whose finger-ends were covered with glove-tips, reducing their sensitiveness from one-twelfth of an inch between compass points to one-seventh, lost nothing appreciable of their quickness and goodness in sewing. An experience of my own here comes in evidence. Towards the close of my salmon-fishing days, I used to observe what a bungler I had become in putting on and taking off artificial flies. As the tactual discriminativeness of my finger-ends, recently tested, comes up to the standard specified by Weber, it is clear that this decrease of manipulative power, accompanying increase of age, was due to decrease in the delicacy of muscular co-ordination and sense of pressure—not to decrease of tactual discriminativeness. But not making much of these criticisms, let us admit the conclusion that this high perceptive power possessed by the forefinger-end may have arisen by survival of the fittest; and let us limit the argument to the other differences.

How about the back of the trunk and its face? Is any advantage derived from possession of greater tactual discriminativeness by the last than by the first? The tip of the nose has more than three times the power of distinguishing relative positions which the lower part of the forehead has. Can this greater power be shown to have any advantage? The back of the hand has scarcely more discriminative ability than the crown of the head, and has only one-fourteenth of that which the finger-tip has. Why is this? Advantage might occasionally be derived if the back of the hand could tell us more than it does about the shapes of the surfaces touched. Why should the thigh near the knee be twice as perceptive as the middle of the thigh? And, last of all, why should the middle of the forearm, middle of the thigh, middle of the back of the neck, and middle of the back, all stand on the lowest level, as having but one-thirtieth of the perceptive power which the tip of the forefinger has? To prove that these differences have arisen by natural selection, it has to be shown that such small variation in one of the parts as might occur in a generation—say one-tenth extra amount—has yielded an appreciably greater power of self-preservation, and that those inheriting it have continued to be so far advantaged as to multiply more than those who, in other respects equal, were less endowed with this trait. Does any one think he can show this?

But if this distribution of tactual perceptiveness cannot be explained by survival of the fittest, how can it be explained? The reply is that, if there has been in operation a cause which it is now the fashion among biologists to ignore or deny, these various differences are at once accounted for. This cause is the inheritance of acquired characters. As a preliminary to setting forth the argument showing this, I have made some experiments.

It is a current belief that the fingers of the blind, more practised in tactual exploration than the fingers of those who can see, acquire greater discriminativeness: especially the fingers of those blind who have been taught to read from raised letters. Not wishing to trust to this current belief, I recently tested two youths, one of fifteen and the other younger, at the School for the Blind in Upper Avenue Road, and found the belief to be correct. Instead of being unable to distinguish between points of the compasses until they were opened to one-twelfth of an inch apart, I found that both of them could distinguish between points when only one-fourteenth of an inch apart. They had thick and coarse skins; and doubtless, had this intervening obstacle so produced been less, the discriminative power would have been greater. It afterwards occurred to me that a better test would be furnished by those whose finger-ends are exercised in tactual perceptions, not occasionally, as by the blind in reading, but all day long in pursuit of their occupations. The facts answered expectation. Two skilled compositors, on whom I experimented, were both able to distinguish between points when they were only one-seventeenth of an inch apart. Thus we have clear proof that constant exercise of the tactual nervous structures leads to further development.*

Now if acquired structural traits are inheritable, the various contrasts above set down are obvious consequences; for the gradations in tactual perceptiveness correspond with the gradations in the tactual exercises of the parts. Save by contact with clothes, which present only broad surfaces having but slight and indefinite contrasts, the trunk has but little converse with external bodies, and it has but small discriminative power; but what discriminative power it has is greater on its face than on its back, corresponding to the fact that the chest and abdomen are much more frequently explored by the hands: this difference being probably in part inherited from inferior creatures, for, as we may see in dogs and cats, the belly is far more accessible to feet and tongue than the back. No less obtuse than the back are the middle of the back of the neck, the middle of the forearm, and the middle of the thigh; and these parts have but rare experiences of irregular foreign bodies. The crown of the head is occasionally felt by the fingers, as also the back of one hand by the fingers of the other; but neither of these surfaces, which are only twice as perceptive as the back, is used with any frequency for touching objects, much less for examining them. The lower part of the forehead, though more perceptive than the crown of the head, in correspondence with a somewhat greater converse with the hands, is less than one-third as perceptive as the tip of the nose; and manifestly, both in virtue of its relative prominence, in virtue of its contacts with things smelt at, and in virtue of its frequent acquaint-

* Let me here note in passing a highly significant implication. The development of nervous structures which in such cases takes place, cannot be limited to the finger-ends. If we figure to ourselves the separate sensitive areas which severally yield independent feelings, as constituting a network (not, indeed, a network sharply marked out, but probably one such that the ultimate fibrils in each area intrude more or less into adjacent areas, so that the separations are indefinite), it is manifest that when, with exercise, the structure has become further elaborated, and the meshes of the network smaller, there must be a multiplication of fibres communicating with the central nervous system. If two adjacent areas were supplied by branches of one fibre, the touching of either would yield to consciousness the same sensation: there could be no discrimination between points touching the two. That there may be discrimination, there must be a distinct connection between each area and the tract of grey matter which receives the impressions. Nay more, there must be, in this central recipient-tract, an added number of the separate elements which, by their excitement, yield separate feelings. So that this increased power of tactual discrimination implies a peripheral development, a multiplication of fibres in the trunk-nerve, and a complication of the nerve-centre. It can scarcely be doubted that analogous changes occur under analogous conditions throughout all parts of the nervous system—not in its sensory appliances only, but in all its higher co-ordinating appliances up to the highest.

ance with the handkerchief, the tip of the nose has far greater tactual experience. Passing to the inner surfaces of the hands, which, taken as wholes, are more constantly occupied in touching than are the back, breast, thigh, forearm, forehead, or back of the hand, Weber's scale shows that they are much more perceptive, and that the degrees of perceptiveness of different parts correspond with their tactual activities. The palms have but one-fifth the perceptiveness possessed by the fore-finger-ends; the inner surfaces of the finger-joints next the palms have but one-third, while the inner surfaces of the second joints have but one-half. These abilities correspond with the facts that whereas the inner parts of the hand are used only in grasping things, the tips of the fingers come into play not only when things are grasped, but when such things, as well as smaller things, are felt at or manipulated. It needs but to observe the relative actions of these parts in writing, in sewing, in judging textures, &c., to see that above all other parts the finger-ends, and especially the forefinger-ends, have the most multiplied experiences. If, then, it be that the extra perceptiveness acquired from extra tactual activities, as in a compositor, is inheritable, these gradations of tactual perceptiveness are explained.

Doubtless some of those who remember Weber's results, have had on the tip of the tongue the argument derived from the tip of the tongue. This part exceeds all other parts in power of tactual discrimination: doubling, in that respect, the power of the forefinger-tip. It can distinguish points that are only one-twenty-fourth of an inch apart. Why this unparalleled perceptiveness? If survival of the fittest be the ascribed cause, then it has to be shown what the advantages achieved have been; and, further, that those advantages have been sufficiently great to have had effects on the maintenance of life.

Besides tasting, there are two functions conducive to life, which the tongue performs. It enables us to move about food during mastication, and it enables us to make many of the articulations constituting speech. But how does the extreme discriminativeness of the tongue-tip aid these functions? The food is moved about, not by the tongue-tip, but by the body of the tongue; and even were the tip largely employed in this process, it would still have to be shown that its ability to distinguish between points one-twenty-fourth of an inch apart, is of service to that end, which cannot be shown. It may, indeed, be said that the tactual perceptiveness of the tongue-tip serves for detection of foreign bodies in the food, as plum-stones or as fish-bones. But such extreme perceptiveness is needless for the purpose—a perceptiveness equal to that of the finger-ends would suffice; and further, even were such extreme perceptiveness useful, it could not have caused survival of individuals who possessed it in slightly higher degrees than others. It needs but to observe a dog crunching small bones, and swallowing with impunity the sharp-angled pieces, to see that but a very small amount of mortality would be prevented.

