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A

REJOINDER TO PROFESSOR WEISMANN.

by H. Spencer

As a species of literature, controversy is characterised by a terrible fertility. Each proposition becomes the parent of half a dozen; so that a few replies and rejoinders produce an unmanageable population of issues, old and new, which end in being a nuisance to everybody. Remembering this, I shall refrain from dealing with all the points of Professor Weismann's answer. I must limit myself to a part; and that there may be no suspicion of a selection convenient to myself, I will take those contained in his first article.

Before dealing with his special arguments, let me say something about the general mode of argument which Professor Weismann adopts.

The title of his article is "The All-Sufficiency of Natural Selection."* Very soon, however, as on p. 322, we come to the admission, which he has himself italicised, "that *it is really very difficult to imagine this process of natural selection in its details;* and to this day it is impossible to demonstrate it in any one point." Elsewhere, as on pp. 327 and 336 *à propos* of other cases, there are like admissions. But now if the sufficiency of an assigned cause cannot in any case be

* *Contemporary Review*, September 1893.

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demonstrated, and if it is "really very difficult to imagine" in what way it has produced its alleged effects, what becomes of the "all-sufficiency" of the cause? How can its all-sufficiency be alleged when its action can neither be demonstrated nor easily imagined? Evidently to fit Professor Weismann's argument the title of the article should have been "The Doubtful Sufficiency of Natural Selection."

Observe, again, how entirely opposite are the ways in which he treats his own interpretation and the antagonist interpretation. He takes the problem presented by certain beautifully adapted structures on the anterior legs of "very many insects," which they use for cleansing their antennæ. These, he argues, cannot have resulted from the inheritance of acquired characters; since any supposed changes produced by function would be changes in the chitinous exo-skeleton, which, being a dead substance, cannot have had its changes transmitted. He then proceeds, very candidly, to point out the extreme difficulties which lie in the way of supposing these structures to have resulted from natural selection: admitting that an opponent might "say that it was absurd" to assume that the successive small variations implied were severally life-saving in their effects. Nevertheless, he holds it unquestionable that natural selection has been the cause. See then the difference. The supposition that the apparatus has been produced by the inheritance of acquired characters is rejected *because* it presents insuperable difficulties. But the supposition that the apparatus has been produced by natural selection is accepted, *though* it presents insuperable difficulties. If this mode of reasoning is allowable, no fair comparison between diverse hypotheses can be made.

With these remarks on Professor Weismann's method at large, let me now pass to the particular arguments he uses, taking them *seriatim.*

The first case he deals with is that of the progressive degradation of the human little toe. This he considers a good test case; and he proceeds to discuss an assigned cause—the inherited and accumulated effects of boot-pressure. Without much difficulty he shows that this interpretation is inadequate; since fusion of the phalanges, which constitutes in part the progressive degradation, is found among peoples who go barefoot, and has been found also in Egyptian mummies. Having thus disposed of Mr. Buckman's interpretation, Professor Weismann forthwith concludes that the ascription of this anatomical change to the inheritance of acquired characters is disposed of, and assumes, as the only other possible interpretation, a dwindling "through panmixia": "the hereditary degeneration of the little toe is thus quite simply explained from my standpoint."

It is surprising that Professor Weismann should not have seen that there is an explanation against which his criticism does not tell. If we go back to the genesis of the human type from some lower type of *primates*, we see that while the little toe has ceased to be of any use for climbing purposes, it has not come into any considerable use for walking and running. A glance at the feet of the sub-human *primates* in general, shows that the inner digits are, as compared with those of men, quite small—have no such relative length and massiveness as the human great toes. Leaving out the question of cause, it is manifest that the great toes have been immensely developed, since there took place the change from arboreal habits to terrestrial habits. A study of the mechanics of walking shows why this has happened Stability requires that the "line of direction" (the vertical line let fall from the centre of gravity) shall fall within the base, and, in walking, shall be brought at each step within the area of support, or so near it that any tendency to fall may be checked at the next step. A necessary result is that if, at each step, the chief stress of support is thrown on the outer side of the foot, the body must be swayed so that the "line of direction" may fall within the outer side of the foot, or close to it; and when the next step is taken it must be similarly swayed in an opposite way, so that the outer side of the other foot may bear the weight. That is to say, the body must oscillate from side to side, or waddle. The movements of a duck when walking or running show what happens when the points of support are wide apart. Clearly this kind of movement conflicts with efficient locomotion. There is a waste of muscular energy in making these lateral movements, and they are at variance with the forward movement. We may infer, then, that the developing man profited by throwing the stress as much as possible on the inner sides of the feet; and was especially led to do this when going fast, which enabled him to abridge the oscillations: as indeed we now see in a drunken man. Thus there was thrown a continually increasing stress upon the inner digits as they progressively developed from the effects of use; until now that the inner digits, so large compared with the others, bear the greater part of the weight, and being relatively near one another, render needless any marked swayings from side to side. But what has meanwhile happened to the outer digits? Evidently as fast as the great toes have come more and more into play and developed, the little toes have gone more and more out of play and have been dwindling for—how long shall we say?—perhaps a hundred thousand years.

So far then am I from feeling that Professor Weismann has here raised a difficulty in the way of the doctrine I hold, that I feel indebted to him for having drawn attention to a very strong evidence in its support. This modification in the form of the foot, which has occurred since arboreal habits have given place to terrestrial habits, shows the effects of use and disuse simultaneously. The inner digits have increased by use while the outer digits have decreased by disuse.

Saying that he will not "pause to refute other apparent proofs of the transmission of acquired characters," Professor

Weismann proceeds to deal with the argument which, with various illustrations, I have several times urged—the argument that the natural selection of fortuitously-arising variations cannot account for the adjustment of co-operative parts. Very clearly and very fairly he summarises this argument as used in *The Principles of Biology* in 1864. Admitting that in this case there are "enormous difficulties" in the way of any other interpretation than the inheritance of acquired characters, Professor Weismann before proceeding to assault this "last bulwark of the Lamarckian principle," premises that the inheritance of acquired characters cannot be a cause of change because inactive as well as active parts degenerate when they cease to be of use: instancing the "skin and skin-armature of crabs and insects." On this I may remark in the first place that an argument derived from degeneracy of passive structures scarcely meets the case of development of active structures; and I may remark in the second place that I have never dreamt of denying the efficiency of natural selection as a cause of degeneracy in passive structures when the degeneracy is such as aids the prosperity of the stirp.

