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BY

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To

Thomas Arthur Bramsdon

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CHAPTER I

"DURING the quarter of a century which has elapsed since Biology began to occupy itself again with general problems, at least one main fact has been made clear by the united labours of numerous men of science, viz. the fact that the theory of descent, the idea of development in the organic world, is the only conception as to the origin of the latter which is scientifically tenable. It is not only that, in the light of this theory, numerous facts receive for the first time a meaning and a significance; it is not that, under its influence, all the ascertained facts can be harmoniously grouped together; but in some departments it has already yielded the highest results which can be expected from any theory, it has rendered possible the prediction of facts, not indeed with the absolute certainty of calculation, but with a high degree of probability. It has been predicted that man, who in the adult state only possesses twelve pairs of ribs, would be found to have thirteen or fourteen in his embryonic state; it has been predicted that, in this early period in his existence, he would possess the insignificant remnant of a very small bone in his wrist, the so-called *os centrale*, which must have existed in the adult condition of his extremely remote ancestors. Both predictions have been fulfilled, just as the planet Neptune was discovered after its existence had been predicted from the disturbance induced in the orbit of Uranus.

"That existing species have not arisen independently, but have been derived from other and mostly extinct species, and that on the whole this development has taken place in the direction of greater complexity, may be maintained with the same degree of certainty as that with which astronomy asserts that the earth moves round the sun; for a conclusion may be arrived at as safely by other methods as by mathematical calculation.

"If I make this assertion so unhesitatingly I do not make it in the belief that I am bringing forward anything new, nor because I think that any opposition will be encountered, but simply because I wish to begin by pointing out the firm ground on which we stand, before considering the numerous problems which still remain unsolved.

"Such problems appear as soon as we pass from the facts of the case to their explanation; as soon as we pass from the statement, The organic world has arisen by development, to the question, But how has this been effected, by the action of what forces, by what means, and under what circumstances?

"In attempting to answer these questions we are very far from dealing with certainties; and opinions are still conflicting. But the answer lies in the domain of future investigation, that unknown country which we have to explore.

"It is true that this country is not entirely unknown, and if I am not mistaken, Charles Darwin, who in our time has been the first to revive the long dormant theory of descent, has already given a sketch, which may well serve as a basis for the complete map of the domain; although perhaps many details will be added, and many others taken away. In the principle of natural selection, Darwin has indicated the route by which we must enter this unknown land."—WEISMANN.

"The continuity of the germ plasm, like Darwin's selection, is a fact not a theory."—HAYCRAFT.

"'Struggle for existence,' as perhaps it was in Mr. Darwin's world of advancing beasts and developing vegetables. But now the plan is so turned about by the arrival of man on the scene, and by his civilization, that you cannot watch even Darwin and Huxley themselves without seeing that the struggle that they and other good men wage is no struggle for existence but a struggle against mere existence. The struggle for existence is brutal life; a struggle to do something more than exist is human life—the mission of the human soul. What is the use of alcohol in such a struggle? The question is a wide one. It might lead us to inquire what that is which men want to obtain beyond mere existence. Watching some eminent teachers, you might suppose it to be a very detailed knowledge of the common frog.

But men are human because they look upwards and to the future, not downwards to the past. And Darwin and Huxley, and even Haeckel, will in time learn that over-scrutinizing insufficient evidence does not make it more complete."—MOXON.

Above I give three extracts—one from the writings of a very eminent naturalist, the second from those of an eminent physiologist, and the third from those of a physician, also eminent. They disclose a difference of opinion which is remarkable. Professor Weismann scarcely deigns to discuss the theory of evolution, since, in his opinion, it must be considered as proved with as much certainty as is the fact that the world moves round the sun, or as if it had been demonstrated mathematically; and he adds, that we have now only to discuss the details, merely to fill in the minutiæ of the map, the outlines of which Darwin has already sketched. Dr. Haycraft also thinks that the theory of evolution has passed from the category of mere hypothesis into the category of that which must be accepted as proven fact. But Dr. Moxon appears to think that, if a struggle for existence leading to evolution ever did occur, it has now ceased so far at least as man is concerned, and the struggle has become one against mere existence, whatever that may mean, and he adds—"Darwin, Huxley, and even Haeckel will in time learn that over-scrutinizing insufficient evidence does not make it more complete."

His attitude is very characteristic of that of the vast majority of the general public, and characteristic also of that of the majority of medical men, who, while observing the effects of disease on man the individual, have signally failed to observe its effects on man the species. Had Dr. Moxon, when he wrote his essay on alcoholism, which in its effects is a disease, occupied a wider outlook, had he considered the species as well as the individual, he might perhaps have been led to conclusions which are as true as they are surprising.

Taking into account this attitude of the general public, and in particular that of the mass of medical men, to whom, if to any, this work should prove of interest, and considering also that a wide audience must be appealed to if it is to have that amount of practical usefulness which the author hopes for it, it seems needful, before proceeding with the main body of the work, to set forth as briefly and clearly as possible certain biological data on which the argument is founded; especially as, so far as the author is aware, they have never yet been explained in sufficiently simple terms to be comprehensible by the general reader; and more especially since, in his conception of the process of evolution, the author differs somewhat from accepted views, or rather since in his opinion acknowledged authorities have not recognized or have not laid sufficient stress on certain processes of evolution which appear to him of the greatest importance. This book is therefore divisible into two parts: in the first the problem of evolution in general is very briefly dealt with, but an attempt is made to penetrate somewhat deeper in certain directions than has hitherto been done; in the second part, the conclusions arrived at in the first are applied to the problem of man's present evolution, and an endeavour is made to show that this evolution is proceeding in a direction hitherto altogether unsuspected.

To many, and, surprising as it may seem, even to some medical men, in spite of what ought to be a scientific training, the theory of evolution means nothing more than the theory of the descent of man from the monkey. In reality it means much more; it teaches that plant and animal types have not persisted unchanged from the time life first became possible on the cooling surface of the globe, but that all types known to us have arisen by a process of gradual evolution from pre-existing types, and that this process of evolution has generally been in an upward direction from the low to the high, from the small to the great, from the simple to the complex. Pushed to its logical conclusion the theory teaches yet more; it teaches, as the only hypothesis scientifically tenable, that life originally arose by a process of evolution from that which was non-living; that under conditions of which we are ignorant, of moisture, or of heat, light, electricity, or other of the protean forms of energy, non-living chemical compounds did in the beginning of life overpass the border space which divides the non-living from the living, and become living beings.[1] The lowest living beings, or rather those highest chemical compounds which first display signs, however slight, of what is called life, if any such now exist on earth, probably exist in masses so minute and so little differentiated from mere chemical compounds, as to be beyond all devisable means of observation. Regarding them therefore there is no evidence to offer, but as regards the evolution of higher life from lower life, the evidence is so vast and so decisive that it is impossible to avoid coming to the conclusion that Darwin, Huxley, and even Haeckel did not over-scrutinize insufficient evidence, but that Dr. Moxon under-scrutinized overwhelming evidence, that, in fact, he adopted the device familiar to theologians and dear to them, of abusing evidence instead of examining it.

But quite apart from all evidence that evolution *has* occurred, the conditions of life are such that we may

[1] This is of course the theory of spontaneous generation, which is popularly supposed to be quite exploded. What is exploded is that such highly organized beings as the infusorians arose spontaneously.

infer from them with mathematical certainty that it *must have* occurred. It is a matter of common knowledge and experience (1) that in both the animal and vegetable kingdoms every individual differs somewhat from every other individual, but that (2) while the offspring is never an exact copy of the parent, there is yet a tendency for the peculiarities of the parent to reappear in the offspring in a greater or lesser degree. Whence it is clear, if some individuals of a species possess a peculiarity, such as a superior keenness of sight, or of hearing, or of scent, or a power of muscle, or a capacity of resisting heat, or cold, or hunger, or thirst, or disease, &c., in a greater degree than that possessed by the other members of the species, under conditions which render this peculiarity of importance in the struggle for existence, that the individuals which possess the peculiarity in the greater degree will be at an advantage, and will, on the whole, survive in greater numbers, and, consequently, other things equal, have a more numerous offspring than those which possess the peculiarity in the lesser degree. And further, since the offspring, while inheriting their parents' peculiarities, tend to vary somewhat from them, there will, in the next generation, probably be some who have the peculiarity in a greater degree than their parents, and others who have it in a lesser degree, when, if the conditions remain the same, there will be such a new survival of the fit, and such a fresh elimination of the unfit, as will leave the survivors in the third generation with the peculiarity developed, in however slight a degree, more than it was developed in their grandparents; and it follows that this process, repeating itself through innumerable generations, and during long epochs of time, will at length develop the peculiarity to a point beyond which any further increase is no longer useful, or to a point beyond which it is so little useful, that those individuals who possess the increase are no longer at such an advantage in the struggle for existence as to survive to an appreciable extent beyond those who have it not. Therefore since individuals vary each one from all others, since the offspring, while varying somewhat from their parents, tend to transmit the peculiarities of their parents, and since individuals that vary favourably tend of course to survive and have offspring, whereas individuals that vary unfavourably tend to be eliminated and have no offspring, it is deductively so certain that whenever there is a struggle for existence,—and throughout nature there is always such a struggle,—the survival of the fittest *must* lead to evolution, that it is scarcely necessary to appeal to facts for inductive confirmation. If, however, we do seek such confirmation, we find it in three great bodies of facts collected mainly in different fields of research, each of which is separately decisive, and which collectively furnish confirmation so absolute that, practically speaking, no student of biological science now believes otherwise than that the whole organic world arose by a process of evolution.

The first great mass of evidence is furnished by the science of Comparative Anatomy, especially by that branch of it which deals with Comparative Embryology. In the interests of the general reader, we may with advantage defer the consideration of it.

The second great mass of evidence is furnished by the science of Paleontology, which teaches, on the evidence of fossil remains, that the earth has not always been inhabited by the same forms of life, but that during the whole vast period which intervened between the deposit of the earliest fossiliferous rocks and the present age, there occurred a constant but gradual change of form, as a result of which type shaded into type, generally in an upward direction, and which can have been due to evolution only, or, as the single other alternative, to an immense series of special creations, made not only when life began, but also during its entire continuance, in exact imitation of a process of evolution—a preposterous theory, for which there is no more warrant than for the hypothesis that our various breeds of dogs have not descended from a common ancestry, but were separately created.

The third great mass of evidence is that furnished by experiment. It is therefore the most conclusive of all, since any theory which can be proved experimentally may be said to be proved decisively. For instance, if we have a theory that the boiling-point of water varies with the atmospheric pressure, and find on experiment that it does so vary, we may conclude that we have proved our theory beyond cavil. As a disputed point is involved, which it will be advantageous to discuss thus early, we will consider this part of the evidence more at length than we did the rest. The theory of evolution may be put to the test of experiment. We may take any species of plant and animal, and by copying the process of natural selection, by destroying those individuals of the species which we regard as unfit,—*i.e.* those which vary unfavourably as regards any peculiarity we wish to develop,—and by allowing only those individuals who vary favourably as regards that peculiarity to continue the race, we may develop the chosen peculiarity in the chosen species to almost any extent; the one condition being, that our operations shall extend over a sufficient number of generations. The efforts of man to improve his cultivated plants and domesticated animals may be described as a lengthened series of such experiments, conducted on a gigantic scale, with the result that he has been able to vary plant and animal structures almost at will, and so greatly that, in the absence of scientific and historic testimony, no one could guess that the peach and nectarine, for instance, were cultivated varieties of the wild almond, or that the various breeds of dogs have a common and, geologically speaking, very recent ancestry.

It may be objected however, and it often is objected, that natural selection is not the same thing as artificial selection; but the objection cannot be sustained, for in effect it is the same thing, artificial selection being merely natural selection with man as the governing agent. Man, for instance, has caused the evolution of speed in the greyhound and in the race-horse by breeding, on the whole, from the swiftest animals of each species; in like manner the carnivora have caused the evolution of speed in the hare and the antelope by permitting, other things equal, only the swiftest to continue the race. Artificially caused evolution differs from naturally caused evolution only in that it is usually more rapid; for man, in his endeavours to produce a wished-for evolution, fixes his attention on but a few traits, and breeds with the intention of developing those traits only. Thus in the case of the greyhound he has bred, generally speaking, with the intention of producing a very swift animal with sight keen enough to see the prey, and jaws and teeth adapted to seize and kill it. In the case of the race-horse he has bred with the intention of producing another swift animal, strong enough and tractable enough to carry him. But natural selection in the hare and the antelope has developed not only speed, but also hearing, watchfulness, and many other traits which are as essential.

Other things—*i. e.* other essential traits—equal, pre-eminence in one or more essential traits is favourable to survival. But because in a state of nature many traits are essential, evolution by natural selection must proceed along many lines, and, in consequence, be correspondingly slow. To take an example—a buck and a doe antelope, whose sight and hearing respectively are keener than the average, but whose other qualities are equal to the mean of the species, are at an advantage as regards the sense of sight and hearing respectively. If they mate, the offspring, while attaining to the specific mean in the other qualities, will in general surpass it as regards sight and hearing, but will fall below the male parent as regards the sight, and the female parent as regards the hearing; for the comparative dullness of the male's hearing will militate in the offspring against the keenness of the female's hearing, and *vice versâ* as regards the sight. So also with the offspring of a buck pre-eminent for endurance and a doe pre-eminent for speed. Then, if the offspring of the two pairs mate, their offspring will surpass, but in a lessening degree, the specific mean in keenness of sight and hearing, in speed and in endurance, but will fall below each of the grandparents as regards the one quality in which that grandparent excelled, though they will surpass each of them as regards all the other qualities. Therefore if the essential qualities are numerous,—and they always are so in the case of wild animals,—the descendants of numerous ancestors, each one of whom was pre-eminent in one of these qualities, will tend more and more, generation after generation, to approach the general racial mean as regards all these qualities;—which appears to lead to the absurd conclusion, that in the end there will be no evolution at all. But in coming to that conclusion we shall have forgotten that the offspring never present an exact mixture of the qualities of the parents, but that spontaneous variations, caused we know not how, continually arise, in consequence of which one or more of the offspring of the keen-sighted father and the quick-hearing mother (for instance) may surpass both parents as regards both qualities; and though there is reason, as we shall presently see, to conclude that on the whole the offspring are usually inferior to the parents, nevertheless, since they are generally numerous, and the process of selection almost always severe, the survivors in each generation, as a rule, surpass their parents in the sum of their qualities; and as a result, there is usually evolution as regards the whole race even when the essential qualities are many. Moreover, we have assumed for the sake of simplicity that each ancestor was pre-eminent in one quality only, but of course, as constantly happens in nature, a single individual may approach pre-eminence in several or many qualities, or conversely, may fall below the specific mean in several or many qualities: in the latter case he tends of course to be eliminated, and to leave no offspring, and therefore to have no influence on the future of the race; but in the former he tends, even more than if he were eminent in one quality only, to survive and to leave offspring who, inheriting his characteristics, tend as a result to survive in greater numbers than the offspring of the less happily endowed, and therefore to render concurrent evolution in several directions more rapid.

Lastly, the statement that artificial selection differs from natural selection in that but a few qualities are developed by it, is true in a very limited sense only; for instance, by causing the evolution of the greyhound man has necessarily caused the evolution of a great number of subsidiary qualities, each subserved by a number, in some cases by an immense number, of finely co-ordinated structures. Therefore though speed alone has been aimed at, yet to obtain it evolution in many directions has been necessary (*e. g.* in "wind," "limb," circulatory and nervous systems, &c.). On this score then there is no essential difference between artificial and natural selection, and the evolution which resul from the former therefore furnishes experimental (*i.* conclusive) proof of the evolution which results fro. the latter.

The reduction towards the specific mean which inte breeding tends to bring about in the qualities of ind viduals as exhibited in their descendants, has cause some biologists to insist that isolation is a necessa antecedent to evolution.[1] But if we bear in mind tl fact (to be more fully discussed in a future page) th evolution proceeds *not on lines of traits, however favou able, which occur infrequently or abnormally, but on lin of traits common to all the individuals of the whole specie that is, in which every individual rises above or falls belo the specific average*; and bear in mind also, that general those individuals who on the whole vary fortunately regards these traits, survive and have offspring, where generally those who on the whole vary unfortunatel are eliminated and have no offspring, we shall have n difficulty in understanding that evolution is qui possible in the absence of isolation. What is n possible in the absence of isolation is evolution o diverging lines. For instance, if the whole of a specie of antelope inhabited an open plain under conditio common to all there might be evolution, but it woul be in a direction common to the whole species. But i owing to any circumstance, such as a deficiency of foo supply, a portion of the species separated from the res and migrated permanently to forest land, then th two divisions of the species, being unable to interbree

[1] "Without isolation, or the prevention of free intercrossin organic evolution is in no case possible. Isolation has been tl universal condition of modification. Heredity and variabilit being given, the whole theory of organic evolution becomes theory of the causes and conditions which lead to isolation"-ROMANES.

and differently conditioned as to the environment, would develop on divergent lines of evolution.

The conclusion, deductively arrived at, that the conditions under which life has existed and still exists, are such that evolution must have occurred and still occurs, is therefore decisively confirmed by the conclusion, inductively arrived at, that evolution certainly has occurred, and therefore, though in the subsequent pages of this work many proofs will incidentally be afforded of the actuality of Organic Evolution, in future it will be assumed that the truth of it is admitted, and we shall endeavour only to fill in the details of the map as to the fidelity of the outlines of which no well-informed man any longer entertains a doubt.