But what about speech? Well, neither here can there be shown any advantage derived from this extreme perceptiveness. For making the *s* and *z*, the tongue has to be partially applied to a portion of the palate next the teeth. Not only, however, must the contact be incomplete, but its place is indefinite—may be half an inch further back. To make the *sh* and *zh*, the contact has to be made, not with the tip, but with the upper surface of the tongue; and must be an incomplete contact. Though, for making the liquids, the tip of the tongue and the sides of the tongue are used, yet the requisite is not any exact adjustment of the tip, but an imperfect contact with the palate. For the *th*, the tip is used along with the edges of the tongue; but no perfect adjustment is required, either to the edges of the teeth, or to the junction of the teeth with the palate, where the sound may equally well be made. Though for the *t* and *d* complete contact of the tip and edges of the tongue with the palate is required, yet the place of contact is not definite, and the tip takes no more important share in the action than the sides. Any one who observes the movements of his tongue in speaking, will find that there occur no cases in which the adjustments must have an exactness corresponding to the extreme power of discrimination which the tip possesses: for speech, this endowment is useless. Even were it useful, it could not be shown that it has been developed by survival of the fittest; for though perfect articulation is useful, yet imperfect articulation has rarely such an effect as to impede a man in the maintenance of his life. If he is a good workman, a German's interchanges of *b's* and *p's* do not disadvantage him. A Frenchman who, in place of the sound of *th*, always makes the sound of *z*, succeeds as a teacher of music or dancing, no less than if he achieved the English pronunciation. Nay, even such an imperfection of speech as that which arises from cleft palate, does not prevent a man from getting on if he is capable. True, it may go against him as a candidate for Parliament, or as an "orator" of the unemployed (mostly not worth employing). But in the struggle for life he is not hindered by the effect to the extent of being less able than others to maintain himself and his offspring. Clearly, then, even if this unparalleled perceptiveness of the tongue-tip is required for perfect speech, this use is not sufficiently important to have been developed by natural selection.

How, then, is this remarkable trait of the tongue-tip to be accounted for? Without difficulty, if there is inheritance of acquired characters. For the tongue-tip has, above all other parts of the body, unceasing experiences of small irregularities of surface. It is in contact with the teeth, and either consciously or unconsciously is continually exploring them. There is hardly a moment in which impressions of adjacent but different positions are not being yielded to it by either the surfaces of the teeth or their edges; and it is continually being moved about from some of them to others. No advantage is gained. It is simply that the tongue's position renders perpetual exploration almost inevitable; and by perpetual exploration is developed this unique power of discrimination. Thus the law holds throughout, from this highest degree of perceptiveness of the tongue-tip to its lowest degree on the back of the trunk; and no other explanation of the facts seems possible.

"Yes, there is another explanation," I hear some one say: "they may be explained by *panmixia*." Well, in the first place, as the explanation by *panmixia* implies that these gradations of perceptiveness have been arrived at by the dwindling of nervous structures, there lies at the basis of the explanation an unproved and improbable assumption; and, even were there no such difficulty, it may with certainty be denied that *panmixia* can furnish an explanation. Let us look at its pretensions.

It was not without good reason that Bentham protested against metaphors. Figures of speech in general, valuable as they are in poetry and rhetoric, cannot be used without danger in science and philosophy. The title of Mr. Darwin's great work furnishes us with an instance of the misleading effects produced by them. It runs:—"The Origin of Species by means of Natural Selection, or the preservation of Favoured Races in the Struggle for Life." Here are two figures of speech which conspire to produce an impression more or less erroneous. The expression "natural selection" was chosen as serving to indicate some parallelism with artificial selection—the selection exercised by breeders. Now selection connotes volition, and thus gives to the thoughts of readers a wrong bias. Some increase of this bias is produced by the words in the second title, "favoured races;" for anything which is favoured implies the existence of some agent conferring a favour. I do not mean that Mr. Darwin himself failed to recognise the misleading connotations of his words, or that he did not avoid being misled by them. In chapter iv. of the "Origin of Species" he says that, considered literally, "natural selection is a false term," and that the personification of Nature is objectionable; but he thinks that readers, and those who adopt his views, will soon learn to guard themselves against the wrong implications. Here I venture to think that he was mistaken. For thinking this there is the reason that even his disciple, Mr. Wallace—no, not his disciple, but his co-discoverer, ever to be honoured—has apparently been influenced by them. When for example, in combating a view of mine, he says that "the very thing said to be impossible by variation and natural selection has been again and again effected by variation and artificial selection"; he seems clearly to imply that the processes are analogous and operate in the same way. Now this is untrue. They are analogous only within certain narrow limits; and, in the great majority of cases, natural selection is utterly incapable of doing that which artificial selection does.

To see this it needs only to de-personalise Nature, and to remember that, as Mr. Darwin says, Nature is "only the aggregate action and product of many natural laws [forces]." Observe its relative shortcomings. Artificial selection can pick out a particular trait, and, regardless of other traits of the individuals displaying it, can increase it by selective breeding in successive generations. For, to the breeder or fancier, it matters little whether such individuals are otherwise well constituted. They may be in this or that way so unfit for carrying on the struggle for life, that, were they without human care, they would disappear forthwith. On the other hand, if we regard Nature as that which it is, an assemblage of various forces, inorganic and organic, some favourable to the maintenance of life and many at variance with its maintenance—forces which operate blindly—we see that there is no such selection of this or that trait, but that there is a selection only of individuals which are, by the aggregate of their traits, best fitted for living. And here I may note an advantage possessed by the expression "survival of the fittest;" since this does not tend to raise the thought of any one character which, more than others, is to be maintained or increased; but tends rather to raise the thought of a general adaptation for all purposes. It implies the process which Nature can alone carry on—the leaving alive of those which are best able to utilise surrounding aids to life, and best able to combat or avoid surrounding dangers. And while this phrase covers the great mass of cases in which there are preserved well-constituted individuals, it also covers those special cases which are suggested by the phrase "natural selection," in which individuals succeed beyond others in the struggle for life by the help of particular characters which conduce in important ways to prosperity and multiplication. For now observe the fact which here chiefly concerns us, that survival of the fittest can increase any serviceable trait only if that trait conduces to prosperity of the individual, or of posterity, or of both, *in an important degree*. There can be no increase of any structure by natural selection unless, amid all the slightly varying structures constituting the organism, increase of this particular one is so advantageous as to cause greater multiplication of the family in which it arises than of other families. Variations which, though advantageous, fail to do this, must disappear again. Let us take a case.

Keenness of scent in a deer, by giving early notice of approaching enemies, subserves life so greatly that, other things equal, an individual having it in an unusual degree is more likely than others to survive, and, among descendants, to leave some similarly endowed or more endowed, who again transmit the variation with, in some cases, increase. Clearly this highly useful power may be developed by natural selection. So also, for like reasons, may quickness of vision and delicacy of hearing. Though it may be remarked in passing that since this extra sense-endowment, serving to give early alarm, profits the herd as a whole, which takes the alarm from one individual, selection of it is not so easy, unless it occurs in a conquering stag. But now suppose that one member of the herd—perhaps because of more efficient teeth, perhaps by greater muscularity of stomach, perhaps by secretion of more appropriate gastric juices—is enabled to eat and digest a not uncommon plant which the others refuse. This peculiarity may, if food is scarce, conduce to better self-maintenance, and better fostering of young, if the individual is a hind. But unless this plant is abundant, and the advantage consequently great, the advantages which other members of the herd gain from other slight variations may be equivalent. This one has unusual agility and leaps a chasm which others balk at. That one develops longer hair in winter, and resists the cold better. Another has a skin less irritated by flies, and can graze without so much interruption. Here is one which has an unusual power of detecting food under the snow; and there is one which shows extra sagacity in the choice of a shelter from wind and rain. That the variation giving the ability to eat a plant before unutilised, may become a trait of the herd, and eventually of a variety, it is needful that the individual in which it occurs shall have more descendants, or better descendants, or both, than have the various other individuals severally having their small superiorities. If these other individuals severally profit by their small superiorities, and transmit them to equally large numbers of offspring, no increase of the variation in question can take place: it must soon be cancelled. Whether in the "Origin of Species" Mr. Darwin has recognised this fact, I do not remember, but he has certainly done it by implication in his "Animals and Plants under Domestication." Speaking of variations in domestic animals, he there says that, "Any particular variation would generally be lost by crossing, reversion, and the accidental destruction of the varying individuals, unless carefully preserved by man" (vol. ii. 292). That which survival of the fittest does in cases like the one I have instanced is to keep all faculties up to the mark, by destroying such as have faculties in some respect below the mark; and it can produce development of some one faculty only if that faculty is predominantly important. It seems to me that many naturalists have practically lost sight of this, and assume that natural selection will increase *any* advantageous trait. Certainly a view now widely accepted assumes as much.