Making this parenthetical reply to his parenthetical criticism I pass to his discussion of this particular argument which he undertakes to dispose of.

His *cheval de bataille* is furnished him by the social insects —not a fresh one, however, as might be supposed from the way in which he mounts it. From time to time it has carried other riders, who have couched their lances with fatal effects as they supposed. But I hope to show that no one of them has unhorsed an antagonist, and that Professor Weismann fails to do this just as completely as his predecessors. I am, indeed, not sorry that he has afforded me the opportunity of criticising the general discussion concerning the peculiarities of these interesting creatures, which it has often seemed to me sets out with illegitimate assumptions. The supposition always is that the specialities of structures and instincts in the unlike classes of their communities, have arisen during the period in which the communities have existed in something like their present forms. This cannot be. It is doubtless true that association without differentiations of classes may pre-exist for co-operative purposes, as among wolves, and as among various insects which swarm under certain circumstances. Hence we may suppose that there arise in some cases permanent swarms—that survival of the fittest will establish these constant swarms where they are advantageous. But admitting this, we have also to admit a gradual rise of the associated state out of the solitary state. Wasps and bees present us with gradations. If then we are to understand how the organized societies have arisen, either out of the solitary state or out of undifferentiated swarms, we must assume that the differences of structure and instinct among the members of them arose little by little, as the social organization arose little by little. Fortunately we are able to trace the greater part of the process in the annually-formed communities of the common wasp; and we shall recognize in it an all-important factor (ignored by Professor Weismann) to which the phenomena, or at any rate the greater part of them, are due.

But before describing the wasp's annual history, let me set down certain observations made when, as a boy, I was given to angling, and, in July or August, sometimes used for bait "wasp-grubs," as they were called. After having had for two or three days the combs or "cakes" of these, full of unfed larvæ in all stages of growth, I often saw some of them devouring the edges of their cells to satisfy their appetites; and saw others, probably the most advanced in growth, which were spinning the little covering caps to their cells, in preparation for assuming the pupa state. It is to be inferred that if, after a certain stage of growth has been reached, the food-supply becomes inadequate or is stopped altogether, the larva undergoes its transformation prematurely; and, as we shall presently see, this premature transformation has several natural sequences.

Let us return now to the wasp's family history. In the spring, a queen-wasp or mother-wasp which has survived the winter, begins to make a small nest containing four or more cells in which she lays eggs, and as fast as she builds additional cells, she lays an egg in each. Presently, to these activities, is added the feeding of the larvæ: one result being that the multiplication of larvæ involves a restriction of the food that can be given to each. If we suppose that the mother-wasp rears no more larvæ than she can fully feed, there will result queens or mothers like herself, relatively few in number. But if we suppose that, laying more numerous eggs she produces more larvæ than she can fully feed, the result will be that when these have reached a certain stage of growth, inadequate supply of food will be followed by premature retirement and transformation into pupæ. What will be the characters of the developed insects? The first effect of arrested nutrition will be smaller size. This we find. A second effect will be defective development of parts that are latest formed and least important for the survival of the individual. Hence we may look for arrested development of the reproductive organs—non-essential to individual life. And this expectation is in accord with what we see in animal development at large; for (passing over entirely sexless individuals) we see that though the reproductive organs may be marked out early in the course of development, they are not made fit for action until after the structures for carrying on individual life are nearly complete. The implication is, then, that an inadequately-fed and small larva will become a sterile imago. Having noted this, let us pass to a remarkable concomitant. In the course of development, organs are formed not alone in the order of their original succession, but partly in the order of importance and the share they have to take in adult activities—a change of order called by Haeckel "heterochrony." Hence the fact that we often see the maternal instinct precede the sexual instinct. Every little girl with her doll shows us that the one may become alive while the other remains dormant. In the case of wasps, then, premature arrest of development may result in incompleteness of the sexual traits, along with completeness of the maternal traits. What happens? Leave out the laying of eggs, and the energies of the mother-wasp are spent wholly in building cells and feeding larvæ, and the worker-wasp forthwith begins to spend its life in building cells and feeding larvæ. Thus interpreting the facts, we have no occasion to assume any constitutional difference between the eggs of worker-wasps and the eggs of queens; and that their eggs are not different we see, first, in the fact that occasionally the worker-wasp is fertile and lays drone-producing eggs, and we see secondly that (if in this respect they are like the bees, of which, however, we have no proof) the larva of a worker-wasp can be changed into the larva of a queen-wasp by special feeding. But be this as it may, we have good evidence that the feeding determines everything. Says Dr. Ormerod, in his *British Social Wasps*:—

"When the swarm is strong and food plentiful . . . the well fed larvæ develop into females, full, large, and overflowing with fat. There are all gradations of size, from the large fat female to the smallest worker. . . . . The larger the wasp, the larger and better developed, as the rule, are the female organs, in all their details. In the largest wasps, which are to be the queens of another year, the ovaries differ to all appearances in nothing but their size from those of the larger worker wasps. . . . . Small feeble swarms produce few or no perfect females; but in large strong swarms they are found by the score" (pp. 248–9).

To this evidence add the further evidence that queens and workers pass through certain parallel stages in respect of their maternal activities. At first the queen, besides laying eggs, builds cells and feeds larvæ, but after a time ceases to build cells, and feeds larvæ only, and eventually doing neither one nor the other, only lays eggs, and is supplied with food by the workers. So it is in part with the workers. While the members of each successive brood, when in full vigour, build cells and feed larvæ, by-and-by they cease to build cells, and only feed larvæ : the maternal activities and instincts undergo analogous changes. In this case, then, we are not obliged to assume that only by a process of natural selection can the differences of structure and instinct between queens and workers be produced. The only way in which natural selection here comes into play is in the better survival of the families of those queens which made as many cells, and laid as many eggs, as resulted in the best number of half-fed larvæ, producing workers ; since by a rapid multiplication of workers the family is advantaged, and the ultimate production of more queens surviving into the next year insured.