The upward march of life from the earliest beginnings may be compared to that of a horde of men, leaving their old habitations and entering new lands; travelling ever forwards, but ever sending out branch swarms that part from the parent horde, never to reunite with it, and ever leaving some of their members behind on the way, some of whom may journey backwards; such hordes as those which in ancient times came from the East, settled the countries they passed over, sent offshoots to the North and South, and rolled on the tide of conquest till they destroyed the old Roman Empire. The lowest, or in other words the least differentiated and specialized forms of life, may be compared to those members of the horde that stayed behind in the original habitat, the intermediate forms to those that halted and settled by the way, and the highest forms to those that journeyed till they reached the farthest limits of the wanderings. The comparison is made yet closer if we imagine, as generally true of life, that which is generally true of emigrant swarms of men, namely, that those that stayed in the original habitat, that those that halted, diverged, or retrogressed by the way, and that those that reached the farthest limits, did not remain altogether unchanged, but changed somewhat as ages passed and generations lived and died; those that remained in the original habitat having changed least, those that wandered farther having changed more, and those that wandered farthest having changed most, and this for the reason that though the environment nowhere remained absolutely unaltered, yet for those that travelled farthest it altered most; so those Aryans who dwell in or near the original habitat of the race have changed least, those who halted by the way have changed more, and those that journeyed farthest to the West, or diverged to the North or South, have changed most. So also in the march of life, the lowest organisms may be considered as having stayed behind at or near the original starting-point; the sponges as having diverged from the main road at an early period; the Cœlenterates, the Mollusks, the Annulosa to have halted by the way at a later stage; whereas the Vertebrates, with mammals at their head, and man at the head of the mammals, may be considered as having reached the farthest limits. But just as it becomes increasingly difficult the farther we penetrate into historical antiquity to trace the descent of the nations of any race of human beings, —the Teutonic for example,—not only for the reason that the peoples who have travelled farthest have changed so much, have undergone evolution, and because the peoples who halted by the way or stayed behind have changed also, but because some of the links of the chain are broken, because some of the nations which halted by the way are lost and cannot be recognized, or because they have perished utterly in the struggle for existence; so also the farther we penetrate into geological antiquity, the harder it becomes to trace the descent of species, the more numerous become the broken links, the wider the gaps; nevertheless, however numerous the broken links, however wide the gaps, just as it is in general possible to trace with more or less accuracy the descent of the nations, so it is in general possible to trace the descent of species.

CHAPTER II

It has already been pointed out, that the conditions of life are usually such as to cause the number of traits necessary for survival to be numerous, and that this again causes the process of evolution to be slow, though it is no bar to it as some biologists have supposed. But however slow the multiplicity of essential traits may render evolution, it is difficult at first sight to understand why evolution should be so extremely slow as it generally is. Every plant or animal pair usually gives origin to numerous offspring, sometimes the offspring are numbered by millions (*e. g.* the offspring of cod-fish); yet the number of individuals in each species does not increase as a rule, whence it is clear that nature, by destroying the majority of the offspring, exercises her power of selection with extreme stringency. Under the circumstances we might *à priori* expect evolution to be rapid. But what are the facts? Some types, such as the lamp-shells, have persisted almost unchanged during enormous epochs of time, and in almost all cases nature requires hundreds, nay thousands or tens of thousands of years to bring about comparatively trifling changes of structure. Why is this?

The answer is probably found if we consider certain groups of apparently unconnected facts. Children who do not especially resemble their parents often resemble their grandparents, or even more remote ancestors; at any rate they more often resemble an ancestor than they do any other given individual. For instance, if before a child is born we indicate any individual of our acquaintance, the chances are immensely greater that the child when grown will resemble a particular ancestor than that he will resemble the person indicated. That is to say, there is a greater tendency for the child to vary from its parents in the direction of its ancestry than in any other given direction. Now if the parent has varied from the grandparent in any other direction than towards the ancestry, this, the child's variation towards the ancestry, is evidently an omission of the last step made in the evolution of the race. It is what is known as *atavism*,[1] and is sometimes carried so far that the variations (*i. e.* the evolution) of hundreds of ancestors nearest it are omitted, and the child resembles some extremely remote ancestor, being ape-like in features, especially in times of famine, when want of nourishment checks the development, and sometimes being covered with long hair. So also the progeny of a pair of thoroughbred horses is sometimes an "arrant weed," or it may revert to an even more remote ancestor, by showing zebra-like stripes. So also "a blue pigeon, like the ancient Columba Livia, may be hatched in the dovecot." On the other hand, the progeny of a pair of ordinary horses never has the peculiar characteristics of the racer; nor has the progeny of a blue pigeon ever those of the pouter or fantail.

It is well known that race-horses have been developed by an extremely stringent process of artificial selection. I believe I am right in saying that when this process

[1] 'Atavism, which is the name given to the recurrence of ancestral traits, is proved by many and varied facts. In the picture-galleries of old families, and on the monumental brasses in adjacent churches, are often seen types of feature that are still, from time to time, repeated in members of their families.' —Herbert Spencer's *Principles of Biology*, vol. i. p. 252.

began, the rate of speed evolution (which depends on structural evolution) was comparatively rapid, but that this rate has been gradually slowing, so that now the race-horses of one decade do not greatly surpass the race-horses of the previous decade. To some extent this, of course, may be due to the fact that to obtain any increment of speed a more than corresponding increment of force must be put forth, as is exemplified by the fact that an oarsman, to increase his speed from two to four miles an hour, must use more than twice the amount of force, and to increase his speed to six miles an hour, must use a still more disproportionate amount of force. But the race-horse surpasses the ordinary horse not so much because it is more powerful, but because it is better shaped for speed, just as the racing boat surpasses in speed the ordinary rowing boat for the same reason. We can hardly conclude that the evolution of the race-horse is now slow because he has nearly reached the perfection of shape for speed, for another of the equidæ, the wild ass, a smaller animal, is said to be swifter. The decreasing rate of evolution, therefore, must be set down to another cause.

If we mate two ordinary horses there is a fair prospect that some at least of the offspring will be as fine, or even finer, animals than either the sire or the dam. But if we mate a Derby winner with a good mare—a better mare than his dam—we shall find that the majority of his progeny are inferior to himself, and that only very exceptionally does he procreate a son or a daughter that can match him for speed. Thus, I venture to say that very few, if any, of the sons or daughters of Ormonde will be his equals. Rapid evolution, therefore, rapidly becomes more slow. Why? Evidently because there is a tendency for the offspring to omit more or less of the latest evolution of the race and revert to the ancestry, sometimes the remote ancestry, and therefore in cases in which evolution has been rapid, as with race-horses, this reversion produces wide divergences of structure from the parent, but in cases in which the evolution has been slow, as with lamp-shells, this reversion produces very slight divergences of structure. For instance, to take an extreme example, that amount of reversion which in the offspring of a man would result in a fish, would in the offspring of a lamp-shell result in another lamp-shell, only a little modified. For this reason it is that characters long present in a race are, as is well known, little capable of undergoing retrogression. Since race-horses have been rapidly evolved under a process of extremely stringent selection, reversion of the offspring to a comparatively recent ancestor causes a considerable amount of divergence from the parent. Let us try to imagine what would happen among race-horses if the stringency of selection were relaxed, or rather if selection in this case were altogether abolished, so that the inferior animals were allowed to propagate the species as well as the superior. I think we may prophesy with tolerable confidence. The selective action being withdrawn, the tendency towards reversion, towards atavism, would operate uncontrolled, and the race would rapidly revert to its ancestor, the ordinary horse.

We are now in a position to understand why evolution caused by natural selection is so extremely slow. In every species natural selection as a cause of evolution, and atavism as a cause of retrogression, are constantly at war. If the one force predominate we have evolution; if the other, retrogression; if the two forces balance one another, the species in which this occurs undergoes for the time neither evolution nor retrogression. Rapid evolution is accompanied by an increasing tendency to retrogression, because in such a case any ancestor reverted to differs more in structure from the last member of the race than a corresponding ancestor in a race that has been more slowly evolved. I must, however, guard myself against possible misconception. When I speak of atavism as a cause of retrogression, I do not necessarily mean that in each case of atavism the whole organism in all its parts retrogresses. This must rarely happen, especially when evolution has been slow. What I do mean is that, generally speaking, atavism, by causing various small retrogressive changes in various parts of the organism, is by its cumulative effects a check to evolution, and even sometimes a cause of retrogression. An organism may undergo retrogressive changes in some of its parts owing to the severity of natural selection becoming relaxed as regards them, at the same time that, owing to increased severity of natural selection, it is undergoing evolution as regards other parts. Thus in the case of a species of bird, the habits of which are becoming less and less aerial, natural selection will cause the legs to undergo evolution, but atavism the wings to undergo continued retrogression backwards, successively from recent ancestors to remoter ancestors, till, if the power of flight no longer influences the survival rate, there will be an approximation in structure, as far as the wings are concerned, to the structure of a remote wingless or rather limbless ancestor; but an approximation only, for, as regards such complex organs as wings, we must not expect to find that a degenerate example in any stage of the retrogression resembles very closely an ancestral wing, in a corresponding stage of the evolution; for instance, we must not suppose that the wing of the apteryx is a close reproduction of a remote ancestral form, for the reason that though the wing in all its parts undergoes retrogression, yet, like the parts of a complex organism, all the parts of the wing do not undergo equal retrogression. Various causes prevent the retrogression from being everywhere equal, since the conditions under which retrogression takes place cannot be an exact reversal of those under which evolution occurred; for instance, the retrogressing wing of the apteryx is attached to a body which has not undergone a *pari passu* retrogression. Moreover, even while the wing was undergoing evolution, parts of it must have undergone retrogression; this and that structure or part of a structure in it, useless in a wing, but useful in the organ which preceded it, must have tended to disappear at the same time that other structures or parts of structures, more useful in a wing than in the organ that preceded it, tended to increase; the two warring forces of natural selection and atavism thus operating to bring the wing and the organism to which it belongs into completer harmony with the environment. Thus all the parts of the degenerated wing of an apteryx cannot have undergone equal retrogression, and therefore the resemblance between the degenerated wing and the ancestral form in any of its stages cannot be very close.

It follows, if the above theory be correct, that while it is not possible by means of selection, natural or artificial, to bring about rapid and extensive evolution, since such evolution must soon be checked by the increasing tendency towards far-reaching reversion, it is possible by means of selection to bring about rapid and extensive, indeed unlimited, retrogression. This hypothesis, however, is at variance with accepted doctrines. Mr. Herbert Spencer says—

"Leaving open the question whether, in indefinite time, indefinite modifications may not be produced; experience proves that within assigned times, the changes wrought in races of organisms by changes of conditions fall within narrow limits. We see, for instance, that though by discipline, aided by selective breeding, one variety of horse has had its locomotive power increased considerably beyond the locomotive powers of other varieties; yet that further increase takes place, if at all, at an unappreciable rate. The different kinds of dogs, too, in which different forms and capacities have been established, do not show aptitudes for diverging in the same directions at considerable rates. In domestic animals generally, certain accessions of intelligence have been produced by culture, but accessions beyond these are inconspicuous. It seems that in each species of organism there is a margin for functional oscillations on all sides of a mean state, and a consequent margin of structural variations; that it is possible rapidly to push functional and structural changes towards the extreme of this margin in any direction, both in an individual and in a race; but that to push these changes further in any direction, and so to alter the organism as to bring its mean state up to the extreme of the margin in that direction, is a comparatively slow process."—*Principles of Biology*, vol. i. p. 188.

The domesticated dog is presumably descended from one or more of the different wild varieties, or from their relatives the wolves. Now, considering the length of time dogs have been domesticated, and the severity of the selection to which they have been subjected, our largest dogs, the St. Bernards, Newfoundlands, mastiffs, boarhounds, do not very greatly exceed wild dogs or wolves in size, nor do our most intelligent dogs greatly surpass them in intelligence; but our smallest dogs, some of them little bigger than rats, are very much smaller, and some of our tame breeds are exceedingly stupid. Clearly as regards dogs, we have been able to produce little evolution, but great retrogression. In some breeds there has undoubtedly been evolution in this or that respect—in the St. Bernard of size, in the mastiff of strength, in the greyhound of speed, in the bulldog of courage; but on the whole the tendency has been towards retrogression—a retrogression which has operated unequally on the animal parts, physical and mental, and has not infrequently been concomitant with partial evolution. To this unequal retrogression, combined in some cases with limited evolution, is probably to be attributed the diversity of shapes and capacities which characterize our many domesticated breeds. Thus the lapdog and the pug are probably examples mainly of retrogression, not evolution; probably their peculiarities of shape and character are due mainly to the fact that man has permitted or caused retrogression in them in certain directions by selecting the most degenerate individuals in these respects, at the same time that he has checked retrogression, and even caused some evolution, in other directions. Thus also the bulldog has undergone retrogression as regards size, but evolution as regards courage; the greyhound retrogressed as regards strength and power of scent, but evolved as regards speed; the St. Bernard has gained in size, but lost in speed; and so also with all other breeds.

Dogs among civilized peoples especially have often been the mere playthings of their owners, who, taking unconscious advantage of the tendency towards retrogression, have gratified their caprices by rearing many grotesque varieties. As regards the equidæ and bovidæ the case has been entirely different. Reared almost solely for use, their powers and capabilities preserved to the utmost, the domesticated varieties show little retrogression and some evolution. But suppose we selected a number of the swiftest race-horses, or the largest dray-horses, or the best milk-giving cows, and bred from them, selecting afterwards in succeeding generations always the slowest, the smallest, or the least milk-giving individuals respectively wherewith to continue the race, is there any limit to the retrogression which would result in "assigned" time? We certainly should not bring about the reappearance, for instance, of the remotely ancestral horse, the five-toed Eohippus, for the form of the modern horse, like that of other animals, has resulted not only from evolution, but also from retrogression, the disappearance of the four lateral toes being examples mainly of the latter, while the great size of the middle toe is an example of the former; but it cannot be doubted that we should cause, in a time comparatively short as compared to that which elapsed during the evolution, such extreme retrogression as would result in an animal quite incapable of existence.

The improvements in our cultivated plants are the result of centuries of stringent selection; and here again it cannot be doubted, that if the process by which they were evolved were reversed, were we to propagate only from the most inferior plants, that the rate of retrogression would be much more rapid than was the rate of evolution.

It is to be noted, however, that some cultivated plants exhibit comparatively extreme evolution, and in fruit or flower or other particular greatly surpass the wild individuals of the species, *e.g.* peach, apple, pear, rose, and therefore furnish apparent exceptions to the law that rapid evolution is soon checked by an increasing tendency towards retrogression. In reality they afford the strongest proof of it. I think I am right in saying, that in every instance such plants have been propagated principally by cuttings and not by seed; *i. e.* they are not descendants in any true sense of their immediate predecessors, but detached portions of them. Their evolution appears to have been effected as follows. A plant that varied favourably has been chosen, and a vast number of plants have been reared from slips cut from it, the process being repeated through any number of pseudo-generations. In this way a single plant with a favourable variation is multiplied into many similar plants, and the variation is preserved for an indefinite time. Seeds reared from such plants generally develop into inferior plants, but exceptionally a superior plant develops, and is then propagated as before by means of slips. The parent variety is then ignored for breeding purposes and is used only for stock on which to engraft the newer variety. The more improved variety thus takes the place of the variety which is less improved. This process, the repetition of which has resulted in such extreme examples of rapid evolution as the peach and garden rose, is therefore a tremendously stringent process of selection. Practically speaking the most favourable individual of a species has been chosen and multiplied by means of slips, the rest of the species being eliminated; and in each new seminal generation the same process has been followed. It is as though, in an endeavour to increase the height of men, we chose the tallest man in the world, eliminated the rest of his species, multiplied him—not by seminal generation, but by some process analogous to the multiplication of plants by slips—into thousands of men and women as tall as himself, chose the tallest individual among the children of these, and repeated the process, and continued to do so through several seminal generations. The process of selection under which the cultivated rose and the peach have been evolved has therefore been, as I say, tremendously severe—a thousand times more severe than it is possible to make it among annual plants and among the higher animals; and their evolution has therefore been extremely rapid—even more rapid than would at first sight appear, for it is probable that but a few seminal generations intervene between the most divergent cultivated plants and their wild progenitors. But now suppose we chose any one of these highly divergent varieties, and without using any selection, bred from seed alone, what would happen? There is ample evidence leading us to believe that in the vast majority of instances the variety would swiftly (*i. e.* in a very few generations) revert to something very like the wild stock from which they originally descended;—but not to the wild stock precisely, for no doubt while the cultivated species was undergoing evolution in one direction, it was under the changed conditions undergoing retrogression in other particulars, and in these the reverted variety would differ from the original stock.

The truth therefore appears to be, that while there is a limit within "assigned" time to evolution, there is practically none to retrogression.