The consideration of this view, to which the foregoing paragraph is introductory, may now be entered upon. This view concerns, not direct selection, but what has been called, in questionable logic, "reversed selection"—the selection which effects, not increase of an organ, but decrease of it. For as, under some conditions, it is of advantage to an individual and its descendants to have some structure of larger size, it may be, under other conditions—namely, when the organ becomes useless—of advantage to have it of smaller size; since, even if it is not in the way, its weight and the cost of its nutrition are injurious taxes on the organism. But now comes the truth to be emphasised. Just as direct selection can increase an organ only in certain cases, so can reversed selection decrease it only in certain cases. Like the increase produced by a variation, the decrease produced by one must be such as will sensibly conduce to preservation and multiplication. It is, for instance, conceivable that were the long and massive tail of the kangaroo to become useless (say by the forcing of the species into a mountainous and rocky habitat filled with brushwood), a variation which considerably reduced the tail might sensibly profit the individual in which it occurred; and, in seasons when food was scarce, might cause survival when individuals with large tails died. But the economy of nutrition must be considerable before any such result could occur. Suppose that in this new habitat the kangaroo had no enemies; and suppose that, consequently, quickness of hearing not being called for, large ears gave no greater advantage than small ones. Would an individual with smaller ears than usual survive and propagate better than other individuals in consequence of the economy of nutrition achieved? To suppose this is to suppose that the saving of a grain or two of protein per day would determine the kangaroo's fate.

Long ago I discussed this matter in the "Principles of Biology" (§ 166), taking as an instance the decrease of the jaw implied by the crowding of the teeth, and now proved by measurement to have taken place. Here is the passage:—

"No functional superiority possessed by a small jaw over a large jaw, in civilised life, can be named as having caused the more frequent survival of small-jawed individuals. The only advantage which smallness of jaw might be supposed to give, is the advantage of economised nutrition; and this could not be great enough to further the preservation of men possessing it. The decrease of weight in the jaw and co-operative parts that has arisen in the course of many thousands of years, does not amount to more than a few ounces. This decrease has to be divided among the many generations that have lived and died in the interval. Let us admit that the weight of these parts diminished to the extent of an ounce in a single generation (which is a large admission); it still cannot be contended that the having to carry an ounce less in weight, or the having to keep in repair an ounce less of tissue, could sensibly affect any man's fate. And if it never did this—nay, if it did not cause a *frequent* survival of small-jawed individuals where large-jawed individuals died, natural selection could neither cause nor aid diminution of the jaw and its appendages."

When writing this passage in 1864, I never dreamt that a quarter of a century later, the supposable cause of degeneration here examined and excluded as impossible, would be enunciated as not only *a* cause, but *the* cause, and the sole cause. This, however, has happened. Weismann's theory of degeneration by *panmixia*, is that when an organ previously maintained of the needful size by natural selection, is no longer maintained at that size, because it has become useless (or because a smaller size is equally useful), it results that among the variations in the size, which take place from generation to generation, the smaller will be preserved continually, and that so the part will decrease. And this is concluded without asking whether the economy in nutrition achieved by the smaller variation, will sensibly affect the survival of the individual, and the multiplication of its stirp. To make clear his hypothesis, and to prepare the way for criticism, let me quote the example he himself gives when contrasting the alleged efficiency of dwindling by *panmixia* with the alleged inefficiency of dwindling from disuse. This example is furnished him by the *Proteus.*

Concerning the "blind fish and amphibia" found in dark places, which have but rudimentary eyes "hidden under the skin," he argues that "it is difficult to reconcile the facts of the case with the ordinary theory that the eyes of these animals have simply degenerated through disuse." After giving instances of rapid degeneration of disused organs, he argues that if "the effects of disuse are so striking in a single life, we should certainly expect, if such effects can be transmitted, that all traces of an eye would soon disappear from a species which lives in the dark." Doubtless this is a reasonable conclusion. To explain the facts on the hypothesis that acquired characters are inheritable seems very difficult. One possible explanation may indeed be named. It appears to be a general law of organisation that structures are stable in proportion to their antiquity; that while organs of relatively modern origin have but a comparatively superficial root in the constitution, and readily disappear if the conditions do not favour their maintenance, organs of ancient origin have deep-seated roots in the constitution, and do not readily disappear. Having been early elements in the type, and having continued to be reproduced as parts of it during a period extending throughout many geological epochs, they are comparatively persistent. Now the eye answers to this description as being a very early organ.* But waiving possible interpretations, let us admit that here is a difficulty

* While the proof of this article is in hand, I learn that the *Proteus* is not quite blind, and that its eyes have a use. It seems that when the underground streams it inhabits are unusually swollen, some individuals of the species are carried out of the caverns into the open (being then sometimes captured). It is also said that the creature shuns the light; this trait being, I presume, observed when it is in captivity. Now obviously, among individuals carried out into the open, those which remain

—a difficulty like countless others which the phenomena of evolution present, as, for instance, the acquirement of such a habit as that of the *Vanessa* larva, hanging itself up by the tail and then changing into a chrysalis which usurps its place—a difficulty which, along with multitudes, has to await future solution, if any can be found. Let it be granted, I say, that here is a serious obstacle in the way of the hypothesis; and now let us turn to the alternative hypothesis, and observe whether it is not met by difficulties which are much more serious. Weissmann writes:—

"The caverns in Carniola and Carinthia, in which the blind *Proteus* and so many other blind animals live, belong geologically to the Jurassic formation; and although we do not exactly know when, for example, the *Proteus* first entered them, the low organisation of this amphibian certainly indicates that it has been sheltered there for a very long period of time, and that thousands of generations of this species have succeeded one another in the caves.

"Hence there is no reason to wonder at the extent to which the degeneration of the eye has been already carried in the *Proteus*, even if we assume that it is merely due to the cessation of the conserving influence of natural selection.

"But it is unnecessary to depend upon this assumption alone, for when a useless organ degenerates, there are also other factors which demand consideration—namely, the higher development of other organs which compensate for the loss of the degenerating structure, or the increase in size of adjacent parts. If these newer developments are of advantage to the species, they finally come to take the place of the organ which natural selection has failed to preserve at its point of highest perfection." *

On these paragraphs let me first remark that one cause is multiplied into two. The cause is stated in the abstract, and it is then re-stated in the concrete, as though it were another cause. Manifestly, if by decrease of the eye an economy of nutriment is achieved, it is implied that the economised nutriment is turned to some advantageous purpose or other; and to specify that the nutriment is used for the further development of compensating organs, simply changes the indefinite statement of advantage into a definite statement of advantage. There are not two causes in operation, though the matter is presented as though there were.

But passing over this, let us now represent to ourselves in detail this process which Professor Weismann thinks will, in thousands of generations, effect the observed reduction of the eyes: the process being that at each successive stage in the decrease, there must take place variations in the size of the eye, some larger, some smaller, than the size previously reached, and that in virtue of the economy, those

visible are apt to be carried off by enemies; whereas, those which, appreciating the difference between light and darkness, shelter themselves in dark places, survive. Hence the tendency of natural selection is to prevent the decrease of the eyes beyond that point at which they can distinguish between light and darkness. Thus the apparent anomaly is explained.

* "Essays upon Heredity," p. 87.

having the smaller will continually survive and propagate, instead of those having the larger. Properly to appreciate this supposition, we must use figures. To give it every advantage we will assume that there have been only two thousand generations, and we will assume that, instead of being reduced to a rudiment, the eye has disappeared altogether. What amounts of variation shall we suppose? If the idea is that the process has operated uniformly on each generation, the implication is that some advantage has been gained by the individuals having the eyes $\frac{1}{2000}$th less in weight; and this will hardly be contended. Not to put the hypothesis at this disadvantage, let us then imagine that there take place, at long intervals, decreasing variations considerable in amount—say $\frac{1}{20}$th, once in a hundred generations. This is an interval almost too long to be assumed; but yet if we assume the successive decrements to occur more frequently, and therefore to be smaller, the amount of each becomes too insignificant. If, seeing the small head, we assume that the eyes of the *Proteus* originally weighed some ten grains each, this would give us, as the amount of the decrement of $\frac{1}{20}$th, occurring once in a hundred generations, one grain. Suppose that this eel-shaped amphibian, about a foot long and more than half an inch in diameter, weighs three ounces—a very moderate estimate. In such case the decrement would amount to $\frac{1}{1440}$th of the creature's weight; or, for convenience, let us say that it amounted to $\frac{1}{1000}$th, which would allow of the eyes being taken at some fourteen grains each.* To this extent, then, each occasional decrement would profit the organism. The economy in weight to a creature having nearly the same specific gravity as its medium, would be infinitesimal. The economy in nutrition of a rudimentary organ, consisting of passive tissues, would also be but nominal. The only appreciable economy would be in the original building up of the creature's structures; and the hypothesis of Weismann implies that the economy of this thousandth part of its weight, by decrease of the eyes, would so benefit the rest of the creature's organisation as to give it an