The differentiation of classes does not go far among the wasps, because the cycle of processes is limited to a year, or rather to the few months of the summer. It goes further among the hive-bees, which, by storing food, survive from one year into the next. Unlike the queen-wasp, the queen-bee neither builds cells nor gathers food, but is fed by the workers : egg-laying has become her sole business. On the other hand the workers, occupied exclusively in building and nursing, have the reproductive organs more dwarfed than they are in wasps. Still we see that the worker-bee occasionally lays drone-producing eggs, and that, by giving extra nutriment and the required extra space, a worker-larva can be developed into a queen-larva. In respect to the leading traits, therefore, the same interpretation holds. Doubtless there are subsidiary instincts which are apparently not thus interpretable. But before it can be assumed that an interpretation of another kind is necessary, it must be shown that these instincts cannot be traced back to those pre-social types and semi-social types which must have preceded the social types we now see. For unquestionably existing bees must have brought with them from the pre-social state an extensive endowment of instincts, and, acquiring other instincts during the unorganized social state, must have brought these into the present organized social state. It is clear, for instance, that the cell-building instinct in all its elaboration was mainly developed in the pre-social stage; for the transition from species building solitary cells to those building combs is traceable. We are similarly enabled to account for swarming as being an inheritance from remote ancestral types. For just in the same way that, with under-feeding of larvæ, there result individuals with imperfectly developed reproductive systems, so there will result individuals with imperfect sexual instincts; and just as the imperfect reproductive system partially operates upon occasion, so will the imperfect sexual instinct. Whence it will result that on the event which causes a queen to undertake a nuptial flight, which is effectual, the workers may take abortive nuptial flights: so causing a swarm.

And here, before going further, let us note an instructive class of facts related to the class of facts above set forth. Summing up, in a chapter on "The Determination of Sex," an induction from many cases, Professor Geddes and Mr. Thompson remark that "such conditions as deficient or abnormal food," and others causing "preponderance of waste over repair . . . . tend to result in production of males;" while "abundant and rich nutrition" and other conditions which "favour constructive processes . . . . result in the production of females."* Among such evidences of this, as immediately concern us, are these:—J. H. Fabre found that in the nests of *Osmia tricornis*, eggs at the bottom, first laid, and accompanied by much food, produced females, while those at the top, last laid, and accompanied by one-half or one-third the quantity of food, produced males.† Huber's observations on egg-laying by the honey-bee, show that in the normal course of things, the queen lays eggs of workers for eleven months, and only then lays eggs of drones: that is, when declining nutrition or exhaustion has set in. Further,

* *Evolution of Sex*, p. 50.

† *Souvenirs Entomologiques*, 3[me] Série, p. 328.

we have the above-named fact, shown by wasps and bees, that when workers lay eggs these produce drones only.* Special evidence, harmonizing with general evidence, thus proves that among these social insects the sex is determined by degree of nutrition while the egg is being formed. See then how congruous this evidence is with the conclusion above drawn; for it is proved that after an egg, predetermined as a female, has been laid, the character of the produced insect as a perfect female or imperfect female is determined by the nutrition of the larva. *That is, one set of differences in structures and instincts is determined by nutrition before the egg is laid, and a further set of differences in structures and instincts is determined by nutrition after the egg is laid.*

We come now to the extreme case—that of the ants. Is it not probable that the process of differentiation has been similar? There are sundry reasons for thinking so. With ants as with wasps and bees—the workers occasionally lay eggs; and an ant-community can, like a bee-community, when need be, produce queens out of worker-larvæ: presumably in the same manner by extra feeding. But here we have to add special evidence of great significance. For observe that the very facts concerning ants, which Professor Weismann names as exemplifying the formation of the worker type by selection, serve, as in the case of wasps, to exemplify its formation by arrested nutrition. He says that in several species the egg-tubes in the ovaries show progressive decrease in number; and this, like the different degrees of arrest in the ovaries of the worker-wasps, indicates arrest of larva-feeding at different stages. He gives cases showing that, in different degrees, the eyes of workers are less developed in the number of their facets than those of the perfect insects; and he also refers to the wings of workers as not being developed: remarking, however, that the rudiments of their wings show that the ancestral forms had wings. Are not these traits also results of arrested nutrition? Generally

* *Natural History of Bees*, new ed. p. 33.

among insects the larvæ are either blind or have but rudimentary eyes; that is to say, visual organs are among the latest organs to arise in the genesis of the perfect organism. Hence early arrest of nutrition will stop formation of these, while various more ancient structures have become tolerably complete. Similarly with wings. Wings are late organs in insect phylogeny, and therefore will be among those most likely to abort where development is prematurely arrested. And both these traits will, for the same reason, naturally go along with arrested development of the reproductive system. Even more significant, however, is some evidence assigned by Mr. Darwin respecting the caste-gradations among the driver-ants of West Africa. He says:—

"But the most important fact for us is, that, though the workers can be grouped into castes of different sizes, yet they graduate insensibly into each other, as does the widely-different structure of their jaws."*

"Graduate insensibly," he says; implying that there are very numerous intermediate forms. This is exactly what is to be expected if arrest of nutrition be the cause; for unless the ants have definite measures, enabling them to stop feeding at just the same stages, it must happen that the stoppage of feeding will be indefinite; and that, therefore, there will be all gradations between the extreme forms—"insensible gradations," both in size and in jaw-structure.