The above considerations may afford an explanation of another set of facts, viz. that cultivated plants and domesticated animals are much more variable than the wild varieties of the same species. In a state of nature plants and animals exist under conditions which, normally, are uniform during long ages; owing to which, and to the circumstance that in a state of nature many traits are essential to survival, evolution is slow, and therefore the traits of wild animals and plants acquire a certain fixity, because if any recent ancestor be reverted to in any particular, the change is not great; moreover, any retrogression must generally cause elimination; therefore since evolution is slow, since any reversion can seldom be great, and since reversion tends to cause elimination, there can seldom be any great or observable change of form. With cultivated plants and domesticated animals fewer traits are essential to survival, and therefore evolution is more rapid as regards them. Many traits essential in a wild state for survival, in a cultivated or domesticated state are not essential, and therefore as regards them there is retrogression. In a few generations "sports" appear; but these sports, if I am right, must generally be examples of retrogression, not of evolution (retrogression in traits which were essential to the wild ancestors, but are no longer essential to the cultivated or domesticated descendants, or far-reaching retrogression in traits rapidly acquired under artificial selection); must generally be due to reversion to the ancestral form, not to an advance beyond it.

Again, atavism is not the only cause of retrogression. Evolution may be a cause of apparent retrogression, as in the case of certain insects, which, living as they do in storm-swept islands, are exposed when flying to the danger of being carried to sea. In them natural selection, reversed selection as it is called in such cases, has co-operated with atavism to deprive them of the power of flight. Therefore, if this theory of retrogression be correct, it may afford us a not unimportant insight into the past life-history of species, and enable us to decide what retrogressive changes are due to atavism and what to natural selection. For instance, man as we know has descended from a hairy ancestor: if his present partially hairless condition is due to atavism, then since he has reverted in this respect to a remote hairless ancestry, his embryo will not be hairy, nor will he in cases of atavism be hairy. We know that this is not the case; therefore his present hairlessness is due mainly to natural selection (reversed selection), possibly to that form of natural selection known as sexual selection.

This theory has not, so far as I am aware, been propounded before, and it is opposed to another theory (presently to be noticed) which has the support of many eminent men of science. The mere fact, however, that organisms vary more frequently in the direction of their ancestry than in any other direction, appears to me to raise a strong presumption in its favour. But whether it be true or not, this at least is known to be true ; if any characteristic become of little or no importance in the struggle for existence it is certain to undergo retrogression, *i. e.* cessation of natural selection is invariably followed by retrogression.[1]

[1] Any interest this chapter may possess will be greatly increased if in connection with it Mr. Darwin's book, *Animals and Plants under Domestication*, be read. In that great mine of information many facts bearing on the subject under discussion are to be found.

CHAPTER III

It has already been explained that the lowest organisms are probably quite beyond our ken. Higher organisms than these appear as minute and apparently formless specks, the protogenes of Haeckel, visible only under the highest powers of the microscope, and composed of that transparent jelly, "the formal basis of all life," which is known as protoplasm. About them also we have as yet been able to learn little beyond the fact that they are living beings. Higher in the scale are such organisms as the *amœba;* about them we are able to learn much that is important and instructive. They occupy that point in the scale of life at which the plant and animal kingdoms begin to diverge the one from the other, and though excessively minute, are larger than the protogenes, and therefore better observable. Each is a little mass of protoplasm in which may be seen a dot, the *nucleus*, which is usually situated eccentrically, and which, as modern research seems to have established, is the most important part of the organism. Such a speck of living protoplasm as the amœba is known to biologists as a cell; and of such cells or variations of them the structures of all plants and animals are built up, a plant or animal composed of a single cell being known as a unicellular organism, whereas a plant or animal composed of a plurality of associated cells is known as a multicellular organism. Since such unicellular organisms as the amœba are fairly observable, we may conveniently begin the study of life at that stage of evolution which they have reached. Watched under the microscope the amœba is known by its actions to be a living being. It puts forth or withdraws thinner or thicker processes known as pseudo-podia. It moves by flowing forward, or by putting forth pseudo-podia and flowing into them. Streaming motions of granules may be observed in its substance. It engulfs food particles, and having assimilated the digestible portions, flows away, leaving the indigestible remainder behind. It shrinks from harmful contact.

In describing the amœba I have described also the white blood corpuscle, the leucocyte of man and other animals. These occur in great numbers, maintaining a separate existence in the blood, or wandering through the tissues. It might be thought that they are parasites, but this is disproved by the fact that if any tissue is injured, as for instance by a cut, they crowd in countless numbers to the spot, and repair the injury with their bodies, which thereupon undergo changes in shape and structure. What is known as pus or "matter," such as flows from an abscess, is a clear fluid rendered turbid by the multitude of their dead. In cases of zymotic disease they have been seen with the invading microbes enclosed in their substance, when either the leucocyte or the microbe perishes.

Both the amœba and the leucocyte multiply by fission, by dividing the one into two. First the nucleus divides by a complicated process known as Karyokinesis, the completion of which is followed by the division of the cell body. The daughter-cells grow, and in time divide in the same manner as the parent. This process is repeated through an immense though finite number of generations by the leucocyte, and through an infinite number of generations by the amœba. Whence it is clear that the amœba is *potentially immortal.* It may be killed by external circumstances, by starvation, by heat, by cold, by violence, &c., but if the conditions are favourable no death occurs, for, as Weismann pertinently remarks, "If it is mortal, if it dies—What is it that dies? where is the dead body?" The leucocyte, however, is not potentially immortal, for in time it or its descendants die.

If we watch low unicellular organisms of almost any species we see that occasionally two of them come together and fuse more or less completely, so that the two animals become one, or so that an exchange of substance takes place between their nuclei; subsequently the dual animal divides and re-divides many times before fusion again occurs. But however many the number of cell-divisions subsequent to fusion, recent investigations seem to show that the descendants of the conjugated pair ultimately perish, unless fusion again occurs.

"The riddle was in part solved by a long series of careful observations. In November 1885, Mons. Maupas isolated an infusorian 'Stylonichia Pustulata,' and observed its generations till March 1886. By that time there had been two hundred and fifteen generations produced by ordinary division, and since these lowly organisms do not conjugate with near relatives, there had of course been no sexual union.

"What was the result? At the date referred to the family was observed to have exhausted itself. The members were being born old and debilitated. The asexual division came to a standstill and the powers of nutrition were lost.

"Meanwhile before the generations had exhausted themselves several of the individuals had been restored to their natural condition, where they conjugated with unrelated forms of the species. One of these was isolated and watched for five months and the usual number of successive generations occurred. On to the one hundred and thirtieth generation members were removed at different stages, and were observed to conjugate successfully with unrelated forms.

"When the family began to draw near its end even removal to fresh conditions was without effect. About the one hundred and eightieth generation, the strange sight was seen of individuals of the same family attempting to unite with one another. The results were however nil, and the conjugates did not even recover from the effects of their forlorn hope.

"Without the normal sexual union then, the family becomes senile. Powers of nutrition, division, and conjugation with unrelated forms comes to a standstill. This senile degeneration is very interesting. The first symptom is decrease in size, which may go on till the individuals may not measure over a quarter of their normal proportions. Various internal structures then degenerate until at last we see formless abortions incapable of living and reproducing themselves."—Thompson, *Elements of Zoology*.

Among these low forms conjugation is evidently the same thing as sexual union among the higher animals, to which, and to the vertebrates in particular, popular observation is almost exclusively directed; and as sexual union among the vertebrates invariably precedes the birth of a new individual, it is naturally supposed that it is the cause of the genesis of a new individual; but this is certainly not so, for various invertebrate species are able to multiply asexually. Some organisms comparatively high in the scale are able to reproduce imperfectly without sexual union; for instance, among bees the offspring of asexual reproduction are drones only, not workers and queens, who are essential to the continuance of the race. Others again, such as aphides, are able to multiply asexually perfectly for many generations before sexual reproduction occurs; and lastly, Weismann has never been able to discover sexual union among a species of crustaceans which he has observed for a number of years. Sexual union therefore, when it does occur, appears to be an essential condition, not the cause of reproduction, much as a sufficient supply of nutriment or of heat is an essential condition, not a cause. Various hypotheses have been put forth as to the *raison d'être* of it. By some it is supposed that its object is the maintenance of the specific average—an absurd hypothesis, for the law governing organic beings is that of the "survival of the fittest," not that of the maintenance of the specific average. Another hypothesis is that sexual union causes a rejuvenescence and revitalization, so that by virtue of sexual union Maupas's infusorians when they conjugated grew young and vigorous and were able to continue the race, but when they were unable to conjugate grew old and feeble and perished, and with them the race. But as Weismann remarks, it is difficult to understand how one aged and debilitated individual can be rejuvenated and revitalized by union with another individual equally aged and debilitated. Another *raison d'être* must, I think, be sought for sexual conjugation. Weismann's hypothesis appears to be the most probable; he thinks that sexual union is very prevalent because by it is produced an increased amount of variability in the offspring.

On consideration it is apparent that of two individuals the one that produces offspring that vary within certain limits more from itself is the better placed as regards the ultimate survival of its descendants, other things equal, than the other individual which produces offspring that vary less, assuming of course, what must on the average happen, namely, that some of the offspring vary favourably as compared to the parent and some unfavourably. Though some of the descendants of the first, because they have varied extremely unfavourably, must perish and leave no offspring, yet some of his descendants, because they have varied extremely favourably, must, when there is a struggle for existence, other things equal, survive in greater numbers, and cause the ultimate elimination of the offspring of the second. Now, of two individuals, one of which produces offspring without collaboration with another individual, and the other in collaboration with another and somewhat dissimilar individual, the offspring of the latter must tend to vary more than the offspring of the former, and therefore in the struggle for existence to bring about their ultimate extinction, whereby only the descendants of the individual that reproduced sexually, and which inherited this peculiarity, would be left to continue the race. Weismann's hypothesis therefore appears to be reasonable, and it is moreover supported by other considerations, for if we assume, as we must, that non-living chemical compounds, under conditions we are ignorant of, did in the beginning of life pass over the border space and become living beings, it is difficult to imagine how they could possibly have multiplied sexually from the very first—how sexual union can have been the rule in the beginning. It seems more reasonable to suppose that these earliest forms multiplied asexually by fission, and that conjugation only occurred later, the purpose of it being to produce a greater amount of variability among the offspring. Possibly conjugation had its origin in attempts at cannibalism, which, among such excessively simple types as the first must have been, perhaps resulted in coalescence instead of assimilation. The extermination of those that multiplied asexually appears therefore to be the cause of the almost universal prevalence of sexual reproduction among low as well as among high organisms. If it be asked, Why, if sexual reproduction produces variability, and variability is of such importance, do low organisms only conjugate at rare intervals? the answer may be given in the following extract—

> "It has often been alleged that the subsequent dividing is accelerated by conjugation, but Maupas finds that this is by no means the case. The reverse in fact is true. While a pair of infusorians were engaged in conjugation, a single individual had, by ordinary asexual division, given rise to a family of from forty to fifty thousand individuals. Moreover, the intense internal change preparatory to fertilization, and the general inertia during subsequent reconstruction, not only involved loss of time, but exposed the infusorians to great risk. Conjugation seems to involve danger and death rather than to conduce to multiplication and birth."—Thompson, *Elements of Zoology.*

The above explains also why sexual reproduction does not occur in all instances, *e. g.* when the specific persistence is secured by extreme rapidity of multiplication rather than by close adaptation to the environment.

In speculating on the origin of species we may conceive it possible, or rather certain, that among the innumerable variations which occurred among the vast multitudes of low unicellular organisms, such a variation occasionally occurred as the following: that when one cell divided into two the resulting cells did not separate, as normally happened, but remained adherent; and further, that this variation, whether for purposes of food-getting, locomotion, protection, &c., proved a fortunate one. This variation, which, like other variations, would tend to be transmitted, and which, if fortunate, would tend to cause the ultimate survival of those organisms that possessed it, would be the first step in the evolution of the multicellular from the unicellular organism. The dual animal which resulted would reproduce by each of its cells dividing into two, so that there would be four single cells which would separate, so as again to form unicellular organisms. But each unicellular organism would in general inherit the peculiarities, and repeat the life-history of its grandparent-cell, by dividing into two adherent cells. A race of two-celled organisms would thus be established. We may fairly believe that in time a second variation, which also proved fortunate, occurred, whereby the four grand-daughter cells remained adherent until reproduction; and afterwards other variations of the same nature, till an organism was at length evolved which consisted of a multitude of cells adherent together for the common benefit—the morula. The component cells would all be alike, each would perform all the functions of life, assimilation, locomotion, &c.; and when reproduction (*i. e.* reproduction of the whole organism) took place the whole would break up into single cells, every one of which, by repeating one by one the variations of its ancestors, would build up, step by step, a multicellular organism similar to that from which its parent cell was derived; the steps of the *ontogeny* or development of the individual thus following those of the *phylogeny*, or evolution of the race.

The cells of such a simple multicellular organism, though similar in kind, would yet necessarily differ as regards position, some being internal and some external, and therefore it would obviously be an advantage (1) if the organism assumed such a shape as would enable all its constituent cells to perform their common functions to the greatest advantage; or (2) if among the cells a division of labour took place by virtue of which the internal and external cells performed functions different in kind; or (3) if the two variations were combined so that changes in the shape of the organism were accompanied by differentiations in the functions of the cells, some cells taking on one function, *e. g.* food-getting or locomotion, some another, *e. g.* digestion or reproduction, accordingly as they were best placed to perform it.

Now, since it cannot be doubted that low organisms vary as well as high organisms, we may legitimately suppose that some such variations as the above did occur, indeed we have abundant evidence that they must have occurred, and that they were so seized upon and accumulated by the action of natural selection (*i. e.* by the survival of the fittest), that such differentiations were thereby brought about in the forms of multicellular organisms (*i. e.* in masses of cells adherent for the common benefit), and such differentiations in structure and specializations in function in their component cells as resulted, after long ages and innumerable generations, in all the varied and wonderful forms of plant and animal life; and in the equally wonderful and varied differentiations in structures and specializations in functions of the cells composing those plants and animals; in such lordly forms as the Wellingtonia Gigantia and the elephant, as well as in such lowly forms as the lichen and the hydra; in such highly differentiated cells as nerve muscle or gland cells, as well as in the white blood corpuscle, which may be likened to an amœba, or the bone cell, which secretes round itself a calcareous envelope like a rhizopod.

The single cell of the amœba performs of necessity all the functions of life; but even in such low organisms as sponges a great amount of cell-specialization is already observable. In them cells which are differently situated as regards the environment differ somewhat in structure and function. All the cells are to some extent capable of performing all the functions of life, but some cells perform some one function better and other functions less well than other cells differently situated, which in turn display a like peculiarity. Thus as regards the function of reproduction some cells subserve it more than others; yet many of the cells of all portions of the whole organism are capable of performing it, for almost any fragment of a sponge, if separated from the parent mass and bedded out, is capable of growing into an "individual" and of continuing the race. If we pass in succession to higher species, we find cell-specialization becoming more and more marked; we find cells becoming more and more capable of performing efficiently some one function and less and less capable of performing all other functions. A division of labour thus occurs in the cell-community similar to that which distinguishes communities of civilized men from savage communities. Every savage does everything for himself, receiving small help from the rest of the community and giving little in return; and the more utterly savage the community is, the more complete in general is the independence of the individual. But while the savage performs all the labours necessary for supporting life, he performs each one less efficiently than he would did he devote himself entirely to it. In civilized communities each man devotes himself to some special work which he performs specially well, but which would not enable him to live unless it were supplemented by the labours of other men, who in like manner perform other special duties equally well. Thus a bricklayer performs a special duty better than he could do were he also a carpenter, a farmer, and a miner; but except he receive help from the labours of others his work does not produce all that he requires to maintain life. It is not, for instance, directly productive of food and clothing. For these he depends on the labours of other specialists; and therefore the higher the state of civilization the more complete is the state of interdependence of the members of the community.

So in higher animal organisms we find that all cells have taken up special duties—have all become specialists.

Every kind of cell performs one function specially well and the others not so well, or not at all. The result of this cell-specialization and differentiation is, like the results of increased specialization and differentiation in the labours of civilized men, increased efficiency of the organism as a whole, and the result of the increased efficiency is, of course, an increased average rate of survival.

CHAPTER IV

ONE function which, in high animal organisms, is performed exclusively by one class of cells alone, the germ cells, is that of reproduction, that is, reproduction of the whole organism.[1] All the other cells are capable of reproducing their like by that process of fission which, as we have seen, is very prevalent among unicellular organisms. Thus muscle, nerve, or skin cells can severally produce their like, but no muscle, nerve, or skin cell can produce a cell of an unlike kind; still less can it produce a whole organism, in which there are many different kinds of cells arranged in definite relations to one another. Unlike the case in lower animals, such as sponges, then, this power among higher animals, such as vertebrates, is limited to the germ cells alone, each of which presents in addition the remarkable peculiarities unknown among any other of the cells, (1) of conjugating with another cell, and (2) of being incapable of reproducing other cells, like or unlike, except it first conjugates with another germ cell, and that germ cell not a near relation, not a descendant of the pair of germ cells from which the organism of which it forms a part

[1] "This is very generally the case, but it is not universal. 'Self' fertilization—that is, union of the eggs and sperms of the same organism—has been proved to occur in several trematodes, and to be almost universal in cestodes. This may be one of the conditions of the degeneracy of these parasites, for, frequent as hermaphroditism is among plants and animals, self-fertilization is extremely rare."—*Evolution of Sex*, p. 71.

is derived, *i. e.* not a germ cell from the same body, but only with a germ from another body.