* I find that the eye of a small smelt (the only appropriate small fish obtainable here, St. Leonards) is about $\frac{1}{180}$th of its weight; and since in young fish the eyes are disproportionately large, in the full-grown smelt the eye would be probably not more than $\frac{1}{200}$th of the creature's weight. On turning to highly-finished plates, published by the Bibliographisches Institut of Leipzig, of this perenni branchiate *Proteus*, and other amphibians, I find that in the nearest ally there represented, the caducibranchiate axolotl, the diameter of the eye, less than half that of the smelt, bears a much smaller ratio to the length of the body: the proportion in the smelt being $\frac{1}{30}$th of the length, and in the axolotl about $\frac{1}{60}$th (the body being also more bulky than that of the smelt). If, then, we take the linear ratio of the eye to body in this amphibian as one-half the ratio which the fish presents, it results that the ratio of the mass of the eye to the mass of the body will be but one-eighth. So that the weight of the eye of the amphibian will be but $\frac{1}{1600}$th of that of the body. It is a liberal estimate, therefore, to suppose that its original weight in the *Proteus* was 1000th of that of the body. I may add that any one who glances at the representation of the axolotl, will see that, were the eye to disappear entirely by a single variation, the economy achieved could not have any appreciable physiological effect on the organism.

appreciably greater chance of survival, and an appreciably greater multiplication of descendants. Does any one accept this inference?

Of course the quantifications of data above set down can be only approximate; but I think no reasonable changes of them can alter the general result. If, instead of supposing the eyes to have disappeared wholly, we recognise them as being in fact rudimentary, the case is made worse. If, instead of 2000 generations, we assume 10,000 generations, which, considering the probably great age of the caverns, would be a far more reasonable assumption than the other, the case is made still worse. And if we assume larger variations—say decreases of one-fourth—to occur only at intervals of many hundreds or thousands of generations, which is not a very reasonable assumption, the implied conclusion would still remain indefensible. For an economy of $\frac{1}{200}$th part of the creature's weight could not appreciably affect its survival and the increase of its posterity.

Is it not then, as said above, that the use of the expression, "natural selection" has had seriously perverting effects? Must we not infer that there has been produced in the minds of naturalists, the tacit assumption that it can do what artificial selection does—can pick out and select any small advantageous trait; while it can, in fact, pick out no traits, but can only further the development of traits which, *in marked ways*, increase the general fitness for the conditions of existence? And is it not inferable that, failing to bear in mind the limiting condition, that to become established an advantageous variation must be such as will, other things remaining equal, add to the prosperity of the stirp, many naturalists have been unawares led to espouse an untenable hypothesis?

HERBERT SPENCER.

(*To be concluded.*)

THE INADEQUACY OF "NATURAL SELECTION."

(*Concluded.*)

ALONG with that inadequacy of natural selection to explain changes of structure which do not aid life in important ways, alleged in § 166 of "The Principles of Biology," a further inadequacy was alleged. It was contended that the relative powers of co-operative parts cannot be adjusted solely by survival of the fittest; and especially where the parts are numerous and the co-operation complex. In illustration it was pointed out that immensely developed horns, such as those of the extinct Irish elk, weighing over a hundred-weight, could not, with the massive skull bearing them, be carried at the extremity of the outstretched neck without many and great modifications of adjacent bones and muscles of the neck and thorax; and that without strengthening of the fore-legs, too, there would be failure alike in fighting and in locomotion. And it was argued that while we cannot assume spontaneous increase of all these parts proportionate to the additional strains, we cannot suppose them to increase by variation one at once, without supposing the creature to be disadvantaged by the weight and nutrition of parts that were for the time useless—parts, moreover, which would revert to their original sizes before the other needful variations occurred.

When, in reply to me, it was contended that co-operative parts vary together, I named facts conflicting with this assertion—the fact that the blind crabs of the Kentucky caves have lost their eyes but not the foot-stalks carrying them; the fact that the normal proportion between tongue and beak in certain selected varieties of pigeons is lost; the fact that lack of concomitance in decrease of jaws and teeth in sundry kinds of pet dogs, has caused great crowding of the teeth ("The Factors of Organic Evolution," pp. 12, 13). And I then argued that if co-operative parts, small in number and so closely associated as these are, do not vary together, it is unwarrantable to allege that co-operative parts which are very numerous and remote from one another vary together. After making this rejoinder I enforced my argument by a further example—that of the giraffe. Tacitly recognising the truth that the unusual structure of this creature must have been, in its more conspicuous traits, the result of survival of the fittest (since it is absurd to suppose that efforts to reach a high branch could lengthen the legs), I illustrated afresh the obstacles to co-adaptation. Not dwelling on the objection that increase of any components of the fore-quarters out of adjustment to the others would cause evil rather than good, I went on to argue that the co-adaptation of parts required to make the giraffe's structure useful, is much greater than at first appears. This animal has a grotesque gallop, necessitated by the great difference in length between the fore and the hind limbs. I pointed out that the mode of action of the hind limbs shows that the bones and muscles have all been changed in their proportions and adjustments; and I contended that, difficult as it is to believe that all parts of the fore-quarters have been co-adapted by the appropriate variations now of this part, now of that, it becomes impossible to believe that all the parts in the hind-quarters have been simultaneously co-adapted to one another and to all the parts of the fore-quarters: adding that want of co-adaptation, even in a single muscle, would cause fatal results when high speed had to be maintained while escaping from an enemy.

Since this argument, repeated with this fresh illustration, was published in 1886, I have met with nothing to be called a reply; and might, I think, if convictions usually followed proofs, leave the matter as it stands. It is true that, in his "Darwinism," Mr. Wallace has adverted to my renewed objection and, as already said, contended that changes such as those instanced can be effected by natural selection, since such changes can be effected by artificial selection: a contention which, as I have pointed out, assumes a parallelism that does not exist. But now, instead of pursuing the argument further along the same line, let me take a somewhat different line.

If there occurs some change in an organ, say, by increase of its size, which adapts it better to the creature's needs, it is admitted that when, as commonly happens, the use of the organ demands the co-operation of other organs, the change in it will generally be of no service unless the co-operative organs are changed. If, for instance, there takes place such a modification of a rodent's tail as that which, by successive increases, produces the trowel-shaped tail of the beaver, no advantage will be derived unless there also take place certain modifications in the bulks and shapes of the adjacent vertebræ and their attached muscles, as well, probably, as in the hind limbs, enabling them to withstand the reactions of the blows given by the tail. And the question is, by what process these many parts, changed in different degrees, are co-adapted to the new requirements—whether variation and natural selection alone can effect the readjustment. There are three conceivable ways in which the parts may simultaneously change:—(1) they may all increase or decrease together in like degrees; (2) they may all simultaneously increase or decrease independently, so as not to maintain their previous proportions or assume any other special proportions; (3) they may vary in such ways and degrees as to make them jointly serviceable for the new end. Let us consider closely these several conceivabilities.

And first of all, what are we to understand by co-operative parts? In a general sense, all the organs of the body are co-operative parts, and are respectively liable to be more or less changed by change in any one. In a narrower sense, more directly relevant to the argument, we may, if we choose to multiply difficulties, take the entire framework of bones and muscles as formed of co-operative parts; for these are so related that any considerable change in the actions of some entails change in the actions of most others. It needs only to observe how, when putting out an effort, there goes, along with a deep breath, an expansion of the chest and a bracing up of the abdomen, to see that various muscles beyond those directly concerned are strained along with them. Or, when suffering from lumbago, an effort to lift a chair will cause an acute consciousness that not the arms only are brought into action, but also the muscles of the back. These cases show how the motor organs are so tied together that altered actions of some implicate others quite remote from them.

But without using the advantage which this interpretation of the words would give, let us take as co-operative organs those which are obviously such—the organs of locomotion. What, then, shall we say of the fore and hind limbs of terrestrial mammals, which co-operate closely and perpetually? Do they vary together? If so, how have there been produced such contrasted structures as that of the kangaroo, with its large hind limbs and small fore limbs, and that of the giraffe, in which the hind limbs are small and the fore limbs large—how does it happen that, descending from the same primitive mammal, these creatures have diverged in the proportions of their limbs in opposite directions? Take, again, the articulate animals. Compare one of the lower types, with its rows of almost equal-sized limbs, and one of the higher types, as a crab or a lobster, with limbs some very small and some very large. How came this contrast to arise in the course of evolution, if there was the equality of variation supposed?