In contrast with this interpretation, consider now that of Professor Weismann. From whichever of the two possible suppositions he sets out, the result is equally fatal. If he is consistent, he must say that each of these intermediate forms of workers must have its special set of "determinants," causing its special set of modifications of organs; for he cannot assume that while perfect females and the extreme types of workers have their different sets of determinants, the intermediate types of workers have not. Hence we are introduced to the strange conclusion that

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* *Origin of Species*, 6th ed. p. 232.

besides the markedly-distinguished sets of determinants there must be, to produce these intermediate forms, many other sets slightly distinguished from one another—a score or more kinds of germ-plasm in addition to the four chief kinds. Next comes an introduction to the still stranger conclusion, that these numerous kinds of germ-plasm, producing these numerous intermediate forms, are not simply needless but injurious—produce forms not well fitted for either of the functions discharged by the extreme forms: the implication being that natural selection has originated these disadvantageous forms! If to escape from this necessity for suicide, Professor Weismann accepts the inference that the differences among these numerous intermediate forms are caused by arrested feeding of the larvæ at different stages, then he is bound to admit that the differences between the extreme forms, and between these and perfect females, are similarly caused. But if he does this, what becomes of his hypothesis that the several castes are constitutionally distinct, and result from the operation of natural selection? Observe, too, that his theory does not even allow him to make this choice; for we have clear proof that unlikenesses among the forms of the same species cannot be determined this way or that way by differences of nutrition. English greyhounds and Scotch greyhounds do not differ from one another so much as do the Amazon-workers from the inferior workers, or the workers from the queens. But no matter how a pregnant Scotch greyhound is fed, or her pups after they are born, they cannot be changed into English greyhounds: the different germ-plasms assert themselves spite of all treatment. But in these social insects the different structures of queens and workers *are* determinable by differences of feeding. Therefore the production of their various castes does not result from the natural selection of varying germ-plasm.

Before dealing with Professor Weismann's crucial case—that co-adaptation of parts, which, in the soldier-ants, has, he thinks, arisen without inheritance of acquired characters—let me deal with an ancillary case which he puts forward as explicable by "panmixia alone." This is the "degeneration, in the warlike Amazon-ants, of the instinct to search for food."* Let us first ask what have been the probable antecedents of these Amazon-ants; for, as I have above said, it is absurd to speculate about the structures and instincts the species possesses in its existing organized social state without asking what structures and instincts it brought with it from its original solitary state and its unorganised social state. From the outset these ants were predatory. Some variety of them led to swarm—probably at the sexual season—did not again disperse so soon as other varieties. Those which thus kept together derived advantages from making simultaneous attacks on prey, and prospered accordingly. Of descendants the varieties which carried on longest the associated state prospered most; until, at length, the associated state became permanent. All which social progress took place while there existed only perfect males and females. What was the next step? Ants utilize other insects, and, among other ways of doing this, sometimes make their nests where there are useful insects ready to be utilized. Giving an account of certain New Zealand species of *Tetramorium*, Mr. W. W. Smith says they seek out underground places where there are "root-feeding aphides and coccids," which they begin to treat as domestic animals; and further he says that when, after the pairing season, new nests are being formed, there are "a few ants of both sexes . . . from two up to eight or ten."† Carrying with us this fact as a key, let us ask what habits will be fallen into by the conquering species of ants. They, too, will seek places where there are creatures to be utilized; and, finding it profitable, will invade the habitations not of defenceless creatures only, but of creatures whose powers of defence are inadequate—weaker species of their own order. A very small modification will affiliate their

* *Contemporary Review*, September 1893, p. 333.

† *The Entomologist's Monthly Magazine*, March 1892, p. 61.

habits on habits of their prototypes. Instead of being supplied with sweet substance excreted by the aphides they are supplied with sweet substance by the ants among which they parasitically settle themselves. How easily the subjugated ants may fall into the habit of feeding them, we shall see on remembering that already they feed not only larvæ but adults—individuals bigger than themselves. And that attentions kindred to these paid to parasitic ants may be established without difficulty, is shown us by the small birds which continue to feed a young cuckoo in their nest when it has outgrown them. This advanced form of parasitism grew up while there were yet only perfect males and females, as happens in the initial stage with these New Zealand ants. What further modifications of habits were probably then acquired? From the practice of settling themselves where there already exist colonies of aphides, which they carry about to suitable places in the nest, like *Tetramorium*, other ants pass to the practice of making excursions to get aphides, and putting them in better feeding places where they become more productive of saccharine matter. By a parallel step these soldier-ants pass from the stage of settling themselves among other ants which feed them, to the stage of fetching the pupæ of such ants to the nest: a transition like that which occurs among slave-making human beings. Thus by processes analogous to those we see going on, these communities of slave-making ants may be formed. And since the transition from an unorganized social state to a social state characterized by castes, must have been gradual, there must have been a long interval during which the perfect males and females of these conquering ants could acquire habits and transmit them to progeny. A small modification accounts for that seemingly-strange habit which Professor Weismann signalizes. For if, as is observed, those ants which keep aphides solicit them to excrete a supply of ant-food by stroking them with the antennæ, they come very near to doing that which Professor Weismann says the soldier-ants do towards a worker—"they come to it and beg for food:" the food being put into their mouths in this last case as almost or quite in the first. And evidently this habit of passively receiving food, continued through many generations of perfect males and females, may result in such disuse of the power of self-feeding that this is eventually lost. The behaviour of young birds, during, and after, their nest-life, gives us the clue. For a week or more after they are full-grown and fly about with their parents, they may be seen begging for food and making no efforts to recognize and pick up food for themselves. If, generation after generation, feeding of them in full measure continued, they would not learn to feed themselves: the perceptions and instincts implied in self-feeding would be later and later developed, until, with entire disuse of them, they would disappear altogether by inheritance. Thus self-feeding may readily have ceased among these soldier-ants before the caste-organisation arose among them.