Thus these remote cell-descendants of the original pair of cells which conjugated, and from which the body to which they belong is derived, exhibit, after many generations, the same peculiarity which Maupas found distinguished the remote cell-descendants of conjugated unicellular organisms; viz. that after a certain number of cell-generations, unless conjugation occurs anew, they perish, and with them the race. The other cells which do not conjugate cease in time, like Maupas' infusorians, to multiply, and, like those infusorians, perish, and with them the individual of whom they generally form the chief part.

The above seems to bear out the theory that conjugation causes a "rejuvenescence and revitalization" without which the race cannot persist; but on closer examination the facts are found to point the other way. The number of cell-generations following conjugation differs in different individuals of the same species, and may be made to differ in each individual by variations in nutrition, exercise, &c. It differs enormously in different species of plants and animals; thus the number of cell-generations following conjugation is enormously fewer in minute plants and insects than in such great plants and animals as pines and whales. Moreover the number of non-conjugating cell-generations is almost infinitely prolonged along certain lines in such animals as the aphides, which reproduce asexually during the whole summer, and reproduce sexually only on the advent of cold weather, and which, were the warm weather to continue, might reproduce asexually for ever. It is infinitely prolonged in such plants as multiply by means of suckers, or are propagated by cuttings, without their cells ever conjugating. The theory therefore that conjugation causes a "rejuvenescence and revitalization," without which death must occur, is utterly disproved, as is also the theory that the purpose of conjugation is to maintain the specific average. For here we have unlimited cell-multiplication without any rejuvenescence by means of conjugation, while the specific average is perfectly maintained by the similarity of the conditions to which the different individuals of the species are exposed. On the other hand, Weismann's contention, that conjugation is not necessarily essential to persistence, but is merely a condition which is usually, but not always, advantageous—so advantageous that in nearly all plants and animals it periodically occurs—is fully borne out.

Death, that is death from internal causes, from failure of the vital powers, not death from external causes, such as cold, hunger, accident, &c., occurs for quite another reason, which may be set forth as follows. In those plants which multiply by means of suckers or are multiplied by means of cuttings, the cells whence the individual is derived are to be regarded as little differentiated and specialized, like the cells of the sponge, cell-differentiation and specialization not having proceeded nearly so far among plants as among animals; for the highest plants are inferior in this respect to animals low in the scale. As a result, while plants high in the scale are able to reproduce otherwise than by means of germ cells, animals comparatively very low in the scale are not so able. They reproduce solely by means of germ cells, for such, owing to specialization in function, is the interdependence of their other cells, that these latter cannot exist apart from one another, any more than can a bricklayer, who does nothing else to support life, exist apart from other men. They must remain part of the organism or perish, while they cannot continue to multiply indefinitely, for then the organism would grow too large for its available supply of nutriment.

It (the organism) is during life subjected to all manner of external conditions, which tend to destroy it, or so to maim and enfeeble it that it is easily destroyed; for, unlike plants and low animal organisms, the interdependence of the parts as well as of the cells of a higher animal organism is so great, that no one part can be injured without all the other parts being injuriously affected. The ultimate death of all organisms with highly specialized cells is therefore inevitable. To put it another way: all cells or aggregate of cells, which are so specialized as to be incapable of independent existence, cannot continue the race, and must inevitably perish. In high animal organisms the germ cells alone are capable of existing independently, and therefore they alone survive in their descendants. It is true that in the highest animals the germ cells after conjugating are still retained within the body of, and supplied with the nutriment by, one of the parents till development is considerably advanced, but in the sense in which I write they as truly lead a separate existence as the child whom the mother suckles.

But though conjugation is not universally necessary to unending reproduction it is yet generally true; and the point I wish to emphasize is this—that in the highest animals, as well as in the lowly infusorians, unless conjugation occurs after a certain number of cell-generations, cell-multiplication ceases, and the race perishes.

The germ cell, like the unicellular organism, on conjugating, divides and redivides many times without conjugation ever occurring again among its descendant cells, the successive generations of which may be compared to successive non-conjugating generations of infusorians, so that the body of a multicellular organism in the successive stages of its ontogeny is comparable to later and later generations of infusorians; but, unlike the cell-descendants of conjugated infusorians, the cell-descendants of germ cells, (1) instead of separating remain adherent, and (2) the mass thus formed, as cell-proliferation proceeds, takes definite shapes, at first resembling those of very low multicellular organisms, then with more or less indistinctness those of higher organisms, and lastly that of the parent organisms, from which the pair of germ cells which conjugated, and from the union of which the other cells resulted, was derived; and (3) each successive generation of cells shows greater and greater degrees of differentiation and specialization, till such highly differentiated and specialized cells as skin, nerve, gland, blood, &c. as are present in the fully developed organism appear. In other words, the development of the individual is a short, rapid, blurred recapitulation of the evolution of the species; which, if there is any truth in the theory of evolution, is exactly what was to be expected, and which therefore affords convincing proof of its truth.

We have seen that evolution depends on three factors: (1) that offspring in general inherit (*i.e.* recapitulate) the traits of their parents; (2) that offspring vary somewhat from their parents; (3) that there is invariably a struggle for existence, during which natural selection causes evolution by preserving favourable and eliminating unfavourable variations. Now if the son recapitulates the traits of the parent, the parent the traits of the grandparent, the grandparent the traits of the great-grandparent, and so on, it is evident that the son, the last of the race, must recapitulate the traits of each ancestor up to the remotest, or at least up to the unicellular organism, which for convenience of language we may call the first ancestor. In other words, the last descendant recapitulates the traits of the first ancestor, plus the traits, in their order, of all subsequent ancestors, beginning with the traits of the first and ending with those of the last, and therefore in his development he presents a fleeting resemblance to each ancestor in turn.

It follows, therefore, that what is known as *atavism* is nothing other than a failure to recapitulate in the ontogeny the last stages of the phylogeny; *i. e.* it is an arrest of development, the individual halting at a stage reached by a remote ancestor, and developing no farther. A race may differ from its ancestry in three ways: (1) through evolution as a result of selection; (2) through evolution as a result of reversed selection; and (3) through retrogression occurring in the absence of selection. As regards traits evolved under the influence of selection, atavism is a simple arrest of development; that is, the ancestral form is approximated to, because the last stages of the phylogeny are omitted in the ontogeny. As regards traits evolved under the influence of reversed selection, atavism is also an arrest of development; the ancestral form reappears because there is an omission in the last stages of the ontogeny to retrace steps previously made, as was done in the phylogeny. As regards traits suppressed through retrogression, *i. e.* through a lapsing by the race of the last steps of the evolution, there can of course be no atavism; for instance, if the lost toes of the horse have disappeared through retrogression, *i. e.* through a return, in the absence of selection, to a very remote ancestral condition when they did not exist, then their reappearance in the modern horse would be an instance of evolution, not of retrogression, since it would be a return from a more ancient to a much more modern condition; on the other hand, if the toes have disappeared as a result of reversed selection, the embryo should exhibit them, and they should be present in cases of atavism.

It is now easy to understand why all races tend to retrogress unless that tendency is checked by selection; for, as regards any trait, an individual may vary from his parent primarily in two ways. He may vary from him either towards the ancestry or away from it, *i. e.* he may either undergo retrogression or evolution; and, so far as we know, the chances are equal of his doing either the one or the other. But if he vary away from the ancestral type, it does not necessarily follow that the variation will constitute an extension of the previous evolution. It may constitute a reversal of it, or a divergence in an altogether new direction, and, therefore, in the absence of selection, the variation of the offspring from the parent must tend on the whole to bring about retrogression—a tendency which is checked and reversed in an evolving species only by a sufficiently severe process of selection.

Though each multicellular organism has its starting-point in a unicellular organism (the germ cell), yet nevertheless each germ cell, counting from the time of the first evolution of the multicellular organism, when all the cells were more or less germ cells, through succeeding generations, during which ever-increasing differentiation in structure and specialization in function took place, must have become in one sense a more and more complex entity, differing more and more from its ancestor, the unicellular organism, in that it was the starting-point of a more and more complex and heterogeneous multicellular organism. In other words, each successive conjugating germ cell differed more and more from its ancestor, the conjugating unicellular organism, not only in that its non-conjugating descendants, however remote, remained adherent in one mass, but also in two other particulars.

First, in that definite lines of its cell-descendants multiplied at increasingly different rates, whereby were produced differentiations in the shape of the whole mass; for instance, the mass of cells constituting a man owes its particular shape, the human shape, to certain definite inequalities which occur in the rates of multiplication in the lines of the cell-descendants of the germ whence the man is derived. Were there no inequalities in the rates of multiplication, did each line of cell-descendants multiply at the same rate, a solid, spherical mass of cells must result, whereas, owing to their unequal but definite rates of multiplication, the shape of the mass is irregular (*i. e.* not spherical), but nevertheless definite for man and every species of animal. An ox, therefore, differs in shape from a man mainly because the inequalities in the rates of multiplication in the various lines of the cell-descendants of the germ whence it is derived are different from the inequalities in the rates of multiplication in the lines of cells that spring from a human germ; but it resembles other oxen in shape, because the inequalities in the rates of cell-multiplication are much the same. Sometimes, however, though rarely, this or that cell-descendant of the germ reverts to the remote unicellular ancestral type, in so far that it does not multiply at a rate bearing a definite proportion to the rates of multiplication of its other co-descendants of the germ, but at a rate that has no definite proportion to them, and is only proportionate to its supply of nutriment and powers of assimilation. There then results the "morbid" condition which is known as a "tumour," in which the cell-descendants of a cell which has so reverted to the remote ancestral type form a more or less spherical mass which neither bears a definite proportion to the whole mass of the cell-community, nor performs definite functions beneficial to it, and is therefore an encumbrance or worse. But this tendency of cells to revert to the ancestral unicellular type, to multiply at a rate that is only proportionate to the supply of nutriment and the powers of assimilation, is checked by natural selection, which, by placing at a disadvantage, brings about the ultimate elimination of multicellular organisms, in which the tendency is displayed.

On the other hand, this or that cell-descendant of the germ may vary from the normal in a direction which is not towards the ancestral type, but in a different direction, and its descendant cells then form a mass or group which also differs more or less from the normal. The bud variations which have been observed on several cultivated plants, by taking advantage of which gardeners have sometimes been able to establish new varieties, probably result from this cause.

Secondly, each successive germ cell must differ from the ancestral conjugating unicellular organism more and more, in that different lines of its cell-descendants differentiate more and more from one another in structure and function, whence arise in high animals differentiated and specialized tissues such as muscle, bone, skin, gland, nerve, &c.

Moreover, just as each germ cell during the phylogeny differed more and more from its prototype, the unicellular organism, in that it was the starting-point of a more and more complex organism (mass of cell-descendants), so each embryo during the whole process of its ontogeny differs from its prototypes in the phylogeny, in that it carries within it the potentiality of developing beyond those prototypes. We cannot, however, discern in the macroscopic or microscopic appearances of germ or embryo any peculiarities of structure which imply this potentiality. They lie beyond the ken in the minute structure of the cells, probably in those portions of them which are known as the nuclei.

It may be objected, (1) that it is impossible that during the short period of the development of the individual all the vast numbers of variations which occurred among the millions of ancestors from the unicellular organism downwards can be recapitulated; and (2) as regards the highest animals, that it is obvious that the embryo in its various stages cannot present exact copies of its remote ancestors, for the simple reason, that in its various stages it is incapable of living apart from the parent. For instance, no animal that even approximately resembled the human embryo of two months could be capable of living outside the body of its mother. But these objections are met when we take into consideration what is undoubtedly true, viz. that variations occur, and that natural selection (reversed selection in this case), and particularly cessation of natural selection, act, not only at the end of the ontogeny, but during the entire period of development; natural selection during that period seizing upon as favourable and accumulating all such variations as tend to shorten and simplify the process of development, and cessation of natural selection tending to bring about the disappearance of all characters which during the phylogeny had been useful, but which to the embryo, living under different conditions, protected and sustained as it is within the body of its parent in the highest animals, or advantageously placed as regards the environment by the parent in the case of lower animals, are no longer useful.

One other point remains to be cleared up. It has been observed that the embryo of a high animal, *e. g.* man, resembles the embryos of lower animals, never the lower animals themselves; for instance, the embryo of a man in one stage of its ontogeny resembles the embryo of a frog, never the adult frog. The reason for this is obvious; man has not descended from the frog, but the frog and man have descended from a common ancestry, and therefore both animals as they recapitulate in their ontogenies the evolution of their respective races, develop along parallel lines up to the point at which the races diverged, after which they develop along lines which are no longer parallel. It follows that the nearer the relationship between any two species, the longer does the course of the development from germ to adult proceed along parallel lines. Mr. Herbert Spencer puts the matter very clearly.

"The germ out of which a human being is evolved differs in no visible respect from the germ out of which every animal and plant is evolved. The first conspicuous structural change undergone by the human germ, is one characterizing the germs of animals only—differentiates them from the germs of plants. The next distinction established is a distinction exhibited by all *Vertebrata;* but never exhibited by *Annulosa, Mollusca,* or *Coelenterata.* Instead of continuing to resemble, as it now does, the rudiments of all fishes, reptiles, birds, and mammals; the rudiment of man assumes a structure that is seen only in the rudiments of mammals. Later, the embryo undergoes changes which exclude it from the group of implacental mammals; and prove that it belongs to the group of placental mammals. Later still, it grows unlike the embryos of those placental mammals distinguished as ungulate or hoofed; and continues to resemble the unguiculate or clawed. By and by, it ceases to be like any fœtuses but those of the quadrumana; and eventually the fœtuses of only the higher quadrumana are simulated. Lastly, at birth, the infant, belonging to whichever human race it may do, is structurally very much like the infant of all human races; and only afterwards acquires those various minor peculiarities of form that distinguish the variety of man to which it belongs."—*Principles of Biology,* vol. i. p. 142.

Following the above train of thought it may be remarked, that any trait which appears late in the development of the individual, as for instance the horns of deer and the beards of men, must have appeared late in the evolution of the species. The young hornless deer and the young beardless man should therefore correspond to very recent stages in the evolution of their respective species. Whence if there is any truth in my theory of retrogression by atavism, it follows that comparatively hornless deer and comparatively beardless men ought to be comparatively frequent, for the ancestor reverted to would in either case be comparatively recent. As regards the beard, we know that comparatively beardless men are common in a well-bearded race such as the Anglo-Saxon. As regards the horns of deer I cannot speak with authority, not having sufficient knowledge, but I doubt not that others who have had better opportunities for observation will confirm my conclusion in this matter. I think it will be found, for example, that comparatively hornless deer are much more common than comparatively hairless animals of the species.

THE FACTORS OF EVOLUTION

CHAPTER I

All the world, or at any rate all that portion of the world which has a sufficient knowledge of the facts, is practically agreed that the plant and animal kingdoms have arisen by evolution; but all the world is not agreed as to what have been the factors in that evolution. It is not now disputed that natural selection, by perpetuating and accumulating inborn favourable variations, has been a cause of evolution, but it is contended by many biologists, some of whom are of the greatest eminence, that the accumulation of inborn variations has not been the sole nor even the principal cause of evolution; but that the accumulation of acquired variations has been an additional and even the principal cause. To take an example; hares run swiftly, their rapid pace being due to structural evolution from a slower ancestry. By the one school of biologists it is held that the great speed of hares is due to their having lived under conditions which permitted only the naturally swift to survive, so that while those animals that were naturally slow perished, the others continued the race; the other school, while admitting that the evolution of speed in hares may have been partly so caused, contend that it has been partly or mainly brought about by the circumstance that the animals have lived under such conditions as constantly caused them to run as swiftly as possible; which endeavours at swift running caused in the individual hares a development, as in athletes, of the structures that subserve swift running, and that this acquired development, transmitted to the offspring, and accumulated in the course of generations, has been a part or a main cause of the evolution of speed in hares. To take another example; the muscles of a blacksmith's arm are enlarged by exercise. It is contended by the one school that this enlargement, being an acquired not an inborn variation, is not at all transmitted, by the other that it is in part transmitted. To take a third example; suppose a man to be enfeebled by disease. It is contended by the one school that this acquired enfeeblement cannot be transmitted; by the other that it can and is transmitted.

The theory that evolution has resulted from the accumulation of inborn variations alone is usually, but incorrectly, called *the* theory of evolution by Natural Selection. I say incorrectly, for if acquired variations are transmissible, they must equally with inborn variations be seized upon and accumulated by Natural Selection; and therefore a theory that supposes that the organic world has arisen by the accumulation of acquired variations is as much a theory of evolution by Natural Selection, as a theory that supposes that it has arisen by the accumulation of inborn variations alone; particularly since variations acquired in one generation become, if transmitted, inborn in the next generation.

The theory that acquired traits are transmissible is older than the theory which denies their transmissibility. Lamark, finding evidence of evolution in the organic world, published in the beginning of this century a work in which he attributed all evolution to the accumulation of acquired traits. In 1858 Darwin and

Wallace published their theory of evolution by the accumulation of inborn variations, but by Darwin himself his own theory was regarded as merely supplementary to the other, since he believed in the transmissibility of acquired variations. Lately Weismann and others have altogether denied that acquired traits are transmissible, and biologists in general appear to be gradually veering round to this opinion. It is, I think, now admitted on all hands that even if acquired variations are transmitted, they are very much less important as factors in evolution than was formerly supposed.