But now let us narrow the meaning of the phrase still further; giving it a more favourable interpretation. Instead of considering separate limbs as co-operative, let us consider the component parts of the same limb as co-operative, and ask what would result from varying together. It would in that case happen that, though the fore and hind limbs of a mammal might become different in their sizes, they would not become different in their structures. If so, how have there arisen the unlikeness between the hind legs of the kangaroo and those of the elephant? Or if this comparison is objected to, because the creatures belong to the widely different divisions of implacental and placental mammals, take the cases of the rabbit and the elephant, both belonging to the last division. On the hypothesis of evolution these are both derived from the same original form, but the proportions of the parts have become so widely unlike that the corresponding joints are scarcely recognised as such by the unobservant: at what seem corresponding places the legs bend in opposite ways. Equally marked, or more marked, is the parallel fact among the *Articulata*. Take that limb of the lobster which bears the claw and compare it with the corresponding limb in an inferior articulate animal, or the corresponding limb of its near ally, the crayfish, and it becomes obvious that the component segments of the limb have come to bear to one another in the one case proportions immensely different from those they bear in the other case. Undeniably, then, on contemplating the general facts of organic structure, we see that the concomitant variations in the parts of limbs have not been of a kind to produce equal amounts of change in them, but quite the opposite—have been everywhere producing inequalities. Moreover, we are reminded that this production of inequalities among co-operative parts, is an essential principle of development. Had it not been so, there could not have been that progress from homogeneity of structure to heterogeneity of structure which constitutes evolution.

We pass now to the second supposition:—that the variations in co-operative parts occur irregularly, or in such independent ways that they bear no definite relations to one another—miscellaneously, let us say. This is the supposition which best corresponds with the facts. Glances at the faces around yield conspicuous proofs. Many of the muscles of the face and some of the bones, are distinctly co-operative; and these respectively vary in such ways as to produce in each person a different combination. What we see in the face we have reason to believe holds in the limbs as in all other parts. Indeed, it needs but to compare people whose arms are of the same lengths, and observe how stumpy are the fingers of one and how slender those of another; or it needs but to note the unlikeness of gait of passers-by, implying small unlikenesses of structure; to be convinced that the relations among the variations of co-operative parts are anything but fixed. And now, confining our attention to limbs, let us consider what must happen if, by variations taking place miscellaneously, limbs have to be partially changed from fitness for one function to fitness for another function—have to be re-adapted. That the reader may fully comprehend the argument, he must here have patience while a good many anatomical details are set down.

Let us suppose a species of quadruped of which the members have for long past periods been accustomed to locomotion over a relatively even surface, as, for instance, the "prairie-dogs" of North America; and let us suppose that increase of numbers has driven part of them into a region full of obstacles to easy locomotion—covered, say, by the decaying stems of fallen trees, such as one sees in portions of primeval forest. Ability to leap must become a useful trait; and, according to the hypothesis we are considering, this ability will be produced by the selection of favourable variations. What are the variations required? A leap is effected chiefly by the bending of the hind limbs so as to make sharp angles at the joints, and then suddenly straightening them; as any one may see on watching a cat leap on to the table. The first required change, then, is increase of the large extensor muscles, by which the hind limbs are straightened. Their increases must be duly proportioned, for if those which straighten one joint become much stronger than those which straighten the other joint, the result must be collapse of the other joint when the muscles are contracted together. But let us make a large admission, and suppose these muscles to vary together; what further muscular change is next required? In a plantigrade mammal the metatarsal bones chiefly bear the reaction of the leap, though the toes may have a share. In a digitigrade mammal, however, the toes form almost exclusively the fulcrum, and if they are to bear the reaction of a higher leap, the flexor muscles which depress and bend them must be proportionately enlarged; if not, the leap will fail from want of a firm *point d'appui*. Tendons as well as muscles must be modified; and, among others, the many tendons which go to the digits and their phalanges. Stronger muscles and tendons imply greater strains on the joints; and unless these are strengthened, one or other dislocation will be caused by a more powerful spring. Not only the articulations themselves must be so modified as to bear greater stress, but also the numerous ligaments which hold the parts of each in place. Nor can the bodies of the bones remain unstrengthened; for if they have no more than the strengths needed for previous movements they will fail to bear more violent movements. Thus, saying nothing of the required changes in the pelvis as well as in the nerves and blood-vessels, there are, counting bones, muscles, tendons, ligaments, at least fifty different parts in each hind leg which have to be enlarged. Moreover, they have to be enlarged in unlike degrees. The muscles and tendons of the outer toes, for example, need not be added to so much as those of the median toes. Now, throughout their successive stages of growth, all these parts have to be kept fairly well balanced; as any one may infer on remembering sundry of the accidents he has known. Among my own friends I could name one who, when playing lawn-tennis, snapped the Achilles tendon; another who, while swinging his children, tore some of the muscular fibres in the calf of his leg; another who, in getting over a fence, tore a ligament of one knee. Such facts, joined with every one's experiences of sprains, show that during the extreme exertions to which limbs are now and then subject, there is a giving way of parts not quite up to the required level of strength. How, then, is this balance to be maintained? Suppose the extensor muscles have all varied appropriately; their variations are useless unless the other co-operative parts have also varied appropriately. Worse than this. Saying nothing of the disadvantage caused by extra weight and cost of nutrition, they will be causes of mischief—causes of derangement to the rest by contracting with undue force. And then, how long will it take for the rest to be brought into adjustment? As Mr. Darwin says concerning domestic animals:—"Any particular variation would generally be lost by crossing, reversion, &c. . . . . unless carefully preserved by man." In a state of nature, then, favourable variations of these muscles would disappear again long before one or a few of the co-operative parts could be appropriately varied, much more before all of them could.

With this insurmountable difficulty goes a difficulty still more insurmountable—if the expression may be allowed. It is not a question of increased sizes of parts only, but of altered shapes of parts, too. A glance at the skeletons of mammals shows how unlike are the forms of the corresponding bones of their limbs; and shows that they have been severally remoulded in each species to the different requirements entailed by its different habits. The change from the structures of hind limbs fitted only for walking and trotting to hind limbs fitted also for leaping, implies, therefore, that along with strengthenings of bones there must go alterations in their forms. Now the spontaneous alterations of form which may take place in any bone are countless. How long, then, will it be before there takes place that particular alteration which will make the bone fitter for its new action? And what is the probability that the many required changes of shape, as well as of size, in bones will each of them be effected before all the others are lost again? If the probabilities against success are incalculable, when we take account only of changes in the sizes of parts, what shall we say of their incalculableness when differences of form also are taken into account?

"Surely this piling up of difficulties has gone far enough"; the reader will be inclined to say. By no means. There is a difficulty immeasurably transcending those named. We have thus far omitted the second half of the leap, and the provisions to be made for it. After ascent of the animal's body comes descent; and the greater the force with which it is projected up, the greater is the force with which it comes down. Hence, if the supposed creature has undergone such changes in the hind limbs as will enable them to propel it to a greater height, without having undergone any changes in the fore limbs, the result will be that on its descent the fore limbs will give way, and it will come down on its nose. The fore limbs, then, have to be changed simultaneously with the hind. How changed? Contrast the markedly bent hind limbs of a cat with its almost straight fore limbs, or contrast the silence of the upward spring on to the table with the thud which the fore paws make as it jumps off the table. See how unlike the actions of the hind and fore limbs are, and how unlike their structures. In what way, then, is the required co-adaptation to be effected? Even were it a question of relative sizes only, there would be no answer; for facts already given show that we may not assume simultaneous increases of size to take place in the hind and fore limbs; and, indeed, a glance at the various human races, which differ considerably in the ratios of their legs to their arms, shows us this. But it is not simply a question of sizes. To bear the increased shock of descent the fore limbs must be changed throughout in their structures. Like those in the hind limbs, the changes must be of many parts in many proportions; and they must be both in sizes and in shapes. More than this. The scapular arch and its attached muscles must also be strengthened and remoulded. See, then, the total requirements. We must suppose that by natural selection of miscellaneous variations, the parts of the hind limbs shall be co-adapted to one another, in sizes, shapes and ratios; that those of the fore limbs shall undergo co-adaptations similar in their complexity, but dissimilar in their kinds; and that the two sets of co-adaptations shall be effected *pari passu*. If, as may be held, the probabilities are millions to one against the first set of changes being achieved, then it may be held that the probabilities are billions to one against the second being simultaneously achieved, in progressive adjustment to the first.