With this interpretation compare the interpretation of Professor Weismann. I have before protested against arguing in abstracts without descending to concretes. Here let us ask what are the particular changes which the alleged explanation by survival of the fittest involves. Suppose we make the very liberal supposition that an ant's central ganglion bears to its body the same ratio as the human brain bears to the human body—say, one-fortieth of its weight. Assuming this, what shall we assume to be the weight of those ganglion-cells and fibres in which are localized the perceptions of food and the suggestion to take it? Shall we say that these amount to one-tenth of the central ganglion? This is a high estimate considering all the impressions which this ganglion has to receive and all the operations which it has to direct. Still we will say one-tenth. Then it follows that this portion of nervous substance is one–400th of the weight of its body. By what series of variations shall we say that it is reduced from full power to entire incapacity? Shall we say five? This is a small number to assume. Nevertheless we will assume it. What results? That the economy of nerve-substance achieved by each of these five variations will amount to one-2000th of the entire mass. Making these highly favourable assumptions, what follows? The queen-ant lays eggs that give origin to individuals in each of which there is achieved an economy in nerve-substance of one-2000th of its weight; and the implication of the hypothesis is that such an economy will so advantage this ant-community that in the competition with other ant-communities it will conquer. For here let me recall the truth before insisted upon, that natural selection can operate only on those variations which appreciably benefit the stirp. Bearing in mind this requirement, is any one now prepared to say that survival of the fittest can cause this decline of the self-feeding faculty? *

Not limiting himself to the Darwinian interpretation, however, Professor Weismann says that this degradation may be accounted for by "panmixia alone." Here I will not discuss the adequacy of this supposed cause, but will leave it to be dealt with by implication a few pages in advance, where the general hypothesis of panmixia will be reconsidered.

And now, at length, we are prepared for dealing with Professor Weismann's crucial case—with his alleged disproof that co-adaptation of co-operative parts results from inheritance of acquired characters, because, in the case of the Amazon-ants, it has arisen where the inheritance of acquired characters is impossible. For after what has been said, it will be manifest that the whole question is begged when it is assumed that this co-adaptation has arisen since there existed

* Perhaps it will be alleged that nerve-matter is costly, and that this minute economy might be of importance. Anyone who thinks this will no longer think it after contemplating a litter of half-a-dozen young rabbits (in the wild rabbit the number varies from four to eight); and on remembering that the nerve-matter contained in their brains and spinal cords, as well as the materials for building up the bones, muscles, and viscera of their bodies, has been supplied by the doe in the space of a month; at the same time that she has sustained herself and carried on her activities: all this being done on relatively poor food. Nerve-matter cannot be so very costly then.

among these ants an organized social state. Unquestionably this organized social state pre-supposes a series of modifications through which it has been reached. It follows, then, that there can be no rational interpretation without a preceding inquiry concerning that earlier state in which there were no castes, but only males and females. What kinds of individuals were the ancestral ants—at first solitary and then semi-social? They must have had marked powers of offence and defence. Of predacious creatures, it is the more powerful which form societies, not the weaker. Instance human races. Nations originate from the relatively warlike tribes, not from the relatively peaceful tribes. Among the several types of individuals forming the existing ant community, to which, then, did the ancestral ants bear the greatest resemblance? They could not have been like the queens, for these, now devoted to egg-laying, are unfitted for conquest. They could not have been like the inferior class of workers, for these, too, are inadequately armed and lack strength. Hence they must have been most like these Amazon ants or soldier-ants, which now make predatory excursions—which now do, in fact, what their remote ancestors did. What follows? Their co-adapted parts have not been produced by the selection of variations within the ant-community, such as we now see it. They have been inherited from the pre-social and early social types of ants, in which the co-adaptation of parts had been effected by inheritance of acquired characters. It is not that the soldier-ants have gained these traits; it is that the other castes have lost them. Early arrest of development causes absence of them in the inferior workers; and from the queens they have slowly disappeared by inheritance of the effects of disuse. For, in conformity with ordinary facts of development, we may conclude that in a larva which is being so fed as that the development of the reproductive organs is becoming pronounced, there will simultaneously commence arrest in the development of those organs which are not to be used. There are abundant proofs that along with rapid growth of some organs others abort. And if these inferences are true, then Professor Weismann's argument falls to the ground. Nay, it falls to the ground even if conclusions so definite as these be not insisted upon; for before he can get a basis for his argument he must give good reasons for concluding that these traits of the Amazon-ants have *not* been inherited from remote ancestors.

One more step remains. Let us grant him his basis, and let us pass from the above negative criticism to a positive criticism. As before, I decline to follow the practice of talking in abstracts instead of in concretes, and contend that, difficult as it may be to see how natural selection has in all cases operated, we ought, at any rate, to trace out its operation whenever we can, and see where the hypothesis lands us. According to Prof. Weismann's admission, for production of the Amazon-ant by natural selection "*many parts must have varied simultaneously and in harmony with one another*"; * and he names as such, larger jaws, muscles to move them, larger head, and thicker chitin for it, bigger nerves for the muscles, bigger motor centres in the brain, and, for the support of the big head, strengthening of the thorax, limbs, and skeleton generally. As he admits, all these parts must have varied simultaneously in due proportion to one another. What must have been the proximate causes of their variations? They must have been variations in what he calls the "determinants." He says:—

> "We have, however, to deal with the transmission of parts which are *variable* and this necessitates the assumption that just as many independent and variable parts exist in the germ-plasm as are present in the fully formed organism." †

Consequently to produce simultaneously these many variations of parts, adjusted in their sizes and shapes, there must have simultaneously arisen a set of corresponding variations in the "determinants" composing the germ-plasm. What made them simultaneously vary in the requisite ways? Pro-

* *Loc. cit.* p. 318. † *The Germ Plasm*, p. 54.

fessor Weismann will not say that there was somewhere a foregone intention. This would imply supernatural agency. He makes no attempt to assign a physical cause for these simultaneous appropriate variations in the determinants: an adequate physical cause being inconceivable. What, then, remains as the only possible interpretation? Nothing but *a fortuitous concourse of variations;* reminding us of the old "fortuitous concourse of atoms." Nay, indeed, it is the very same thing. For each of the "determinants," made up of "biophors," and these again of protein-molecules, and these again of simpler chemical molecules, must have had its molecular constitution changed in the required way; and the molecular constitutions of all the "determinants," severally modified differently, but in adjustment to one another, must have been thus modified by "a fortuitous concourse of atoms." Now if this is an allowable supposition in respect of the "determinants," and the varying organs arising from them, why is it not an allowable supposition in respect of the organism as a whole? Why not assume "a fortuitous concourse of atoms" in its broad, simple form? Nay, indeed, would not this be much the easier? For observe, this co-adaptation of numerous co-operative parts is not achieved by one set of variations, but is achieved gradually by a series of such sets. That is to say, the "fortuitous concourse of atoms" must have occurred time after time in appropriate ways. We have not one miracle, but a series of miracles!