On the face of it the theory that acquired variations are transmissible seems very reasonable and probable; so much so, that before it is understood that such transmission must lead to evolution, it is in general very readily accepted by people, who on teleological grounds are opposed to the idea of evolution. For instance, it appears only reasonable to suppose, other things equal, that the offspring of a man who has developed his muscles by exercise will have larger muscles than the offspring of a man who has not so developed his muscles; or to suppose that the offspring a man has after his lungs are weakened by disease will be weaker in the lungs than the offspring who were born to him before he fell ill. But on closer inspection the theory presents difficulties which I think insuperable.

We have seen how the germ cell, after being fertilized, divides and redivides many times without conjugation ever occurring among its descendant cells; which cells, though they remain adherent and undergo differentiation in structure and specialization in function, are to be regarded, as the development of the organism proceeds, as homologous to successive generations of the descendants of two conjugated infusorians. No one cell in the body, therefore, is the product of any other co-existing cell or cells. The body cells multiply, as the infusorians do, by dividing the one into two or more; never do two or more cells unite to form another cell, or by giving off living portions of their substance unite, themselves maintaining their individual existences, to form in part or whole another cell—a germ cell, for instance. The germ cells are therefore not produced by other co-existing cells, but are co-descendants with the other cells from a common ancestry. Now if among mammals it is true that the cell-descendants of a pair of conjuated germ cells do not conjugate among themselves, nor emit portions of themselves which unite to form other cells, and it certainly is true if we are to judge by what happens among unicellular animals, among whom the behaviour of cells in this respect can best be observed, it is impossible to understand how acquired variations can be transmissible.

To take an example already given: the muscles in the blacksmith's arm are enlarged by exercise, *i. e.* the muscle cells in the arm, when stimulated by exercise, undergo proliferation and are so multiplied. Now if the blacksmith's child is to inherit his acquired variation, his germ cells, situated far distant from the muscle cells of his arm, and related to them only through a remote cell ancestry, must be so influenced, so structurally altered, as to produce spontaneously in some of their very remote cell-descendants (*i. e.* in the muscle cells of the child's arm) that modification in point of numbers which exercise temporarily produced in the muscle cells of the blacksmith's arm. Is this possible?

I have heard it contended that the muscle cells telegraph by means of the nerves, or in some other way communicate what practically amounts to an order to the germ cells to structurally modify themselves in the desired way, but of course such a theory is too absurd to be met with anything but derision. It is, scientifically speaking, so utterly ridiculous as to be unworthy of discussion. Rejecting this and other similar theories, I think that those who maintain the transmissibility of acquired variations are practically reduced to the choice of one of the three following hypotheses. The first of these supposes that an acquired variation produces, through the blood, such changes in the nutrition of the germ cell as bring about after fertilization a similar variation in the organism of which its remote cell-descendants form the component parts. To take the same example as before, it is supposed by this theory that the blacksmith, by exercising the muscles of his arm, produces such a change in the nutritive fluids which bathe his germ cells as causes them, after conjugation with germ cells from another individual, to proliferate in such a manner as to produce new organisms with muscles similarly enlarged. To my mind this is as wild a theory as that which I have already dismissed. Even were it possible, and I think it is impossible, that such transient changes as are produced in the nutritive fluids by exercising the arm muscles could so permanently and profoundly affect the constitution of the germ cell as to cause it to develop, after long separation from the parent organism, into an individual structurally different from the individual which would otherwise have resulted, even, I say, were this possible, how is it possible that these nutritive changes can cause in the germ cell such *particular* structural alterations as result, after its proliferation, in an individual who has a variation similar to the particular variation in the parent organism, a variation which resulted from exercising the arm muscles? To put it in fewer words: how can the change which any acquired variation produces in the nutritive fluids that bathe the germ cell affect that cell in such a manner as to cause it, after fertilization and separation from the parent organism, when it is no longer directly affected by those changes, to proliferate into an individual who has the same kind of variation as the acquired variation in the parent? Why should not the result be enlarged leg muscles or an enlarged brain or liver? Is it conceivable, if a man takes exercise in such a manner as to enlarge the muscles of his legs, that the nutritive fluids will thereby be so differently changed from what they would be if he took such exercise as would enlarge his arm muscles, as will confer on the germ such a constitution as will cause it to proliferate into an organism with enlarged leg muscles rather than enlarged arm muscles? I think we may dismiss this hypothesis also as unbelievable.

The second of these theories is weighted with the authority of a great and honoured name. Darwin, in striving to account for the fact that complex and heterogeneous multicellular organisms are able to reproduce their like by means of germ cells, supposed that each cell of the multicellular organism sends off portions, which he called gemmules, of itself, which gemmules, entering into the substance of the germ cell, so affect its constitution as to cause it, when fertilized, to proliferate into an organism like to that from which it was derived. Since Darwin held that acquired variations are transmissible, this, his theory of "Pangenesis," must include the corollary that when any part of the organism, for instance the arm muscles, varies in such a manner as to increase the number of its cells, the additional cells send off additional gemmules to each of the germ cells, whereas if it varies in such a manner as to decrease the number of its cells, a number of gemmules corresponding to the decrease are in some way withdrawn from each of the germ cells.

Now the germ cells of many animals, as for instance of a man, are numbered by millions, the other cells, the somatic cells, by billions, and during every moment of time some of the somatic cells are perishing and others are proliferating. Therefore to discover the number of gemmules sent off by the somatic cells of an organism to its germ cells, we must multiply the millions of the germ cells by the billions of the somatic cells, and then multiply the product by the moments of time, multiplied by the number of the separate acts of cell-proliferation and death occurring during each moment of time. This consideration renders the theory wholly incredible, and it is rendered almost unthinkable if we take into consideration also that each germ cell is a mere speck as compared to the rest of the body; that the blood, a fluid mass of considerable volume, must accurately bring from every part of the body to each of these specks its appropriate billions of gemmules, and that, when arrived, each gemmule must take up its proper position in the germ cell so as to cause it, when fertilized, to develop into an organism similar to that of which it (the germ cell) forms an infinitesimal part. It is hard to believe that such a theory can ever have been seriously entertained by biologists of note, and that notwithstanding the fact that it is wholly inapplicable, as is the preceding theory, to plants, the nutrient fluids of which flow generally in one direction and do not *circulate* in the sense that the nutrient fluids of animals circulate.

The third theory is one formulated by Mr. Herbert Spencer. I give his own words as far as space permits.

"As we shall have frequent occasion hereafter to refer to units, which possess the property of arranging themselves into the special structures of the organisms to which they belong, it will be well here to ask what these units are, and by what name they may be most fitly called?

"On the one hand, it cannot be in those proximate chemical compounds composing organic bodies, that this specific polarity dwells. It cannot be that atoms of albumen, or fibrine, or gelatine, or the hypothetical protein substance, possess this power of aggregating into specific shapes, for in such cases there would be nothing to account for the unlikenesses of different organisms. Millions of species of plants and animals, more or less contrasted in their structures, are all mainly built up of these atoms. But if the polarities of these atoms determined the forms of the organisms they composed, the occurrence of such endlessly varied forms would be inexplicable. Hence, what we may call the *chemical units* are clearly not the possessors of this property.

"On the other hand, this property cannot reside in what may roughly be distinguished as the *morphological units*. The germ of every organism is a microscopic cell. It is by multiplication of cells that all the early developmental changes are affected. The various tissues which successively arise in the unfolding organism, are primarily cellular, and in many of them the formation of cells continues to be, throughout life, the process by which repair is carried on. But though cells are so generally the ultimate visible components of organisms, that they may, with some show of reason, be called the morphological units; yet, as they are not universal, we cannot say that this tendency to aggregate into specific forms, dwells in them. Finding that in many cases a fibrous tissue arises out of a structureless blastema without cell formation, and finding that there are creatures, such as the Rhizopods, which are not cellular, but nevertheless exhibit vital activities, and perpetuate in their progeny certain specific distinctions, we are forbidden to ascribe to cells this peculiar power of arrangement. Nor, indeed, were cells universal, would such an hypothesis be acceptable, since the formation of a cell is, to some extent, a manifestation of this peculiar power.

"If, then, this organic polarity can be possessed neither by the chemical units nor by the morphological units, we must conceive it as possessed by certain intermediate units, which we may term *physiological*. There seems no alternative but to suppose that the chemical units combine into units immensely more complex than themselves, complex as they are; and that in each organism, the physiological units produced by this further compounding of highly compound atoms, have a more or less distinctive character. We must conclude that in each case, some slight difference of composition in these units, leading to some slight difference in the mutual play of forces, produces a difference in the form which the aggregate of them assumes."—*Principles of Biology*, vol. i. pp. 182-3.

". . . the assumption to which we seem driven by the *ensemble* of the evidence is, that sperm cells and germ cells are essentially nothing more than vehicles, in which are contained small groups of the physiological units in a fit state for obeying their proclivity towards the structural arrangement of the species they belong to."—*Ibid.* pp. 254-5.

"If the assumption of a special arrangement of parts by an organism is due to the proclivity of its physiological units towards that arrangement, then the assumption of an arrangement of parts slightly different from that of the species, implies physiological units slightly unlike those of the species, and these slightly-unlike physiological units, communicated through the medium of sperm cell or germ cell, will tend, in the offspring, to build themselves into a structure similarly diverging from the average of the species."—*Ibid.* p. 245.

"The repair of a wasted tissue may therefore be considered as due to forces analogous to those by which a crystal reproduces its lost apex, when placed in a solution like that from which it was formed. In either case, a mass of units of a given kind shows a power of integrating with itself diffused units of the same kind; the only difference being that the organic mass of units arranges the different units into special compound forms, before integrating them with itself. . . .

"That other kind of repair which shows itself in the regeneration of lost members, is comprehensible only as an effect of actions like those referred to. The ability of an organism to recomplete itself when one of its parts has been cut off, is of the same order as the ability of an injured crystal to recomplete itself. In either case, the newly-assimilated matter is so deposited as to restore the original outline. And if, in the case of the crystal, we say that the whole aggregate exerts over its parts a force which constrains the newly-integrated atoms to take a certain definite form, we must in the case of the organism assume an analogous force." —*Ibid.* vol. i. pp. 178-9.

" . . . We must infer that a plant or animal of any species is made up of special units, in all of which there dwells the intrinsic aptitudes to aggregate into the form of that species; just as in the atoms of a salt there dwells the intrinsic aptitude to crystallize in a particular way."—*Ibid.* p. 181.

"Setting out with these physiological units, the existence of which various organic phenomena compel us to recognize, and the production of which the general law of Evolution thus leads us to anticipate; we get an insight into the phenomena of Genesis, Heredity, and Variation. If each organism is built of certain of these highly-plastic units peculiar to its species—units which slowly work towards an equilibrium of their complex polarities, in producing an aggregate of the specific structure, and which are at the same time slowly modifiable by the reaction of this aggregate—we see why the multiplication of organisms proceeds in the several ways, and with the various results, which naturalists have observed.

"Heredity, as shown not only in the repetition of the specific structure, but in the repetition of ancestral deviation from it, becomes a matter of course; and it falls into unison with the fact that, in various simple organisms, lost parts can be replaced, and that, in still simpler organisms, a fragment can develop into a whole."—*Ibid.* pp. 287-8.

In Mr. Spencer's view then, the multicellular organism is not a compound being, a being compounded of adherent unicellular organisms, but a simple being, of which the cells are parts, but not absolutely essential parts, since parts of the organism may arise independently of cells. His work was written some years ago, and he does not give his authorities. I have searched every recent work I could lay hands upon, and have found no warrant for his statement that fibrous tissue ever arises independently of cells. Dr. Klein says—

"Fibrous connective tissue is developed from embryonal connective tissue cells, *i.e.* from spindle-shaped or branched nucleated protoplasmic cells of the mesoblast. The former are met with isolated or in bundles, as in the umbilical cord or embryonal tendon. The latter form a network as in the fœtal skin and mucous membrane. In both instances the protoplasm of the embryonal connective tissue cells becomes gradually transformed into a bundle of elementary fibrils, with a granular-looking interstitial substance. The nucleus of the original cell finally disappears. A second mode of the formation of connective tissue is this: the embryonal connective tissue cell, while growing in substance, produces the fibrous tissue at the expense of its peripheral part. A remnant of the protoplasm persists round the nucleus.

"The same modes of formation of connective tissue may be also observed in the adult, under normal and pathological conditions."—*Elements of Histology*, p. 42.

As regards rhizopods again, I can find no warrant for the positive statement that they are not cellular. Some of them are apparently not cellular, but we have good reason for supposing that they really are so, though with the imperfect means of observation at our disposal we are unable to distinguish the cells.

In Claus's *Zoology*, translated and edited by Adam Sedgwick, I find—"Nuclei are also usually present in the sarcode, by which the morphological value of the rhizopod body as cell or cell-aggregate is placed beyond all doubt. There are also forms in the protoplasm of which no trace of a cell-nucleus has been found. In such, either the protoplasm of the nucleus is not differentiated as a separate structure (the monera of E. Haeckel), or we have to do with a transient non-nucleated stage in the life-history." If the sponges be included in this class it is true that in them there is a "syncytium or layer of structureless sarcode;" but of it Dr. Nicholson says—"Others of the sarcodes again become indistinguishably amalgamated with one another in progress of growth, and thus give rise to a so-called 'syncytium' or layer of structureless sarcode."

Whenever a structureless, or apparently structureless, sarcode occurs among rhizopods, we may well believe that it has arisen in them, as in their allies the sponges, through the instrumentality of cells. In higher animals I think Mr. Spencer is demonstrably wrong in supposing that any tissues arise independently of cells. Fibrous tissue certainly results from cell-formation and modification. Bone and cartilage are, it is true, not entirely cellular, but in both the interstitial substance between the cells, a product of their activities, is au exaggerated and modified cell-envelope. Therefore, since in animals higher than the rhizopods cells are universal, and since in animals lower than the rhizopods (the lower unicellular organisms) they are of course also universal, it would be inexplicable if in the intermediate animals, the rhizopods, which presumably mark a stage of evolution between the higher and the lower, cells were not present.

The animal body, in Mr. Spencer's opinion, is a sort of crystal. It is to be regarded as compounded essentially of "physiological units" of very highly complex chemical compounds, not of cells, of unicellular animals; and as the molecules of a crystal arrange themselves in an aggregate which has a particular shape—the shape of the crystal—so, he thinks, do the physiological units of which an animal is compounded, arrange themselves in an aggregate which has a particular shape—the shape of the animal. Differences in animal shapes are by him accounted for by supposing that there is a slight difference in their physiological units, which in the aggregate produces considerable differences in shape. Thus, a dog differs from a sheep because of a difference in physiological units. A son resembles his father because their physiological units are much alike; he differs somewhat from his father because of a slight, a very slight, difference in physiological units. Physiological units according to him are modifiable by all forces that impinge on the aggregate—the animal. The germ, like the rest of the animal, is compounded of physiological units, and, like the rest of the animal, its units are modifiable by the play of forces to which the aggregate is subjected. Therefore, when the aggregate is modified by forces impinging on it, the germ is likewise modified, wherefore, when, by the assimilation of food-molecules, which in it are built up into physiological units similar to those composing it, growth occurs, it develops or crystallizes into an organism, which reproduces the modifications of the parent organism. In this way is the transmissibility of acquired traits accounted for, and, in Mr. Spencer's opinion, to doubt that transmissibility is to doubt the persistence of force.

Mr. Spencer's arguments are marshalled with great skill, and the theory which he supports by them is highly philosophical; but there are fatal objections to it. It is impossible to admit that that form of force which causes, under fit conditions, the development of living organisms, is the same as that which causes, under fit conditions, the growth of crystals. Under fit conditions a crystal grows into a definite shape, according to its kind, and so does an organism; but there the analogy ends. Mr. Spencer, with more reason, might have compared an organism to a river, and reasoned from that basis. A river grows like a crystal and an organism, but, like an organism and unlike a crystal, waxes and wanes according to circumstances, is somewhat heterogeneous in composition, consists from time to time of new material, and so forth.

A crystal is essentially stable and homogeneous throughout, and if in a solution there are molecules of different kinds capable of crystallization, they crystallize separately into different kinds of crystals. "Our very definition of crystal structure is an arrangement of particles, the same about one point as about every other point; hence, in one sense, the smallest fragment of a crystal is complete in itself."[1]

An organism is essentially unstable and heterogeneous; the lowest organisms we are able to examine show in their granular appearance unmistakable evidence of heterogeneity; higher organisms are obviously heterogeneous, and, according to Mr. Spencer, the physiological visits in them must, if they have any existence, be highly heterogeneous, since he speaks of the "specialized molecules of each organ" (*Principles of Biology,* vol. i. p. 178), though here he does not explain by what process of crystallization the molecules of a crystal are "specialized"; since he speaks of "countless different combinations of units derived from parents, and through them from ancestors immediate and remote" (p. 268); and since he speaks of "the mixed physiological units composing any organism, being, as we have seen (?),

[1] *Elements of Crystallography,* p. 10. Williams; Macmillan and Co., 1890.

more or less segregated in the reproductive centres it throws off" (p. 283). Here again it may be asked, by what process of crystallization thousands and tens of thousands of heterogeneous physiological units come to be segregated in the germ? The force of crystallization, as known to physicists, would tend to separate not to segregate them.

"Crystals grow by the addition of regular layers of molecules, arranged just like all other layers. We can set no limit to the size of a crystal so long as the supply of materials and conditions favourable to its formation remain constant. There is in fact the widest divergence in the size of crystal individuals of the same composition and structure. Those of ultra-microscopic dimensions and those many feet in length may be identical in everything but size. Both are equally complete, and one is in no sense the embryo of the other. As a rule, the size of a crystal is inversely proportionate to its purity and perfection of form, but this, as will be seen at once, is dependent on external conditions.