There remains only to notice the third conceivable mode of adjustment. It may be imagined that though, by the natural selection of miscellaneous variations, these adjustments cannot be effected, they may nevertheless be made to take place appropriately. How made? To suppose them so made is to suppose that the prescribed end is somewhere recognised; and that the changes are step by step simultaneously proportioned for achieving it—is to suppose a designed production of these changes. In such case, then, we have to fall back in part upon the primitive hypothesis; and if we do this in part, we may as well do it wholly—may as well avowedly return to the doctrine of special creations.

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

This very pronounced opinion will be met on the part of some by a no less pronounced demurrer, which involves a denial of possibility. It has been of late asserted, and by many believed, that inheritance of acquired characters cannot occur. Weismann, they say, has shown that there is early established in the evolution of each organism, such a distinctness between those component units which carry on the individual life and those which are devoted to maintenance of the species, that changes in the one cannot affect the other. We will look closely into his doctrine.

Basing his argument on the principle of the physiological division of labour, and assuming that the primary division of labour is that between such part of an organism as carries on individual life and such part as is reserved for the production of other lives, Weismann, starting with "the first multicellular organism," says that—"Hence the single group would come to be divided into two groups of cells, which may be called somatic and reproductive—the cells of the body as opposed to those which are concerned with reproduction" ("Essays upon Heredity, p. 27).

Though he admits that this differentiation "was not at first absolute, and indeed is not always so to-day," yet he holds that the differentiation eventually becomes absolute in the sense that the somatic cells, or those which compose the body at large, come to have only a limited power of cell-division, instead of an unlimited power which the reproductive cells have; and also in the sense that eventually there ceases to be any communication between the two, further than that implied by the supplying of nutriment to the reproductive cells by the somatic cells. The outcome of this argument is that, in the absence of communication, changes induced in the somatic cells, constituting the individual, cannot influence the natures of the reproductive cells, and cannot therefore be transmitted to posterity. Such is the theory. Now let us look at a few facts—some familiar, some unfamiliar.

His investigations led Pasteur to the positive conclusion that the silkworm diseases are inherited. The transmission from parent to offspring resulted, not through any contamination of the surface of the egg by the body of the parent while being deposited, but resulted from infection of the egg itself—intrusion of the parasitic organism. Generalised observations concerning the disease called *pébrine* enabled him to decide by inspection of the eggs which were infected and which were not: certain modifications of form distinguishing the diseased ones. More than this, the infection was proved by microscopical examination of the contents of the egg; in proof of which he quotes as follows from Dr. Carlo Vittadini:—

> "Il résulte de mes recherches sur les graines, à l'époque où commence le développement du germe, que les corpuscules, une fois apparus dans l'œuf, augmentent graduellement en nombre, à mesure que l'embryon se développe; que, dans les derniers jours de l'incubation, l'œuf en est plein, au point de faire croire que la majeure partie des granules du jaune se sont transformés en corpuscules.
>
> "Une autre observation importante est que l'embryon aussi est souillé de corpuscules, et à un degré tel qu'on peut soupçonner que l'infection du jaune tire son origine du germe lui-même; en d'autres termes que le germe est primordialement infecté, et porte en lui-même ces corpuscules tout comme les vers adultes, frappés du même mal." *

Thus, then, the substance of the egg, and even its innermost vital part, is permeable by a parasite sufficiently large to be microscopically visible. It is also of course permeable by the invisible molecules of protein, out of which its living tissues are formed, and by absorption of which they subsequently grow. But, according to Weismann, it is not permeable by those invisible units of protoplasm out of which the vitally-active tissues of the parent are constituted: units composed, as we must assume, of variously-arranged molecules of protein. So that the big thing may pass, and the little thing may pass, but the intermediate thing may not pass!

A fact of kindred nature, unhappily more familiar, may be next brought in evidence. It concerns the transmission of a disease not unfrequent among those of unregulated lives. The highest authority concerning this disease, in its inherited form, is Mr. Jonathan

* "Les Maladies des Vers à soie," par L. Pasteur, i. 39.

Hutchinson; and the following are extracts from a letter I have received from him, and which I publish with his assent.

"I do not think that there can be any reasonable doubt that a very large majority of those who suffer from inherited syphilis take the taint from the male parent. . . . . It is the rule when a man marries who has no remaining local lesion, but in whom the taint is not eradicated, for his wife to remain apparently well, whilst her child may suffer. No doubt the child infects its mother's blood, but this does not usually evoke any obvious symptoms of syphilis. . . . , I am sure I have seen hundreds of syphilitic infants whose mothers had not, so far as I could ascertain, ever displayed a single symptom."

See, then, to what we are committed if we accept Weismann's hypothesis. We must conclude that, whereas the reproductive cell may be effectually invaded by an abnormal living element in the parental organism, those normal living elements which constitute the vital protoplasm of the parental organism, cannot invade it. Or if it be admitted that both intrude, then the implication is that, whereas the abnormal element can so modify the development as to cause changes of structure (as of the teeth), the normal element can cause no changes of structure! *

We pass now to evidence not much known in the world at large, but widely known in the biological world, though known in so incomplete a manner as to be undervalued in it. Indeed, when I name it probably many will vent a mental pooh-pooh. The fact to which I refer is one of which record is preserved in the museum of the College of Surgeons, in the shape of paintings of a foal borne by a mare not quite thoroughbred, to a sire which was thoroughbred—a foal which bears the markings of the quagga. The history of this remarkable foal is given by the Earl of Morton, F.R.S., in a letter to the President of the Royal Society (read November 23, 1820). In it he states that wishing to domesticate the quagga, and having obtained a male, but not a female, he made an experiment.

"I tried to breed from the male quagga and a young chestnut mare of seven-eighths Arabian blood, and which had never been bred from; the result was the production of a female hybrid, now five years old, and bearing, both in her form and in her colour, very decided indications of her mixed origin. I subsequently parted with the seven-eighths Arabian mare to Sir Gore Ouseley, who has bred from her by a very fine black Arabian horse. I

* Curiously enough, Weismann refers to, and recognises, syphilitic infection of the reproductive cells. Dealing with Brown-Séquard's cases of inherited epilepsy (concerning which, let me say, that I do not commit myself to any derived conclusions), he says:—"In the case of epilepsy, at any rate, it is easy to imagine [many of Weismann's arguments are based on things 'it is easy to imagine'] that the passage of some specific organism through the reproductive cells may take place, as in the case of syphilis" (p. 82). Here is a sample of his reasoning. It is well known that epilepsy is frequently caused by some peripheral irritation (even by the lodging of a small foreign body under the skin), and that, among peripheral irritations causing it, imperfect healing is one. Yet though, in Brown-Séquard's cases, a peripheral irritation caused in the parent by local injury was the apparent origin, Weismann chooses gratuitously to assume that the progeny were infected by "some specific organism," which produced the epilepsy! And then, though the epileptic virus, like the syphilitic virus, makes itself at home in the egg, the parental protoplasm is not admitted!

yesterday morning examined the produce, namely, a two-year-old filly and a year-old colt. They have the character of the Arabian breed as decidedly as can be expected, where fifteen-sixteenths of the blood are Arabian; and they are fine specimens of that breed; but both in their colour and in the hair of their manes, they have a striking resemblance to the quagga. Their colour is bay, marked more or less like the quagga in a darker tint. Both are distinguished by the dark line along the ridge of the back, the dark stripes across the fore-hand, and the dark bars across the back part of the legs." *

Lord Morton then names sundry further correspondences. Dr. Wollaston, at that time President of the Royal Society, who had seen the animals, testified to the correctness of his description, and, as shown by his remarks, entertained no doubt about the alleged facts. But good reason for doubt may be assigned. There naturally arises the question—How does it happen that parallel results are not observed in other cases? If in any progeny certain traits not belonging to the sire, but belonging to a sire of preceding progeny, are re-produced, how is it that such anomalously-inherited traits are not observed in domestic animals, and indeed in mankind? How is it that the children of a widow by a second husband do not bear traceable resemblances of the first husband? To these questions nothing like satisfactory replies seem forthcoming; and, in the absence of replies, scepticism, if not disbelief, may be held reasonable.