Of the two remaining points in Professor Weismann's first article which demand notice, one concerns his reply to my argument drawn from the distribution of tactual discriminativeness. In what way does he treat this argument? He meets it by an argument derived from hypothetical evidence—not actual evidence. Taking the case of the tongue-tip, I have carefully inquired whether its extreme power of tactual discrimination can give any life-saving advantage in moving about the food during mastication, in detecting foreign bodies in it, or for purposes of speech; and have, I think, shown that the ability to distinguish between points one twenty-fourth of an inch apart is useless for such purposes. Professor Weismann thinks he disposes of this by observing that among the apes the tongue is used as an organ of touch. But surely a counter-argument equivalent in weight to mine should have given a case in which power to discriminate between points one twenty-fourth of an inch apart instead of one-twentieth of an inch apart (a variation of one-sixth) had a life-saving efficacy; or, at any rate, should have suggested such a case. Nothing of the kind is done or even attempted. But now note that his reply, accepted even as it stands, is suicidal. For what has the trusted process of panmixia been doing ever since the human being began to evolve from the ape? Why during thousands of generations has not the nervous structure giving this extreme discriminativeness dwindled away? Even supposing it had been proved of life-saving efficacy to our simian ancestors, it ought, acccording to Professor Weismann's own hypothesis to have disappeared in us. Either there was none of the assumed special capacity in the ape's tongue, in which case his reply fails, or panmixia has not operated, in which case his theory of degeneracy fails.

All this, however, is but preface to the chief answer. The argument drawn from the case of the tongue-tip, with which alone Professor Weismann deals, is but a small part of my argument, the remainder of which he does not attempt to touch—does not even mention. Had I never referred to the tongue-tip at all, the various contrasts in discriminativeness which I have named, between the one extreme of the forefinger-tip and the other extreme of the middle of the back, would have abundantly sufficed to establish my case—would have sufficed to show the inadequacy of natural selection as a key and the adequacy of the inheritance of acquired characters.

It seems to me, then, that judgment must go against him by default. Practically he leaves the matter standing just where it did.*

The other remaining point concerns the vexed question of panmixia. Confirming the statement of Dr. Romanes, Professor Weismann says that I have misunderstood him. Already (*Contemporary Review*, May 1893, p. 758, and Reprint, p. 66) I have quoted passages which appeared to justify my interpretation, arrived at after much seeking. Already, too, in this review (July, 1893, p. 54) I have said why I did

* While Professor Weismann has not dealt with my argument derived from the distribution of discriminativeness on the skin, it has been criticized by Mr. McKeen Cattell, in the last number of *Mind* (October 1893). His general argument, vitiated by extreme misconceptions, I need not deal with. He says :—"Whether changes acquired by the individual are hereditary, and if so to what extent, is a question of great interest for ethics no less than for biology. But Mr. Spencer's application of this doctrine to account for the origin of species [!] simply begs the question. He assumes useful variations [!]—whether of structure or habit is immaterial —without attempting to explain their origin." The only part of Mr. Cattell's criticism requiring reply is that which concerns the "sensation-areas" on the skin. He implies that since Weber, experimental psychologists have practically set aside the theory of sensation-areas : showing, among other things, that relatively great accuracy of discrimination can be quickly acquired by "increased interest and attention. . . . Practice for a few minutes will double the accuracy of discrimination, and practice on one side of the body is carried over to the other." To me it seems manifest that "increased interest and attention" will not enable a patient to discriminate two points where a few minutes before he could perceive only one. That which he can really do in this short time is to learn to discriminate between the *massiveness of a sensation* produced by two points and the massiveness of that produced by one, and to *infer* one point or two points accordingly. Respecting the existence of sensation-areas marked off from one another, I may, in the first place, remark that since the eye originates as a dermal sac, and since its retina is a highly developed part of the sensitive surface at large, and since the discriminative power of the retina depends on the division of it into numerous rods and cones, each of which gives a separate sensation-area, it would be strange were the discriminative power of the skin at large achieved by mechanism fundamentally different. In the second place I may remark that if Mr. Cattell will refer to Professor Karl Retzius's *Biologische Untersuchungen*, New Series,

not hit upon the interpretation now said to be the true one: I never supposed that any one would assume, without assigned cause, that (apart from excluded influence of disuse) the *minus* variations of a disused organ are greater than the *plus* variations. This was a tacit challenge to produce reasons for the assumption. Professor Weismann does not accept the challenge, but simply says:—"In my opinion all organs are maintained at the height of their development only through uninterrupted selection" (p. 332): in the absence of which they decline. Now it is doubtless true that as a naturalist he may claim for his "opinion" a relatively great weight. Still, in pursuance of the methods of science, it seems to me that something more than an opinion is required as the basis of a far-reaching theory.*

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vol. iv. (Stockholm, 1892), he will see elaborate diagrams of superficial nerve-endings in various animals showing many degrees of separateness. I guarded myself against being supposed to think that the sensation-areas are sharply marked off from one another; and suggested, contrariwise, that probably the branching nerve-terminations intruded among the branches of adjacent nerve-terminations. Here let me add that the intrusion may vary greatly in extent; and that where the intruding fibres run far among those of adjacent areas, the discriminativeness will be but small, while it will be great in proportion as each set of branching fibres is restricted more nearly to its own area. All the facts are explicable on this supposition.