"Finally, the individual crystal, unlike the individual organism, will remain unchanged so long as its surroundings are favourable to existence."—*Crystallography*, pp. 10-1.

But living organisms grow after an altogether different fashion, they do not grow by additions to their outer layers. Small crystals resemble large crystals of the same kind in all except size, but embryos do not so resemble adults of the same species; they resemble more closely lower animals. The changes undergone by the embryo, explicable by the cell theory, but inexplicable by Mr. Spencer's theory, are alone sufficient to prove that the development of the organism is not a process of crystallization.

Mr. Spencer says, "While an aggregate of physiological units continues to grow, . . . no equilibrium can be arrived at between the whole and its parts" (p. 288); but as regards all crystals there is perfect equilibrium between the whole and its parts during every stage of growth.

The force of crystallization binds the component parts of the mass firmly together; never do crystals emit minute particles to serve as germs of future crystals, but that force which causes the development of living organisms causes also those organisms in time to separate from themselves minute particles, the germs of future like organisms.

The stag at one season of the year, when he is frequently engaged in combats, carries large and massive horns; these are shed, so that at another season, when he is more peaceably inclined, he is without them, and thereby the shape of the animal taken as a whole is greatly modified. If the stag is a sort of crystal, under what law of crystallization does this crystal modify itself periodically for the purpose of fighting? Are we to suppose that the stag's physiological units periodically undergo so great a change as is implied in his great change of form?

Pages might be filled with similar objections to Mr. Spencer's theory, but it is needless to multiply examples. While fully admitting the persistence of force, we cannot admit that the persistence of force influences heredity in the way he would have us believe. Without better proof we cannot even admit the existence of physiological units, and certainly we cannot admit that higher organisms are not essentially cellular,—that they are not compounded of adherent unicellular organisms and their products.

CHAPTER II

On *à priori* grounds we must conclude therefore, that acquired variations are not transmissible, but here again I must guard myself against misconception. In the last analysis all inborn variations must of course be acquired variations. For, eliminating the effects of conjugation, since the child varies somewhat from the parent, the constitution of the germ cell, whence the child was derived, must also have varied somewhat from the constitution of the germ cell whence the parent was derived, and this variation can only have arisen as the effect of some external cause or causes. By inborn variations are therefore meant such variations in the organism as result from changes in the constitution of the germ cell whence it is derived, which changes in the constitution of the germ cell may, for instance, be produced by such alterations in the nutritive fluids as are caused by exercising the arm muscles. What is denied is, that such changes in the nutritive fluids as the above, usually produce changes of such a kind in the constitution of the germ cell as result, after conjugation and proliferation, in variations in the new organism similar to those variations in the parent organism, which resulted from that exercise which produced the alteration in the nutritive fluids. In other words, though acquired variations may so alter the nutrient fluids as to cause a secondary alteration in the constitution of the germ cell, which in turn may cause it to proliferate into an organism different from what would otherwise have arisen, yet it is impossible to believe that the variation in the new organism will, except as a mere fortuitous coincidence, be of the same kind as the variation in the parent organism.

That external causes do alter the constitution of the germ cells is indeed proved by such facts as the following. If we pass the virus of small-pox, by means of inoculation, through a series of calves, the disease becomes altered in character, becomes cow-pox, not small-pox, and if a man be inoculated from the last of the series he also takes cow-pox, as do a succession of human beings inoculated the one from the other with the altered virus. Now since there can be no doubt that small-pox is caused by a microbe, it is clear that residence in the calf during some generations so profoundly alters the nature of this microbe, that it causes in man a disease quite different from that which its ancestors caused. That is to say, in this case acquired variations are transmitted. The explanation is, that every microbe, being a unicellular organism, is a germ cell, and a germ cell, moreover, which, besides being extremely minute, and relatively simple, is one on which external conditions act directly, and which is therefore comparatively easily altered in constitution, an alteration which it transmits to its offspring. This explanation suffices also to explain why bacteriologists are able to produce what are called attenuated cultivations of various microbes.

Our knowledge of the subject is as yet slight, but it seems possible or probable that just as each multicellular organism is composed of many cells, so each cell may be composed of many smaller units—the biaphors of Weismann—which stand to the cell in the same relation as the cell stands to the multicellular organism; and further, just as the cells of a multicellular organism are differentiated, specialized, and arranged in a definite order, so it is possible or probable are the biaphors of a cell differentiated, specialized, and arranged in a definite order; and as all differences between multicellular organisms are due to differences in the differentiation, specialization, arrangement, and number of their cells, so all differences between cells are possibly or probably due to differences in the differentiation, specialization, arrangement, and number of their biaphors; and as higher multicellular organisms are more heterogeneous and complex as regards the nature and arrangement of their cells than lower multicellular organisms, so higher unicellular organisms are possibly or probably more heterogeneous and complex as regards the nature and arrangement of their biaphors than lower unicellular organisms; from which, if true, it follows as a corollary, that higher unicellular organisms are less capable of transmitting acquired variations than lower unicellular organisms, a deduction which is in accordance with the facts, so far as they have been observed; for while experiments have demonstrated that the microbes (in every case extremely low organisms) of several diseases are modifiable by cultivation in changed media, there is nothing to prove that higher unicellular organisms such as the amœba are as easily modifiable. As regards the small-pox microbe, since it is so readily modifiable by removal to a different medium,—*i. e.* the calf's body,—it must, if there is any truth in this hypothesis, be a very low organism—low as regards the number, arrangement, differentiation, and specialization of its biaphors—a view which is borne out by the fact, that it is so minute that it has never been seen, notwithstanding the exceptional facilities that the limpid vaccine lymph affords for microscopic investigation; possibly it is so minute as to be quite beyond the powers of devisable microscopes, in which case it will never be discovered.

The theory of evolution, as I understand it, is founded on the supposition—or rather I think I may say the known fact—that the cells of a multicellular organism such as man correspond during the successive stages of the ontogeny, to the cells of the successive non-conjugating generations which intervene between one act of conjugation and the next among unicellular organisms. If this be admitted, it does not affect the argument that the number of cell-generations is in some cases almost infinitely prolonged,—*e. g.* among aphides,—nor even that new organisms in some instances arise from non-conjugating cells as among bees, or by budding as among the hydrozoa. The essential point is, that though the cells remain adherent and undergo differentiation and specialization, yet they are all cell-descendants of the germ cell, or the pair of conjugated germ cells, in precisely the same sense as the cell-descendants of a unicellular organism or conjugated pair of unicellular organisms are its or their cell-descendants. By the theory that inborn variations are alone transmissible, it is assumed that no single cell of a multicellular organism is in part or whole a product of any other co-existing cell or cells, *i. e.* that it has not received from any of them living constituents, but that it is merely a co-descendant with the other cells from a common ancestry; for instance, that the germ cells are not in whole or part products of the other co-existing cells, but are co-descendants with the other cells from a common ancestry, and do not receive from them living "gemmules," which become part of them, and so alter their constitution as to cause them to proliferate into organisms different from that which would otherwise have arisen. If this be true, if the germ cells are not even in part products of the other co-existing cells, and if it be also admitted that acquired variations cannot so alter the nutritive fluids as to produce similar variations in the new organisms which arise from the proliferation of the germ cells, then of course acquired variations cannot be transmitted, and evolution can proceed only on lines of inborn variations. If it is not true, if the germ cells are in part the products of the other co-existing cells, and receive from them "gemmules," and acquired variations are therefore transmissible, then besides the difficulties in the way of belief caused, as already indicated, by the incredible number of the assumed "gemmules," and the difficulties in the way of belief due to the impossibility of conceiving their arrangement in the germ cells, we are faced by the further difficulty of explaining when and by what method of evolution such a process arose. Nothing like it can possibly occur among the descendants of a pair of conjugated unicellular organisms; if it exists at all it must have arisen *de novo* among multicellular organisms of a high order, for among lower multicellular organisms there exists no vehicle for the conveyance of "gemmules" to distant cells.

The closer, therefore, the theory that acquired variations are transmissible is examined, the more incredible on *à priori* grounds does it appear. The upholders of it, however, allege that natural selection—*i. e.* the accumulation of *inborn* variations by natural selection—is quite inadequate to account for evolution in its totality. However this may be, and it is certainly disputable, the fact remains, that the more our knowledge of nature expands the better able are we to explain the whole process of evolution by the accumulation of inborn variations alone; moreover, those who maintain that many cases of evolution are explainable only on the assumption that acquired variations are transmissible, are compelled to admit that in many cases of evolution, more complex and far-reaching than any they instance to the contrary, the accumulation of acquired variations can have played no part. For instance, compare the evolution of the horns of deer with the evolution of worker bees.

Mr. W. I. T. Cunninghame, in his English translation of Eimer's *Organic Evolution,* says, "No other mammals have been stated to possess two little symmetrical excrescences on the frontal bones as an occasional variation. What caused such excrescences to appear in the ancestors of horned ruminants? Butting would produce them, and no other cause can be suggested which would." The last sentence may be amplified as follows. "Butting would so irritate or stimulate the periostial cells of the frontal bone as to cause them to deposit more bone, thus giving rise to bony excrescences which, transmitted to descendants, and increased during many generations by more butting, would at last result in horns; thus, and thus only, can we account for the evolution of horns." Mr. Cunninghame notwithstanding, it seems to me easy enough to account for the evolution of horns by the theory that inborn variations are alone transmitted, but in doing so we must not seek, as he imagines is necessary, for instances of other mammals which "possess two little symmetrical excrescences on the frontal bones as an occasional variation." Such excrescences would be rare and abnormal, and evolution does not proceed on lines of rare and abnormal variations, which are soon swamped by interbreeding, but on lines of normal variations, as regards which every individual of the species rises above or falls below the specific mean. All ruminants have frontal bones; when butting became of importance in the unconscious struggle for descendants waged by the hornless ancestors of horned ruminants, it is surely clear that those individuals whose skulls were best adapted to butting, who had varied so that their frontal bones were thick and solid, were, other things equal, at an advantage, and were able to kill or drive away from the females their less "fit" rivals. A race with thick and solid frontal bones would thus have been established, and it is not difficult to understand how, by the same process of evolution, those portions of the frontal bones which most received the impact of blows, and were most employed to injure rivals, thickened and solidified most (not by the accumulation of acquired variations, but solely by the accumulation of inborn variations, by the survival of the fittest), till there gradually appeared excrescences, which, by the continual survival of the more and more "fit," grew larger in each successive generation, till at last the evolution of horns was completed. That the horns of deer are two in number and symmetrical does not affect the question, for a single horn has been evolved in both the rhinoceros and the narwhale, and in the latter the horn is asymmetrical. Moreover, a fatal objection to Mr. Cunninghame's theory is the fact that horns do not grow under direct stimulation, that of use. It may be that they grow in response to some form of indirect stimulation (sexual emotion), and that they would not develop in an imperfect animal, but I am unable to vouch for this. The essential point is that during growth the horns, being covered by tender "velvet," are carefully protected by the animal from injury, and it is not until the velvet is dead and has peeled away, and growth has ceased, that the animal engages in combat. Use therefore can have had nothing to do with the development of horns.

In the case of worker bees it is impossible that evolution can have proceeded on lines of acquired variations, for the simple reason that since workers have no descendants they cannot transmit any variation acquired or inborn. Their evolution must therefore have resulted from variations in the successive ancestral queens of such a nature as caused them to produce in the course of generations better and better workers; those queens which did not vary favourably in this respect perishing with their hives in consequence of competition and leaving no descendants. Now the variations in the queens which caused them to produce better and better workers cannot have been acquired, for queens do not work, and the evolution of workers can therefore have been due only to the accumulation of inborn variations in the successive queens. It is true that the actual anatomical structure and the instincts of the queens and workers, one or both, are to be regarded in great measure as acquired variations, since they result largely from the kind of food supplied to the larvæ, but these acquired variations in the workers cannot be transmitted to offspring, for, as I say, they have no offspring. The very power of acquiring them must have resulted from the accumulation of inborn variations.

The social system and the subdivision of labour among bees (and ants) are so remarkable, that they present perhaps the most extreme examples of evolution to be found in nature, not even excepting that of man. Yet in the face of this we find many writers insisting, in cases as simple as that of the evolution of horns, that acquired variations are factors of the evolution, and doing so when, in other strictly analogous cases, they admit that acquired variations can have had no part. For example, Mr. Herbert Spencer says (*Principles of Biology*, vol. i. pp. 295-6)—

"Taking, for instance, the callosities on the knuckles of the *Gorilla*, which are adapted to its habit of partially supporting itself on its closed hands when moving along the ground,—shall we suppose that these defensive thickenings are produced afresh in each individual by the direct actions, or that they are inherited modifications caused by such direct actions, or that they are wholly due to the natural selection of spontaneous variations? This last supposition does not seem a probable one, since it implies that those slight extra thicknesses of skin on the knuckles with which we must suppose the selection to have commenced, were so advantageous as to cause survivals of the individuals having them. That survivals so caused, if they ever occurred at all, should have occurred with the frequency requisite to establish and increase the variation, is hardly supposable. And if we reject, as also unlikely, the reproduction of these callosities *de novo* in each individual, there remains only the inference that they have arisen by the transmission and accumulation of functional adaption.

"Another case which seems interpretable only in an analogous way, is that of the spurs which are developed on the wings of certain birds—on those of the Chaja screamer for example. These are weapons of offence and defence. It is a familiar fact that many birds strike with their wings, often giving severe blows; and in the birds named, the blows are made more formidable by the horny, dagger-shaped growths standing out on those points of the wings which deliver them. Are these spurs directly or indirectly adaptive? To conclude that natural selection of spontaneous variations has caused them, is to conclude that, without any local stimulus, thickenings of the skin occurred symmetrically on the two wings at the places required; that such thickenings, so localized, happened to arise in birds given to using their wings in fight; and that on their first appearance the thickenings were decided enough to give appreciable advantages to the individuals distinguished by them—advantages in bearing the reaction of the blows, if not in inflicting the blows. But to conclude this is, I think, to conclude against probability. Contrariwise, if we assume that the thickenings of the epidermis produced by habitual rough usage is inheritable, the development of these structures presents no difficulty. The points of impact would become indurated in wings used for striking with unusual frequency. The callosities of the surface thus generated, rendering the parts less sensitive, would enable the bird in which they arose to give, without injury to itself, violent blows and a greater number of them, so, in some cases, helping it to conquer and survive. Among its descendants, inheriting the modification and the accompanying habit, the thickening would be further increased in the same way—survival of the fittest tending ever to accelerate the process. Presently the horny nodes thus formed, hitherto defensive only in their effects, would by their prominence become offensive, would make the blows given more hurtful. And now Natural Selection, aiding more actively, would mould the nodes into spurs; the individuals in which the nodes were most pointed would be apt to survive and propagate; and the pointedness, generation after generation thus increased, would end in the well-adapted shape we see."

But on page 274 he says—

"Besides ascribing to Natural Selection the rise of various internal modifications of other classes than those above, we must ascribe some even of these to Natural Selection. It is so with the dense deposits which form thorns and the shells of nuts: these cannot have resulted from any inner reactions immediately called forth by outer actions, but must have resulted mediately through the effects of such outer actions on the species."

But if the accumulation of inborn variations is considered sufficient to account for the indurated tissues which form the shells of nuts and the thorns of plants, it is difficult to understand why it should be considered insufficient to account for the indurated tissues on the knuckles of the gorilla and on the wings of the Chaja screamer. The transformation of structures which were totally different into shells and thorns appears at the least to furnish as extreme examples of evolution as callosities and spurs, which, after all, are only specialized thickenings of the epidermis. As regards the gorilla, Mr. Spencer apparently forgets that the callosities on the hands of men who do hard manual labour, and on the feet of men who go barefoot, are never reproduced in their infants, who were born as soft-handed and soft-soled as the infants of those who do no labour, nor go barefoot. As regards the Chaja screamer, Mr. Spencer, like Mr. Cunninghame when writing on the evolution of horns, does not correctly state the case when he says, "To conclude that Natural Selection of spontaneous variations has caused them, is to conclude that, without any local stimulus, thickenings of the skin occurred symmetrically on the two wings at the places required; that such thickenings, so localized, happened to arise in birds given to using their wings in fight; and that on their first appearance the thickenings were decided enough to give appreciable advantages to the individuals distinguished by them." His words imply that the thickenings were "occasional" variations, and that, if we hold that spontaneous variations are alone transmitted, we must believe that the evolution of spurs in the Chaja screamer was due to the increased rate of survival conferred by these rare variations—for rare they must have been to have occurred "symmetrically," "at points required." But, as already stated, evolution does not proceed on lines of rare variations, which are necessarily swamped during the process of interbreeding, but on lines of normal variations. Every screamer must have varied above or below a certain mean as regards the thickness and denseness of the tissues "at the points required," and fortunate variations in this case must have been particularly important, since unfavourable variations must have led, in a prolonged combat or in a series of combats, to such a denudation of the epidermis as rendered a renewal of combat painful, and thereby caused a corresponding disinclination to fight for food or females. Fitness in this respect, other things equal, must therefore have contributed, as in the horns of deer, to the ability to leave offspring, and thus led to the evolution of spurs —an evolution slow at first, but afterwards more rapid as the spurs increased in size and importance.