There is an explanation, however. Forty years ago I made acquaintance with a fact which impressed me by its significant implications; and has, for this reason I suppose, remained in my memory. It is set forth in the *Journal of the Royal Agricultural Society*, vol. xiv. (1853), pp. 214 *et seq.*, and concerns certain results of crossing English and French breeds of sheep. The writer of the translated paper, M. Malingié-Nouel, Director of the Agricultural School of La Charmoise, states that when the French breeds of sheep (in which were included "the *mongrel* Merinos") were crossed with an English breed, "the lambs present the following results. Most of them resemble the mother more than the father; some show no trace of the father." Joining the admission respecting the mongrels with the facts subsequently stated, it is tolerably clear that the cases in which the lambs bore no traces of the father were cases in which the mother was of pure breed. Speaking of the results of these crossings in the second generation "having 75 per cent. of English blood," M. Nouel says:—"The lambs thrive, wear a beautiful appearance, and complete the joy of the breeder. . . . . No sooner are the lambs weaned than their strength, their vigour, and their beauty begin to decay. . . . . At last the constitution gives way . . . . he remains stunted for life:" the constitution being thus proved unstable or unadapted to the requirements. How, then, did M. Nouel succeed in obtaining a desirable

* "Philosophical Transactions of the Royal Society for the Year 1821," Part I. pp. 20–24.

combination of a fine English breed with the relatively poor French breeds?

> "He took an animal from 'flocks originally sprung from a mixture of the two distinct races that are established in these two provinces [Berry and La Sologne],' and these he 'united with animals of another mixed breed . . . . which blended the Tourangelle and native Merino blood of' La Beauce and Touraine, and obtained a mixture of all four races 'without decided character, without fixity . . . . but possessing the advantage of being used to our climate and management.'
>
> "Putting one of these 'mixed-blood ewes to a pure New-Kent ram . . . . one obtains a lamb containing fifty-hundredths of the purest and most ancient English blood, with twelve and a half hundredths of four different French races, which are individually lost in the preponderance of English blood, and disappear almost entirely, leaving the improving type in the ascendant. . . . . All the lambs produced strikingly resembled each other, and even Englishmen took them for animals of their own country.'"

M. Nouel goes on to remark that when this derived breed was bred with itself, the marks of the French breeds were lost. "Some slight traces could be detected by experts, but these soon disappeared."

Thus we get proof that relatively pure constitutions predominate in progeny over much mixed constitutions. The reason is not difficult to see. Every organism tends to become adapted to its conditions of life; and all the structures of a species, accustomed through multitudinous generations to the climate, food, and various influences of its locality, are moulded into harmonious co-operation favourable to life in that locality: the result being that in the development of each young individual, the tendencies conspire to produce the fit organisation. It is otherwise when the species is removed to a habitat of different character, or when it is of mixed breed. In the one case its organs, partially out of harmony with the requirements of its new life, become partially out of harmony with one another; since, while one influence, say of climate, is but little changed, another influence, say of food, is much changed; and, consequently, the perturbed relations of the organs interfere with their original stable equilibrium. Still more in the other case is there a disturbance of equilibrium. In a mongrel the constitution derived from each source repeats itself as far as possible. Hence a conflict of tendencies to evolve two structures more or less unlike. The tendencies do not harmoniously conspire; but produce partially incongruous sets of organs. And evidently where the breed is one in which there are united the traits of various lines of ancestry, there results an organisation so full of small incongruities of structure and action, that it has a much-diminished power of maintaining its balance; and while it cannot withstand so well adverse influences, it cannot so well hold its own in the offspring. Concerning parents of pure and mixed breeds respectively, severally tending to reproduce their own structures in progeny, we may therefore say, figuratively, that the house divided against itself cannot withstand the house of which the members are in concord.

Now if this is shown to be the case with breeds the purest of which have been adapted to their habitats and modes of life during some few hundred years only, what shall we say when the question is of a breed which has had a constant mode of life in the same locality for ten thousand years or more, like the quagga? In this the stability of constitution must be such as no domestic animal can approach. Relatively stable as may have been the constitutions of Lord Morton's horses, as compared with the constitutions of ordinary horses, yet, since Arab horses, even in their native country, have probably in the course of successive conquests and migrations of tribes become more or less mixed, and since they have been subject to the conditions of domestic life, differing much from the conditions of their original wild life, and since the English breed has undergone the perturbing effects of change from the climate and food of the East to the climate and food of the West, the organisations of the horse and mare in question could have had nothing like that perfect balance produced in the quagga by a hundred centuries of harmonious co-operation. Hence the result. And hence at the same time the interpretation of the fact that analogous phenomena are not perceived among domestic animals, or among ourselves; since both have relatively mixed, and generally extremely mixed, constitutions, which, as we see in ourselves, have been made generation after generation, not by the formation of a mean between two parents, but by the jumbling of traits of the one with traits of the other, until there exist no such conspiring tendencies among the parts as cause repetition of combined details of structure in posterity.

Expectation that scepticism might be felt respecting this alleged anomaly presented by the quagga-marked foal, had led me to think over the matter; and I had reached this interpretation before sending to the College of Surgeons Museum (being unable to go myself) to obtain the particulars and refer to the records. When there was brought to me a copy of the account as set forth in the "Philosophical Transactions," it was joined with the information that there existed an appended account of pigs, in which a parallel fact had been observed. To my immediate inquiry—"Was the male a wild pig?"—there came the reply: "I did not observe." Of course I forthwith obtained the volume, and there found what I expected. It was contained in a paper communicated by Dr. Wollaston from Daniel Giles, Esq., concerning his "sow and her produce," which said that

"she was one of a well-known black and white breed of Mr. Western, the Member for Essex. About ten years since I put her to a boar of the wild breed, and of a deep chestnut colour, which I had just received from Hatfield House, and which was soon afterwards drowned by accident. The pigs produced (which were her first litter) partook in appearance of both boar and sow, but in some the chestnut colour of the boar strongly prevailed.

"The sow was afterwards put to a boar of Mr. Western's breed (the wild boar having been long dead). The produce was a litter of pigs, some of which, we observed with much surprise, to be stained and clearly marked with the chestnut colour which had prevailed in the former litter."

Mr. Giles adds that in a second litter of pigs, the father of which was of Mr. Western's breed, he and his bailiff believe there was a recurrence, in some, of the chestnut colour, but admits that their "recollection is much less perfect than I wish it to be." He also adds that, in the course of many years' experience, he had never known the least appearance of the chestnut colour in Mr. Western's breed.

What are the probabilities that these two anomalous results should have arisen, under these exceptional conditions, as a matter of chance? Evidently the probabilities against such a coincidence are enormous. The testimony is in both cases so good that, even apart from the coincidence, it would be unreasonable to reject it; but the coincidence makes acceptance of it imperative. There is mutual verification, at the same time that there is a joint interpretation yielded of the strange phenomenon, and of its non-occurrence under ordinary circumstances.

And now, in the presence of these facts, what are we to say? Simply that they are fatal to Weismann's hypothesis. They show that there is none of the alleged independence of the reproductive cells; but that the two sets of cells are in close communion. They prove that while the reproductive cells multiply and arrange themselves during the evolution of the embryo, some of their germ-plasm passes into the mass of somatic cells constituting the parental body, and becomes a permanent component of it. Further, they necessitate the inference that this introduced germ-plasm, everywhere diffused, is some of it included in the reproductive cells subsequently formed. And if we thus get a demonstration that the somewhat different units of a foreign germ-plasm permeating the organism, permeate also the subsequently-formed reproductive cells, and affect the structures of the individuals arising from them, the implication is that the like happens with those native units which have been made somewhat different by modified functions: there must be a tendency to inheritance of acquired characters.

One more step only has to be taken. It remains to ask what is the flaw in the assumption with which Weismann's theory sets out. If, as we see, the conclusions drawn from it do not correspond to the facts, then, either the reasoning is invalid, or the original postulate is untrue. Leaving aside all questions concerning the reasoning, it will suffice here to show the untruth of the postulate. Had his work been written during the early years of the cell-doctrine, the supposition that the multiplying cells of which the Metazoa and the Metaphyta are composed, become completely separate, could not have been met by a reasonable scepticism; but now, not only is scepticism justifiable, but denial is called for. Some dozen years ago it was discovered that in many cases vegetal cells are connected with one another by threads of protoplasm—threads which unite the internal protoplasm of one cell with the internal protoplasms of cells around. It is as though the pseudopodia of imprisoned rhizopods were fused with the pseudopodia of adjacent imprisoned rhizopods. We cannot reasonably suppose that the continuous network of protoplasm thus constituted has been produced after the cells have become adult. These protoplasmic connections must have survived the process of fission. The implication is that the cells forming the embryo-plant retained their protoplasmic connections while they multiplied, and that such connections continued throughout all subsequent multiplications—an implication which has, I believe, been established by researches upon germinating palm-seeds. But now we come to a verifying series of facts which the cell-structures of animals in their early stages present. In his "Monograph of the Development of Peripatus Capensis," Mr. Adam Sedgwick, F.R.S., Reader in Animal Morphology at Cambridge, writes as follows:—

"All the cells of the ovum, ectodermal as well as endodermal, are connected together by a fine protoplasmic reticulum" (p. 41).