* Though Professor Weismann does not take up the challenge, Dr. Romanes does. He says:—"When selection is withdrawn there will be no excessive *plus* variations, because so long as selection was present the efficiency of the organ was maintained at its highest level: it was only the *minus* variations which were then eliminated." (*Contemporary Review*, p. 611.) In the first place, it seems to me that the phrases used in this sentence beg the question. It says that "the efficiency of the organ was maintained at its *highest* level"; which implies that the highest level is the best and that the tendency is to fall below it. This is the very thing I ask proof of. Suppose I invert the idea and say that the organ is maintained at its right size by natural selection, because this prevents increase beyond the size which is best for the organism. Every organ should be in due proportion, and the welfare of the creature as a whole is interfered with by excess as well as by defect. It may be directly interfered with—as for instance by too big an eyelid; and it may be indirectly interfered with, where the organ is large, by needless weight and cost of nutrition. In the second place the question which here concerns us is not what natural selection will do with variations. We are concerned with the previous

Though the counter-opinion of one who is not a naturalist (as Professor Weismann points out) may be of relatively small value, yet I must here again give it, along with a final reason for it. And this reason shall be exhibited, not in a qualitative form, but in a quantitative form. Let us quantify the terms of the hypothesis by weights; and let us take as our test case the rudimentary hind-limbs of the whale. Zoologists are agreed that the whale has been evolved from a mammal which took to aquatic habits, and that its disused hind-limbs have gradually disappeared. When they ceased to be used in swimming, natural selection played a part—probably an important part—in decreasing them; since, being then impediments to movement through the water, they diminished the attainable speed. It may be, too, that for a period after disappearance of the limbs beneath the skin, survival of the fittest had still some effect. But during the latter stages of the process it had no effect; since the rudiments caused no inconvenience and entailed no appreciable cost. Here, therefore, the cause, if Professor Weismann is right, must have been panmixia. Dr. Struthers, Professor of Anatomy at Aberdeen, whose various publications show him to be a high, if not the highest, authority on the anatomy of these great cetaceans, has kindly taken much trouble in furnishing me with the needful data, based upon direct weighing and measuring and estimation of specific gravity. In the Black Whale (*Balænoptera borealis*) there are no rudiments of hind-limbs whatever: rudiments of the pelvic bones

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question—What variations will arise? An organ varies in all ways; and, unless reason to the contrary is shown, the assumption must be that variations in the direction of increase are as frequent and as great as those in the direction of decrease. Take the case of the tongue. Certainly there are tongues inconveniently large, and probably tongues inconveniently small. What reason have we for assuming that the inconveniently small tongues occur more frequently than the inconveniently large ones? None that I can see. Dr. Romanes has not shown that when natural selection ceases to act on an organ the *minus* variations in each new generation will exceed the *plus* variations. But if they are equal the alleged process of panmixia has no place.

only remain. A sample of the Greenland Right Whale, estimated to weigh 44,800 lbs., had femurs weighing together $3\frac{1}{2}$ ozs.; while a sample of the Razor-back Whale (*Balænoptera musculus*), 50 feet long, and estimated to weigh 56,000 lbs., had rudimentary femurs weighing together one ounce; so that these vanishing remnants of hind-limbs weighed but one-896,000th part of the animal. Now in considering the alleged degeneration by panmixia, we have first to ask why these femurs must be supposed to have varied in the direction of decrease rather than in the direction of increase. During its evolution from the original land-mammal, the whale has grown enormously, implying habitual excess of nutrition. Alike in the embryo and in the growing animal, there must have been a chronic plethora. Why, then, should we suppose these rudiments to have become smaller? Why should they not have enlarged by deposit in them of superfluous materials? But let us grant the unwarranted assumption of predominant *minus* variations. Let us say that the last variation was a reduction of one-half—that in some individuals the joint weight of the femurs was suddenly reduced from two ounces to one ounce—a reduction of one-900,000th of the creature's weight. By inter-crossing with those inheriting the variation, the reduction, or a part of the reduction, was made a trait of the species. Now, in the first place, a necessary implication is that this *minus* variation was maintained in posterity. So far from having reason to suppose this, we have reason to suppose the contrary. As before quoted, Mr. Darwin says that "unless carefully preserved by man," "any particular variation would generally be lost by crossing, reversion, and the accidental destruction of the varying individuals." * And Mr. Galton, in his essay on "Regression towards Mediocrity," † contends that not only do deviations of the whole organism from the mean size tend to thus disappear, but that deviations in its components

* *The Variation of Animals and Plants under Domestication*, vol. ii. p. 292.

† *Journal of the Anthropological Institute* for 1885, p. 253.

do so. Hence the chances are against such *minus* variation being so preserved as to affect the species by panmixia. In the second place, supposing it to be preserved, may we reasonably assume that, by inter-crossing, this decrease, amounting to about a millionth part of the creature's weight, will gradually affect the constitutions of all Razor-back Whales distributed over the Arctic seas and the North Atlantic Ocean, from Greenland to the Equator? Is this a credible conclusion? For three reasons, then, the hypothesis must be rejected.

Thus, the only reasonable interpretation is the inheritance of acquired characters. If the effects of use and disuse, which are known causes of change in each individual, influence succeeding individuals—if functionally-produced modifications of structure are transmissible, as well as modifications of structure otherwise arising—then this reduction of the whale's hind limbs to minute rudiments is accounted for. The cause has been unceasingly operative on all individuals of the species ever since the transformation began.

In one case see all. If this cause has thus operated on the limbs of the whale, it has thus operated in all creatures on all parts having active functions.

At the outset I intimated that I must limit my replies to those arguments of Professor Weismann which are contained in his first article. That those contained in his second might be dealt with no less effectually, did time and space permit, is manifest to me; but about the probability of this the reader must form his own judgment. My replies thus far may be summed up as follows:—

Professor Weismann says he has disproved the conclusion that degeneration of the little toe has resulted from inheritance of acquired characters. But his reasoning fails against an interpretation he overlooks. A profound modification of the hind limbs and their appendages must have taken place during the transition from arboreal habits to terrestrial habits; and dwindling of the little toe is an obvious consequence of disuse, at the same time that enlargement of the great toe is an obvious consequence of increased use.

The entire argument based on the unlike forms and instincts presented by castes of social insects is invalidated by an omission. Until probable conclusions are reached respecting the characters which such insects brought with them into the organized social state, no valid inferences can be drawn respecting characters developed during that state.