Again and again in the literature of biology we may find objections to the theory of evolution by the accumulation of inborn variations alone, founded on the assumption that such evolution must depend on the perpetuation and accentuation of abnormal variations; that is, of variations which far transcend the specific mean, or which are entirely new structures, and therefore practically speaking deformities, and which occur in only one individual in a thousand, or in ten thousand, or in ten million. Of course if such an assumption is made, it is easy enough to demolish the theory founded on it, and then to declare that, since evolution certainly has occurred, and since the accumulation of inborn traits is inadequate to account for it, it must be due in part at least to the accumulation of acquired traits. Lord Salisbury made this assumption in his speech delivered before the British Association at Oxford in 1894, and going beyond Mr. Spencer, came to the conclusion, remarkable in a President of that learned body, which has done so much to elucidate the sciences of Paleontology and Embryology, from which are drawn decisive proofs of the theory, that on this account, and because mathematicians deny that the globe has been habitable for so long a period as, in the opinion of geologists, is necessary for the evolution of highest life from lowest life, it (the theory of evolution) must be taken as "not proven."

It is not too much to say that Lord Salisbury entirely mistook the point at issue. It is not now doubted by the majority, the immense majority of those acquainted with the facts, that the organic world arose by evolution; that it did so is as much beyond doubt as the fact (which still has doubters) that the world is of spherical shape. The question of time is certainly interesting, but unless it is proved, and this has never been done, that life cannot have existed long enough on the globe to permit of the evolution of present forms, it does not touch the matter at issue. The mathematicians or the geologists, or both the mathematicians and the geologists, may be wrong, but the undisputed and undisputable fact remains, that, during all geological time, the types of life underwent a gradual change, which, as Lord Salisbury admits, must have been due to evolution, or, as the only alternative, to a process of special creations that exactly mimicked evolution—the last a childish theory, for which there is no shred of evidence. The only point at issue is, as to how the evolution proceeded, by what method. Was it by the inheritance and accumulation of inborn traits, or of acquired traits, or both? Lord Salisbury merely demonstrated, with the skill of the practised debater, that the particular theory of evolution which he was discussing, and which he imagined was the only one, the theory that evolution results from the accumulation of rare abnormalities, is absurd. It certainly is conspicuously absurd, but evolution is not thereby disproved. As well might a man, after demonstrating the absurdity of this or that theory of volcanic action, imagine that he has disproved the existence of volcanoes. I give his words.

"There is the difficulty. We cannot demonstrate the process of Natural Selection in detail; we cannot even, with more or less ease, imagine it. It is purely hypothetical. No man, so far as we know, has ever seen it at work. An accidental variation may have been perpetuated by inheritance, and, in the struggle for existence, the bearer of it have replaced, by virtue of the survival of the fittest, his less improved competitors, but as far as we know, no man or succession of men have ever observed the whole process in any single case, and certainly no man has ever recorded the observation. Variation by *artificial* selection of course we know very well; but the intervention of the cattle breeder and the pigeon fancier is the essence of artificial selection. It is effected by their own action in crossing, by their skill in bringing the right mates together, to produce the progeniture they want. But in Natural Selection who is to supply the breeder's place? Unless the crossing is properly arranged the new breed will never come into being. What is to secure that the two individuals of opposite sexes in the primeval forest, who have been both accidentally blessed with the same advantageous variation, shall meet and transmit by inheritance that variation to their successors? Unless this step is made good, the modification will never get a start; and yet there is nothing to insure that step except pure chance. The law of chances takes the place of the cattle breeder and the pigeon fancier. The biologists do well to ask for an immeasurable expanse of time, if the occasional meetings of advantageously varied couples from age to age are to provide the pedigree of modifications which unite us to our ancestor the jelly-fish. Of course the struggle for existence, and the survival of the fittest, would in the long run secure the predominance of the stronger breed over the weaker. But it would be of no use in setting the improved breed going. There would be no time. No possible variation which is known to our experience, in the short time that elapses in a single life between the moment of maturity and the age of reproduction, could enable the varied individual to clear the field of all competitors, either by slaughtering them or starving them out. But unless the struggle for existence took this summary and internecine character, there would be nothing but mere chance to secure that the advantageously varied bridegroom at one end of the wood should meet the bride who by a happy contingency had been advantageously varied in the same direction at the same time at the other end of the wood. It would be a mere chance if they ever knew of each other's existence—a still more unlikely chance that they should resist on both sides all temptations to a less advantageous alliance. But unless they did so the new breed would never even begin, let alone the question of its perpetuation after it had begun. I think Professor Weismann is justified in saying that we cannot, either with more or less ease, imagine the process of Natural Selection."—*Nature*, August 9, 1894.

I think we can with considerable ease imagine the process of evolution. So far from being purely hypothetical, geology and embryology render it certain. No man has seen it at work among the higher species of plants and animals to which the attention of Lord Salisbury, like that of the rest of the general public, is directed, for since it normally proceeds on lines of minute variations, not on lines of great abnormalities, no man is able to observe a sufficient number of generations; but any man may observe its operation among the lower plants and animals (*e.g.* among the short-lived microbes of disease), and scores of men, as will be seen, have recorded their observations as regards these latter. Even artificial selection usually proceeds on lines of normal variations. For instance, among cattle and pigeons, the domesticated animals Lord Salisbury specially quotes, how many peculiarities that are examples of evolution, not of retrogression, have been otherwise developed. When he speaks of two individuals accidentally blessed with the same variation, he is alluding of course to abnormal inborn variations. It is certainly improbable that two individuals should exhibit the same abnormality at the same time; it is still more improbable that they should meet and interbreed if they do; and even more improbable that the abnormality should be of such a nature as to enable the favoured individuals to exterminate, in the struggle for existence, all competitors in a single generation, especially since abnormalities, because they imply a wide departure from the specific type, are almost always examples of retrogression, not of evolution, and therefore a cause of elimination, not of survival. If we think of any species of wild animal, it is difficult to believe that any of its traits had their starting-points in abnormalities which were afterwards accentuated, but easy to believe that they had, practically speaking, no starting-points, but were all within the potentialities of earliest life when it differentiated from the non-living, and that they arose through the gradual accumulation of normal —that is, small—variations. How, for instance, did the various modifications of epidermal tissue—scales, hair, feathers, horns, nails, teeth, tushes, callosities—arise? Had they their origin in extraordinary abnormalities, whereby an individual or a pair of individuals suddenly developed scales, hair, feathers, &c., which gave them such a great advantage in the struggle for existence that they exterminated all competitors? or did they arise through a gradual process of evolution, during which individuals, who on the whole varied favourably, survived and left offspring, whereas individuals who on the whole varied unfavourably, were eliminated and left no offspring, and as a result scales, hair, feathers, &c. were all evolved from the primitive epidermis, and differences in degree became differences in kind?

Lord Salisbury cannot doubt, and I suppose no one who has the knowledge of a school-boy doubts, that great evolution has occurred in that species of animal with which he is principally familiar—Man—and that without the intervention of the breeder. Among the races of men there are all sorts of differences: there are white and black, copper-coloured and yellow, big and small, bearded and beardless races, and so forth. Does he suppose that in primeval times a man suddenly became white or black, or copper-coloured or yellow, or big or small, or bearded or beardless, and thereby achieved such an advantage that his fellows were exterminated and his progeny alone left to continue the race? or does he believe that differences in colour, in size, in shape, in hair, &c. resulted in man from the gradual accumulation of small normal variations? I think we may say that Lord Salisbury believes that the Past Evolution of man has resulted from the accumulation of small variations, not from the accentuation of great abnormalities. And if he believes this as regards the sizes, shapes, colours, structures, &c. of men, what need is there to believe otherwise as regards the sizes, shapes, colours, structures, &c. of birds and beasts? Moreover, if a little evolution—great actually in the case of man, but comparatively speaking little—has occurred in little time, what ground is there for believing that much evolution has not occurred in immensely longer time? If varietal differences have undoubtedly arisen through evolution in a few thousands of years, why should not specific or ordinal differences have arisen during the lapse of many thousands or millions of years? Why then should Lord Salisbury return "a verdict of not proven upon the wider issues the Darwinian school has raised"? When he does so he is most illogical.

That evolution should occur, there is no need that the male and female at opposite ends of the wood who have varied favourably, should meet and interbreed and resist temptation by the way. Every individual of the species they encounter will, on the average, have varied as favourably as themselves, for those who varied unfavourably will have been exterminated. It is true that, owing to retrogression, the individuals of a new generation, on the average, are less perfectly adapted to the environment than their progenitors—that is, the birth mean of the new generation is below the survival mean of the preceding generation; but in a species that is undergoing evolution the survival mean of the new generation, by reason of the great elimination of its unfit, is somewhat beyond the survival mean of the preceding generation, and thereby small variations shade in the progress of time into great differences. But never, or very rarely,—so rarely that if they ever occur examples of it are altogether unknown to us in nature,—does a specific change of type result from a great individual variation. Never do we see a wild animal possessed of an abnormality—*i. e.* a great variation—which affords to it such an advantage that it far transcends the rest of its species in the perfection of its adaptation to the environment; and most certainly never do we see a wild animal possessed of an abnormality so great, so favourable to survival, so advantageous in the struggle for existence, that as a result all competitors are exterminated, and the abnormality is imprinted on the race. In fact, as I have already said, "evolution proceeds not on lines of traits, however favourable, which occur infrequently or abnormally, but on lines of traits common to all the individuals of the whole species, that is, traits in which every individual rises or falls below the specific average." The Past Evolution of the races of men surely affords conclusive proof of this.[1]

[1] In the November number of the *Nineteenth Century Review* Mr. Herbert Spencer has some remarks on Lord Salisbury's Oxford speech which are curiously like my own. Mr. Spencer had of course not seen my manuscript, while, on the other hand, at the time his article appeared this book had long been in the hands of the publishers, and the passages in question had already been "set up" in type.

CHAPTER III

Many examples similar to those already given, supported by similar arguments, similarly to be rebutted, are found in the writings of numerous supporters of the theory that acquired variations are transmissible. However, it is perhaps not quite fair to make selections in so wide a field, where good examples and strong arguments may be ignored, while bad examples and weak arguments are made the objects of attack. But Mr. Herbert Spencer is a foeman worthy the steel of any antagonist. His philosophy is largely founded on the assumption that acquired traits are transmissible; and he has lately published a pamphlet on *The Inadequacy of Natural Selection*. This pamphlet may fairly be criticized.[1]

At the beginning Mr. Spencer, quoting Weber's researches, points out that tactual discriminativeness varies greatly in different parts of the human body; the tip of the tongue being able to distinguish the points of a compass when only one-twenty-fourth of an inch apart, the tip of the forefinger when one-twelfth of an inch apart, the breast when one inch and a half apart, the middle of the back only when at least two inches and a half apart, and so on. He proceeds to argue very convincingly, that these differences in discriminativeness

[1] *The Inadequacy of Natural Selection*. Williams and Norgate.

cannot be due to the action of Natural Selection (*i. e.* to the accumulation of inborn traits), for, to take an example, the fact that the tip of the nose has more than three times the power of discrimination possessed by the lower part of the forehead is not of such moment to the individual as to be important as a factor in survival, whereas it is manifest that the tactual sensibility of each part is proportionate to the amount of tactual use to which it is put. He argues, therefore, that the differences of tactual sensibility between different parts of the surface of the body are acquired, not inborn variations. All this may be admitted, but it appears to me that it does not touch the matter in dispute. The question is not as to whether variations can be acquired, but whether they can be transmitted. Mr. Spencer himself, by his experiments on the fingers of compositors, proves that greatly increased tactual discriminativeness may be acquired by individuals, but he does not prove, nor does he attempt to prove, that the increase, in however small a degree, is inherited by the offspring. It may, however, be argued, and I think it is implied in Mr. Spencer's paper, that if it be admitted, as I think it must, that the differences in the tactual discriminativeness of the various parts of the body are not altogether or even mainly due to the accumulation of inborn variations, but are in part or even mainly the outcome of acquired variations, then these differences are so great that they cannot have arisen as acquired variations in a single individual, but must have resulted from the accumulation of the acquired and *transmitted* variations of a *line* of individuals. Let us examine the question more closely.

It is known to all that the structures of individual organisms, especially the higher multicellular animal organisms, possess the power of adapting themselves to changes in the environment by varying to quite a remarkable degree (*e.g.* stimulated by exercise, the muscles and glands increase in size and in vigour; under intermittent pressure the skin thickens and hardens; as a consequence of the deep breathing caused by a venous condition of the blood, the lungs expand); but while the power of varying is clearly transmissible, there is nothing to show that the acquired variations themselves, which result from the exercise of it, are at all transmissible, whereas there is a mass of negative evidence tending to prove the converse; for instance, millions of people acquire callosities in their hands and feet, but none ever transmit them to their offspring—only the power to acquire the callosities anew is transmitted. Now this vitally important power, which can have arisen only by the accumulation of inborn variations (for instance, the accumulation of acquired variations can have had nothing to do with the evolution of the power the organism possesses of repairing a cut or abraded surface), is so great that, in a high multicellular adult organism such as a man, it is impossible at first sight to say what part of the whole development is due to the accumulation of inborn variations, and what to *individually* acquired and non-transmissible variations. For instance, since the muscles may be increased considerably in size if exercised, and may practically disappear if not exercised, we cannot say at first sight how much of the muscular development of a man is due to direct inheritance, and how much to exercise—*i. e.* to the power of varying to suit the circumstances, of developing under appropriate stimulation.

If the limb of an infant be paralyzed or so injured that the joints are locked, then, in consequence of the lack of stimulation, it (the limb) grows very little afterwards; muscles, bones, ligaments, blood-vessels, &c. all remain dwarfed and undeveloped. Again, if an adult limb be paralyzed, its component structures, no longer maintained by the stimulation of use, atrophy in their totality, *i.e.* lose part of their full development, especially as regards the most active and changeable tissues, those in which under stimulation chemical and vital changes most actively occur, the muscles. Such an atrophic change, since it results in a condition different from that which normally obtains, we term an acquired variation; but it is a question whether the full normal development of the adult limb is not in itself an acquired variation, for, unlike the horns of deer, which reach their full development without direct stimulation, the adult limb can reach its full normal development only by the aid of such stimulation acting on the inborn power to vary; and since such development can be attained only as a reaction to the direct action of the environment, it is difficult to understand why the "abnormal development" of the blacksmith's arm, due to and maintained by a greater amount of stimulation, should be termed an acquired variation, when the "normal" development of the muscles of an ordinary arm, due to and maintained by a lesser amount of stimulation, should not be so termed. I think on consideration it will be apparent that both the "normal" development and the more than "normal" development are really acquired variations produced by the direct action of the environment on the immense power that high multicellular organisms have of varying in such a manner as to put themselves in harmony with it.

Given a sufficient supply of food, low multicellular organisms are apparently able to reach their full development in the absence of other stimuli, but higher in the scale it becomes apparent that organisms are less and less so able. As the environment becomes more and more complex and heterogeneous, so the action of Natural Selection has developed more and more the power of individual organisms to vary in correspondence along certain lines. Not only are such special structures (*e.g.*

limbs) developed as enable the organism to place itself in harmony with a complex environment, but to further increase this harmony the structures themselves are rendered highly variable; whereby extreme development of this or that structure or organ occurs in stress of need, but useless developments which would only encumber the organism do not occur. For this reason the muscles in a clerk's arm do not develop to the same extent as the muscles in a blacksmith's arm.

This evolution of corporal variability corresponds, as will be seen later, to an equal or greater evolution of mental variability, so that physically as well as mentally the action of Natural Selection has been to produce great individual powers of varying; but the variations produced under this power are not transmitted, only the power to vary and to acquire under appropriate stimulation similar variations is transmitted; thus, to take an example, no infant is born of fully developed parents whose limbs or mental powers can attain a similar full development without stimulation from the environment.

As regards physical variability, with which alone we are at present concerned, in general each individual of a species in his "normal" development nearly attains its limits (*e. g.* beyond the normal development of muscle, but a relatively small further development is possible); the limit being fixed by the struggle between Natural Selection and Atavism, the one operating to increase variability, the other to diminish it, to bring it to a condition of ancestral non-variability.

But all the structures do not "normally" approach the limit to an equal degree, horns, hair (in animals in which hair is merely ornamental, and in those living in an equable climate, when variability under stimulation in this respect is not essential to survival), teeth, nails, &c., if we except the effects of disease, which cannot properly be regarded as developmental, vary little or not at all under stimulation; muscle, as we see, normally attains nearly the limits of its power to vary, as also do healthy bone and cartilage; while other structures, such as skin, adipose tissue, and nervous tissue, do not "normally" approach so nearly the limits of their powers to vary. The skin thickens and hardens greatly in response to the stimulation of intermittent pressure or of moderate irritation, or it may expand immensely as over tumours; adipose tissue has a great power of growth; nervous tissue, judged by the function it performs, whether of sensation, perception, thought, &c., has also the power under continued stimulation of varying very greatly beyond the normal limits.