"The continuity of the various cells of the segmenting ovum is primary, and not secondary; *i.e.*, in the cleavage the segments do not completely separate from one another. But are we justified in speaking of cells at all in this case? *The fully segmented ovum is a syncytium, and there are not and have not been at any stage cell limits*" (p. 41).

"It is becoming more and more clear every day that the cells composing the tissues of animals are not isolated units, but that they are connected with one another. I need only refer to the connection known to exist between connective-tissue cells, cartilage cells, epithelial cells, &c. And not only may the cells of one tissue be continuous with each other, but they may also be continuous with the cells of other tissues" (pp. 47–8).

"Finally, if the protoplasm of the body is primitively a syncytium, and the ovum until maturity a part of that syncytium, the separation of the generative products does not differ essentially from the internal gemmation of a Protozoon, and the inheritance by the offspring of peculiarities first appearing in the parent, though not explained, is rendered less mysterious; for the protoplasm of the whole body being continuous, change in the molecular constitution of any part of it would naturally be expected to spread, in time, through the whole mass" (p. 49).

Mr. Sedgwick's subsequent investigations confirm these conclusions. In a letter of December 27, 1892, passages, which he allows me to publish, run as follows:—

"All the embryological studies that I have made since that to which you refer confirm me more and more in the view that the connections between the cells of adults are not secondary connections, but primary, dating from the time when the embryo was a unicellular structure. . . . . My own investigations on this subject have been confined to the Arthropoda, Elasmobranchii, and Aves. I have thoroughly examined the development of at least one kind of each of these groups, and I have never been able to detect a stage in which the cells were not continuous with each other; and I have studied innumerable stages from the beginning of cleavage onwards."

So that the alleged independence of the reproductive cells does not exist. The *soma*—to use Weismann's name for the aggregate of cells forming the body—is, in the words of Mr. Sedgwick, "a continuous mass of vacuolated protoplasm;" and the reproductive cells are nothing more than portions of it separated some little time before they are required to perform their functions.

Thus the theory of Weismann is doubly disproved. Inductively we are shown that there *does* take place that communication of characters from the somatic cells to the reproductive cells, which he says cannot take place; and deductively we are shown that this communication is a natural sequence of connections between the two which he ignores: his various conclusions are deduced from a postulate which is untrue.

From the title of this essay, and from much of its contents, nine readers out of ten will infer that it is directed against the views of Mr. Darwin. They will be astonished on being told that, contrariwise, it is directed against the views of those who, in a considerable measure, dissent from Mr. Darwin. For the inheritance of acquired characters, which it is now the fashion in the biological world to deny, was, by Mr. Darwin, fully recognised and often insisted on. Such of the foregoing arguments as touch Mr. Darwin's views, simply imply that the cause of evolution which at first he thought unimportant, but the importance of which he increasingly perceived as he grew older, is more important than he admitted even at the last. The neo-Darwinists, however, do not admit this cause at all.

Let it not be supposed that this explanation implies any disapproval of the dissentients, considered as such. Seeing how little regard for authority I have myself usually shown, it would be absurd in me to reflect in any degree upon those who have rejected certain of Mr. Darwin's teachings, for reasons which they have thought sufficient. But while their independence of thought is to be applauded rather than blamed, it is, I think, to be regretted that they have not guarded themselves against a long-standing bias. It is a common trait of human nature to seek some excuse when found in the wrong. Invaded self-esteem sets up a defence, and anything is made to serve. Thus it happened that when geologists and biologists, previously holding that all kinds of organisms arose by special creations, surrendered to the battery opened upon them by "The Origin of Species," they sought to minimise their irrationality by pointing to irrationality on the other side. "Well, at any rate, Lamarck was in the wrong." "It is clear that we were right in rejecting his doctrine." And so, by duly emphasising the fact that he overlooked "Natural Selection" as the chief cause, and by showing how erroneous were some of his interpretations, they succeeded in mitigating the sense of their own error. It is true their creed was that at successive periods in the Earth's history, old Floras and Faunas had been abolished and others introduced; just as though, to use Professor Huxley's figure, the table had been now and again kicked over and a new pack of cards brought out. And it is true that Lamarck, while he rejected this absurd creed, assigned for the facts reasons some of which are absurd. But in consequence of the feeling described, his defensible belief was forgotten and only his indefensible ones remembered. This one-sided estimate has become traditional; so that there is now often shown a subdued contempt for those who suppose that there can be any truth in the conclusions of a man whose general conception was partly sense, at a time when the general conceptions of his contemporaries were wholly nonsense. Hence results unfair treatment—hence result the different dealings with the views of Lamarck and of Weismann.

"Where are the facts proving the inheritance of acquired characters"? ask those who deny it. Well, in the first place, there might be asked the counter-question—Where are the facts which disprove it? Surely if not only the general structures of organisms, but also many of the modifications arising in them, are inheritable, the natural implication is that all modifications are inheritable; and if any say that the inheritableness is limited to those arising in a certain way, the *onus* lies on them of proving that those otherwise arising are not inheritable. Leaving this counter-question aside, however, it will suffice if we ask another counter-question. It is asserted that the dwindling of organs from disuse is due to the successive survivals in posterity of individuals in which the organs had varied in the direction of decrease. Where now are the facts supporting this assertion? Not one has been assigned or can be assigned. Not a single case can be named in which *panmixia* is a proved cause of diminution. Even had the deductive argument for *panmixia* been as valid as we have found it to be invalid, there would still have been required, in pursuance of scientific method, some verifying inductive evidence. Yet though not a shred of such evidence has been given, the doctrine is accepted with acclamation, and adopted as part of current biological theory. Articles are written and letters published in which it is assumed that this mere speculation, justified by not a tittle of proof, displaces large conclusions previously drawn. And then, passing into the outer world, this unsupported belief affects opinion there too; so that we have recently had a Right Honourable lecturer who, taking for granted its truth, represents the inheritance of acquired characters as an exploded hypothesis, and thereupon proceeds to give revised views of human affairs.

Finally, there comes the reply that there *are* facts proving the inheritance of acquired characters. All those assigned by Mr. Darwin, together with others such, remain outstanding when we find that the interpretation by *panmixia* is untenable. Indeed, even had that hypothesis been tenable, it would have been inapplicable to these cases; since in domestic animals, artificially fed and often overfed, the supposed advantage from economy cannot be shown to tell; and since, in these cases, individuals are not naturally selected during the struggle for life in which certain traits are advantageous, but are artificially selected by man without regard to such traits. Should it be urged that the assigned facts are not numerous, it may be replied that there are no persons whose occupations and amusements incidentally bring out such facts; and that they are probably as numerous as those which would have been available for Mr. Darwin's hypothesis, had there been no breeders and fanciers and gardeners who, in pursuit of their profits and hobbies, furnished him with evidence. It may be added that the required facts are not likely to be numerous, if biologists refuse to seek for them.

See, then, how the case stands. Natural selection, or survival of the fittest, is almost exclusively operative throughout the vegetal world and throughout the lower animal world, characterised by relative passivity. But with the ascent to higher types of animals, its effects are in increasing degrees involved with those produced by inheritance of acquired characters; until, in animals of complex structures, inheritance of acquired characters becomes an important, if not the chief, cause of evolution. We have seen that natural selection cannot work any changes in organisms save such as conduce in considerable degrees, directly or indirectly, to the multiplication of the stirp; whence failure to account for various changes ascribed to it. And we have seen that it yields no explanation of the co-adaptation of co-operative parts, even when the co-operation is relatively simple, and still less when it is complex. On the other hand, we see that if, along with the transmission of generic and specific structures, there tend to be transmitted modifications arising in a certain way, there is a strong *a priori* probability that there tend to be transmitted modifications arising in all ways. We have a number of facts confirming this inference, and showing that acquired characters *are* inherited—as large a number as can be expected, considering the difficulty of observing them and the absence of search. And then to these facts may be added the facts with which this essay set out, concerning the distribution of tactual discriminativeness. While we saw that these are inexplicable by survival of the fittest, we saw that they are clearly explicable as resulting from the inheritance of acquired characters. And here let it be added that this conclusion is conspicuously warranted by one of the methods of inductive logic, known as the method of concomitant variations. For throughout the whole series of gradations in perceptive power, we saw that the amount of the effect is proportionate to the amount of the alleged cause.

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