A further large error of interpretation is involved in the assumption that the different caste-characters are transmitted to them in the eggs laid by the mother insect. While we have evidence that the unlike structures of the sexes are determined by nutrition of the germ before egg-laying, we have evidence that the unlike structures of classes are caused by unlikenesses of nutrition of the larvæ. That these varieties of forms do not result from varieties of germ-plasms, is demonstrated by the fact that where there are varieties of germ-plasms, as in varieties of the same species of mammal, no deviations in feeding prevent display of their structural results.

For such caste-modifications as those of the Amazon-ants, which are unable to feed themselves, there is a feasible explanation other than Professor Weismann's. The relation of common ants to their domestic animals—aphides and coccids—which yield them food on solicitation, does not differ widely from this relation between these Amazon-ants and their domestic animals—the slave-ants. And the habit of being fed, contracted during the first stages of their parasitic life, when there were perfect males and females, may, during that stage, have become established by inheritance. Meanwhile the opposed interpretation—that this incapacity has resulted from the selection of those ant-communities the queens of which laid eggs that had so varied as to entail this incapacity—implies that a scarcely appreciable economy of nerve-matter advantaged the stirp so greatly as to cause it to spread more than other stirps: an incredible supposition.

As the outcome of these alternative interpretations we saw that the argument respecting the co-adaptation of co-operative parts, which Professor Weismann thinks is furnished to him by the Amazon-ants, disappears. The ancestral ants were conquering ants. These founded the communities; and hence those members of the present communities which are most like them are the Amazon-ants. If so, the co-adaptation of the co-operative parts was effected by inheritance during the solitary and semi-social stages. Even were there no such solution, the opposed solution will be unacceptable. These simultaneous appropriate variations of the co-operative parts in sizes, shapes, and proportions, are supposed to be effected by simultaneous variations in the "determinants" of the germ-plasms; and in the absence of an assigned physical cause, this implies a fortuitous concourse of appropriate variations, which carries us back to a "fortuitous concourse of atoms." This may just as well be extended to the entire organism. The old hypothesis of special creations is more consistent and comprehensible.

To rebut my inference drawn from the distribution of discriminativeness, Professor Weismann uses not an argument but the blank form of an argument. The ability to discriminate one twenty-fourth of an inch by the tongue-tip *may* have been useful to the ape: no conceivable use being even suggested. And then the great body of my argument derived from the distribution of discriminativeness over the skin, which amply suffices, is wholly ignored.

The tacit challenge I gave to name some facts in support of the hypothesis of panmixia—or even a solitary fact—is passed by. It remains a pure speculation having no basis but Professor Weismann's "opinion." When from the abstract statement of it we pass to a concrete test, in the case of the whale, we find that it necessitates an unproved and improbable assumption respecting *plus* and *minus* variations; that it ignores the unceasing tendency to reversion; and that it implies an effect out of all proportion to the cause.

It is curious what entirely opposite conclusions men may draw from the same evidence. Professor Weismann thinks he has shown that the "last bulwark of the Lamarckian principle is untenable." Most readers will hold with me that he is, to use the mildest word, premature in so thinking. Contrariwise my impression is that he has not shown either this bulwark or any other bulwark to be untenable; but rather that while his assault has failed it has furnished opportunity for strengthening sundry of the bulwarks.

PRINTED BY BALLANTYNE, HANSON AND CO
LONDON AND EDINBURGH

DESCRIPTIVE SOCIOLOGY;

OR GROUPS OF

SOCIOLOGICAL FACTS,

CLASSIFIED AND ARRANGED BY

HERBERT SPENCER,

COMPILED AND ABSTRACTED BY

DAVID DUNCAN, M.A. (now Professor of Logic and Director of Studies at Madras); RICHARD SCHEPPIG, Ph.D.; and JAMES COLLIER.

EXTRACT FROM THE PROVISIONAL PREFACE.

Something to introduce the work of which an instalment is annexed, seems needful, in anticipation of the time when completion of a volume will give occasion for a Permanent Preface.

In preparation for *The Principles of Sociology*, requiring as bases of induction large accumulations of data, fitly arranged for comparison, I, some twelve years ago, commenced, by proxy, the collection and organisation of facts presented by societies of different types, past and present; being fortunate enough to secure the services of gentlemen competent to carry on the process in the way I wished. Though this classified compilation of materials was entered upon solely to facilitate my own work; yet, after having brought the mode of classification to a satisfactory form, and after having had some of the Tables filled up, I decided to have the undertaking executed with a view to publication; the facts collected and arranged for easy reference and convenient study of their relations, being so presented, apart from hypothesis, as to aid all students of social science in testing such conclusions as they have drawn and in drawing others.

The Work consists of three large Divisions. Each comprises a set of Tables exhibiting the facts as abstracted and classified, and a mass of quotations and abridged abstracts otherwise classified on which the statements contained in the Tables are based. The condensed statements, arranged after a uniform manner, give, in each Table or succession of Tables, the phenomena of all orders which each society presents—constitute an account of its morphology, its physiology, and (if a society having a known history) its development. On the other hand, the collected Extracts, serving as authorities for the statements in the Tables, are (or, rather will be, when the Work is complete) classified primarily according to the kinds of phenomena to which they refer, and secondarily according to the societies exhibiting these phenomena; so that each kind of phenomenon as it is displayed in all societies, may be separately studied with convenience.

In further explanation I may say that the classified compilations and digests of materials to be thus brought together under the title of *Descriptive Sociology*, are intended to supply the student of Social Science with data, standing towards his conclusions in a relation like that in which accounts of the structures and functions of different types of animals stand to the conclusions of the biologist. Until there had been such systematic descriptions of different kinds of organisms, as made it possible to compare the connexions, and forms, and actions, and modes of origin, of their parts, the Science of Life could make no progress. And in like manner, before there can be reached in Sociology, generalisations having a certainty making them worthy to be called scientific, there must be definite accounts of the institutions and actions of societies of various types, and in various stages of evolution, so arranged as to furnish the means of readily ascertaining what social phenomena are habitually associated.

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