We may now ask ourselves the question—How much of the tactual discriminativeness of the skin in various parts of the body is due to direct inheritance, and how much is *individually* acquired under the influence of more or less continued stimulation? The answer, I think, must be, that while some of the discriminativeness is doubtless due to direct inheritance,—for in various parts of the body where it is of particular importance (*e. g.* hands and feet in man, tongue in some animals) are found special nervous elements which subserve the sense of touch,—yet beyond doubt much of it or most of it is not due to direct inheritance, but to the power of the individual organism to vary in correspondence with the environment; the tactual discriminativeness being most developed in those parts where it is oftenest stimulated. In this way may be explained all or most of the superior discriminativeness of the front of the body and the nose, as compared to the back of the body and the lower part of the forehead respectively.[1]

[1] Since writing the above I have come across the following paragraph, which amply confirms the foregoing conclusion.

"Mr. H., who in the fall of 1892 was the 'coach' of a university

Mr. Spencer next discusses a question with which we

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football team, had his left forearm broken in a hotly-contested game, while playing his eleven against that of another institution. The surgeon who was called did his work in such a bungling manner that after the bones had begun to knit, the arm had to be broken over again in order to set it properly. To keep it in the correct position a plaster cast was made which held it firmly. This plaster-of-paris case extended from the knuckles to a point above the elbow. After three months the case and bandage were removed. Of course, during the entire period when encased in the plaster, the forearm had not been moved either at the wrist or at the elbow. I then endeavoured to test the sensibility of the skin on this arm which had not been moved for so long a time. To do this I applied the points of a pair of dividers or compasses which were separated more or less widely, after the manner of the usual æsthesiometric tests. The forearm was divided into four different areas for the purposes of more accurate comparison with the sensitiveness of the skin on the uninjured right forearm of the same person. Without going into detail, it should be stated that on the left forearm—the one so long immovable—when the two points touching the skin at a given locality were separated by as much as 55 millimeters, they were felt as one instead of two ; while on the right forearm they would only have to be about 20 millimeters apart in order to be perceived as two. On the back of the 'lame' arm, at a different locality from that just mentioned, it was found that even when the two points of the dividers were 75 and 80 millimeters apart, they were felt as one; while at a corresponding locality on the right arm the skin was so sensitive that points but 17 millimeters apart could be felt as two. It must also be observed that this particular person, with reference to a large number of activities, had always previously been practically ambidextrous—indeed, he never had to favour the left arm at all. In his position as gymnasium instructor, he could (before the accident) manipulate the dumb-bells, Indian clubs, play base-ball, hand-ball, and the like, with the left hand as well as the right. It would seem, then, that the sensibility of the skin over the injured forearm was lost simply because that member was for so long a time immovable. This has an important bearing on the oft-mentioned principle, to the effect that "the localizing power is delicate in proportion as the skin covers a movable part of the body."—Professor W. O. Krohn, *Psychological Review*, pp. 280-1.

It will be observed that Professor Krohn attributes the decrease of sensitivity to the lack of movement, but I think it cannot be doubted, that it should rather be attributed to the lack of appropriate stimulation, to the fact that for a length of time tactual impressions were not received by the skin of the encased arm, in consequence of which variations acquired during the lifetime of the individual were lapsed in part or whole.

have already dealt, and to which therefore we need not devote much space. He writes—

> "For now observe the fact which here chiefly concerns us, that the 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" (pp. 11-2).

He then proceeds to discuss the numerous qualities which in a herd of deer conduce to survival—keenness of scent, sight, and hearing, excellence of digestion through superiority of teeth, gastric juice, stomach, &c., agility, sagacity, power of resisting flies, cold, and so forth. Founding his argument apparently on the assumption that an individual of the herd which is superior as regards one quality will be inferior as regards the others, he comes to the conclusion—"If these other individuals severally profit by their small superiorities, and transmit them to equally large numbers of descendants, no increase of the variation in question can take place, it must soon be cancelled" (p. 13). In other words, interbreeding must soon swamp the specified variation.

The fallacy that underlies Mr. Spencer's argument is this. Excellence in one quality is not usually found dissociated with excellence in other qualities. The vigorous in one respect may be, and commonly are, vigorous in other respects also. Moreover, the qualities are not isolated—keenness of sight in one deer, keenness of hearing in another, agility in a third, and so forth; but every normal deer (and abnormality is rare among wild animals), as regards every other quality, varies a little—not much—above or below a certain mean. If the fittest survive, and we know that on the whole they do survive, it is surely clear, that those animals who in the aggregate of their essential qualities rise above the specific mean, will be those that continue the race; and since among the offspring there will be some who in the aggregate of their qualities surpass their parents, the continued survival of the fittest will in time result in evolution as regards all the other qualities.

The fundamental error is the same as that which he fell into when discussing the spurs of the Chaja screamer, and which Mr. Cunninghame fell into when discussing the horns of deer—the tacit assumption that evolution, if due to the accumulation of inborn variations, must proceed on lines of abnormal variations. Thus he writes in the passage under discussion—"But now suppose that one member of a herd—perhaps because of more efficient teeth, perhaps by great 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 the young if the individual is a hind." But is it necessary to entertain such a very improbable supposition? A deer which suddenly varies in such a manner as to be able to eat and digest a plant which the rest of its species refuse to eat and cannot digest, would be as abnormal as a mammal "with two little excrescences on its frontal bones as an occasional variation," or a bird "much given to fight," "on the wings of which thickenings of the skin occurred," "symmetrically," "at the points required," which "on their first appearance were decided enough to give appreciable advantages to the individuals distinguished by them." Did such a variation occur as an abnormality it would soon be swamped by interbreeding. But if under stress of scarcity the herd were compelled to supplement its diet with the leaves of the hitherto uneaten plant, it is easy to understand that, other things equal, evolution would occur through the continual survival of those that digested the plant best—and we have no reason to suppose that the other things, keenness of scent, agility, &c., would not on the average be equal, but every reason to suppose that they would. Peculiarities like the above do not spring into the world like Minerva, fully developed, but arise by a slow process of growth.

Mr. Spencer next discusses the question of retrogression—such retrogression, for example, as is exhibited by the eyes of the proteus, an animal now inhabiting dark caverns, but evidently descended from ancestors that lived in the light of day, and possessed well-developed eyes. As usual there are two principal theories whereby it is sought to account for this retrogression; one of which attributes it to the cessation of selection, the other in whole or part to the cessation of use; the one to the progressive lapsing, in consequence of the withdrawal of the preserving influence of selection, of those inborn variations, the accumulation of which, as is supposed by the supporters of this theory, resulted in the fully-developed ancestral eye; the other in whole or part to the progressive lapsing, in consequence of the withdrawal of the preserving influence of use, of those acquired variations, the accumulation of which, as is supposed by the supporters of this theory, resulted in the fully-developed eye. These are the two main positions, but a subsidiary question is—Has Natural Selection—reversed Natural Selection, as it is called in this case—aided Cessation of Selection, or Cessation of Use, in causing retrogression in the eyes of animals to whom, since they have migrated to dark places, sight is useless? I think there is abundant evidence proving that it has so aided. An eye, if useless, must be much worse than useless to the animal possessing it. In its exposed position it is in constant danger of injury, and, from the delicate nature of the organ, harm to it more disastrously affects the whole organism than harm to an equal extent of any other part of the surface. Eyeless animals, or animals with small rudimentary eyes, therefore, are at an advantage in the dark, as compared to animals with fully-developed eyes. The latter would therefore tend to be eliminated, while the former would tend to survive and have offspring. Accordingly we find that certain cavern-inhabiting crustacea, descended from stalk-eyed ancestors, have lost their eyes, but not the stalks on which the eyes were carried. The loss of the eyes cannot be set down to Cessation of Selection alone, for, since the stalks must have appeared later in the phylogeny than the eyes, the former are presumably the less stable structures, and therefore liable to disappear sooner on Cessation of Selection, whereas they have persisted. In like manner the disappearance of the eyes cannot be attributed entirely to Cessation of Use, for, if Cessation of Use is a cause of specific degeneration, it ought to have caused an equal or indeed greater retrogression of the less stable stalks. Therefore in this case we can account for the persistence of the stalk, when the eye has disappeared, only by supposing that Natural Selection—reversed Natural Selection—has co-operated with Cessation of Selection (or Cessation of Use, according to the theory we hold) to remove the worse than useless eye, leaving the no more useful but much less harmful stalk persistent. Similarly we can explain the fact that the degenerate eyes of the proteus are covered with skin neither by the theory of Cessation of Selection nor by the theory of the Cessation of Use, but only by supposing that the accumulation of inborn variations has resulted in an overgrowth of protective skin.

This latter, however, is a case of evolution, not of retrogression, and is therefore not due to "what has been called in questionable logic," reversed selection—"the selection which effects not increase of an organ but decrease of it" (p. 13).

Reversed Selection is supposed by some biologists to have operated in another way as a cause of retrogression, viz. by effecting economy of nutrition. They hold that when in any species an organ becomes useless, those individuals that vary in such a manner that in them supplies of nutrition are diverted from the useless organ to useful organs are at an advantage over those individuals which do not so vary, and that this advantage, due to economy of nutrition, is great enough to be an important factor in survival, and that to it therefore is due in whole or part the retrogression of a useless organ.

In championing the opinion that retrogression is due to disuse, Mr. Spencer directs his whole powers of attack against this last theory, quite ignoring the other theories I have discussed, which attribute retrogression (1) to Cessation of Selection as regards useless organs, or (2) to Reversed Selection operating against worse than useless organs, or (3) to the combined action of both Cessation of Selection and Reversed Selection. So far as his arguments go, I think it must be admitted that they are conclusive.

He says, "Suppose that in a 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 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" (p. 14).

He next enters into elaborate arithmetical calculation concerning the eye of the proteus. "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 the decrease of the eyes, would so benefit the rest of the creature's organization as to give it an appreciably greater chance of survival, and an appreciably greater multiplication of descendants. Does any one accept this inference?" (pp. 18-9).

In reply to the above Mr. Wallace writes—

"The eye is treated as if it were mere protoplasm weighing so many grains, instead of being a highly complex organ, with which muscles, blood-vessels, and nerves are connected and co-ordinated in greater proportion perhaps than any other organ. I presume the original eye of the ancestral proteus must have had its three distinct sets of nerves—those of vision, of sensation, and of motion—involving in their normal use the expenditure of a considerable amount of nervous energy, besides the various muscles and blood-vessels connected with it. To measure the benefit to be derived from the entire suppression of such a complex organ, when it became useless, as no more than the gain of so many grains of simple muscular tissue, appears to me to be an extraordinary misconception of the conditions of the problem."—*Fortnightly Review*, 1893, p. 655.

But though the eye is perhaps more richly supplied with muscles, blood-vessels, and nerves than any other organ, yet, presumably, these structures here require the same kind of nutriment as they do elsewhere in the body; and since they are relatively minute they must, however functionally active, require a relatively minute quantity of nutriment—a quantity far too little when diverted to other organs to be important as a factor in survival. Therefore if the nutrition supplied to the eye is held to be such a drain on the resources of the organism that the saving of it appreciably affects the survival rate, we must suppose that the material supplied to its peculiar structures is physiologically extremely expensive. But as to this Mr. Spencer writes—

"If any one remembers how relatively enormous are the eyes of a fish just out of the egg—a pair of eyes with a body and head attached; if he then remembers that every egg contains material for such a pair of eyes, he will see that eye-material constitutes a very considerable part of the fish's roe; and that, since the female fish provides this quantity every year, it cannot be expensive" (p. 65).

In the proteus in particular eye-material cannot be expensive, for, since the animal does not exercise vision, the material is not used up, and therefore has not to be replaced. I think therefore that the hypothesis that the retrogression of the eyes of cavern-dwelling animals is at all due to increased survival caused by diversion of nutriment to more useful organs, is untenable. Moreover, the case of the eye is quite an exceptional one; it is an extreme example. Much less can we suppose that the retrogression of other organs is due to economy of nutrition. For instance, as regards the toes of horses; the modern horse has only one toe on each limb, his ancestors had five. While the transition from five toes to one was occurring, thousands of generations lived and died, and tens of thousands of years elapsed. If economy of nutrition, in this case, led to the retrogression of the four toes which are now absent, by influencing the survival rate, what was the amount of nutriment saved for the other organs by those individuals that varied favourably? Such an amount only as could have been supplied by an extra mouthful—I had almost said an extra blade—of grass per diem. It is therefore doubly plain that this theory will not bear examination, and that Mr. Spencer is right in ridiculing it.

As regards the theory that the retrogression of useless organs is due to the effect that injury to them exercises on the chances of survival, here again the eye is an extreme example. No other organ in the body can be thought of as delicate and important which can be as easily injured, and therefore the retrogression of no other organ can be as readily affected by this means.

To take the example given by Mr. Spencer: "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 his ears being less exposed to injury? Assuredly not."

But we know from analogy that under such circumstances the kangaroo's ears, like all structures which become useless or less useful, would undergo retrogression. Therefore since reversed selection could not cause the retrogression, it is clear that the cause that would must be some other cause. Only two other theories remain available; we are driven to attribute this hypothetical retrogression either to Cessation of Selection or to Cessation of Use—to the lapsing of inborn variations or to the lapsing of acquired variations.

The external ears of the kangaroo, to which, obviously, Mr. Spencer alone refers, are passive structures, moved by muscles perhaps, but not owing any part of their spread to those muscles. Mere funnels of cartilage and skin, they are directed this way and that to catch and conduct sounds, but on them as distinguished from the middle and internal ear the loudest sound makes no greater impression than it does on the animal's tail. It is clear therefore that increase or decrease of functional activity cannot cause them to increase or decrease in size or vary in any other way. Whence it is further clear, since, in this case, variations due to functional activity cannot be individually acquired, they cannot be transmitted, and therefore evolution or retrogression in the external ear cannot, in the slightest degree, be due to use or disuse, to the accumulation or the lapsing of acquired variations, but must be attributed wholly to the accumulation or the lapsing of inborn variations.

As regards the eye of the proteus, we cannot prove as positively as in the case of the external ear, that its evolution and subsequent retrogression were due wholly first to the accumulation and subsequently to the lapsing of inborn variations, for here variations may be acquired by the individual, and in the individual acquired variations may be lapsed. Nevertheless since the accumulation of inborn variations is sufficient to account for such examples of extreme evolution as those presented by the special structures of the worker-bee, while the lapsing of inborn variations is sufficient to account for such examples of equally extreme retrogression as those presented by the reproductive organs of that animal, we are entitled to assume that the evolution or the retrogression of the eye is due to the accumulation or to the lapsing of inborn variations alone, unless it can be shown that these causes do not in whole or part sufficiently account for the phenomena, but that these latter must in whole or part be attributed to other causes—*i.e.* to the effects of use or disuse, to the accumulation or to the lapsing of acquired variations.

No serious attempt is made by Mr. Spencer to prove that Cessation of Selection is insufficient to account for retrogression in the eyes of cavern-dwelling species; he sets himself to prove merely that "economy of nutrition" is insufficient, and appears to think that this is all that is necessary. Having destroyed a non-essential outwork he imagines he has conquered the fortress. His error is indicated by Professor Romanes (*Contemporary Review,* April 1893), but he still maintains his view (pp. 66-7). He is criticizing Professor Weismann's theories, and insists that with that scientist Panmixia —*i. e.* Cessation of Selection—"is clearly identified with the selection of smaller variations, and for the reason that economy of nutrition is so achieved." I am not concerned in defending Professor Weismann, and if he believes as Mr. Spencer thinks he believes, in my opinion he is wrong. I am bound to say, however, that his works have conveyed an impression to me entirely different.

When at last Mr. Spencer's attention is forced to Cessation of Selection as a cause of retrogression, he discusses—and dismisses the subject in the following words—

"But while I cannot admit my failure to understand Weismann, I confess that I do not understand Dr. Romanes. How when natural selection, direct or reversed, is set aside, the mere cessation of selection should cause decrease of an organ *irrespective of the direct effects of disuse,* I am unable to see. Clearer conceptions of this matter would be reached if, instead of thinking in abstract terms, the physiological processes concerned were brought into the foreground. Beyond the production of changes in the sizes of parts by the selection of fortuitously-arising variations, I can see but one other cause for the production of them—the competition among the parts for nutriment. This has the effect that active parts are well supplied and grow, while inactive parts are ill supplied and dwindle. This competition is the cause of 'economy of growth'; this is the cause of decrease from disuse; and this is the only conceivable cause of that decrease which Dr. Romanes contends follows the cessation of selection. The three things are aspects of the same thing. And now, before leaving this question, let me remark on the strange proposition which has to be defended by those who deny the dwindling of organs by disuse. Their proposition amounts to this—that for a hundred generations an inactive organ may be partially denuded of blood all through life, and yet in the hundredth generation will be produced of just the same size as in the first" (pp. 67-8).

I have already set forth my reasons for believing that, as regards any structure, retrogression follows Cessation of Selection, because there is a greater tendency in every organism to vary ancestor-wards than in any other direction, and that therefore retrogression in such a case is nothing other than a continued lapsing of inborn variations, the more recent in the phylogeny first, the more ancient in succession later, till there is at length a reversion to the remotest condition, the condition when there was no such structure. But as this theory was not before Mr. Spencer I need not discuss it further here, beyond saying that it offers an explanation why "the mere cessation of selection should cause decrease of an organ *irrespective of the direct effects of disuse.*"

Other parts of the passage quoted above call for more detailed criticism. Mr. Spencer writes—"And now, before leaving this question, let me remark on the strange proposition which has to be defended by those who deny the dwindling of organs by disuse. Their proposition amounts to this—that for a hundred generations an inactive organ may be partially denuded of blood all through life, and yet in the hundredth generation will be produced of just the same size as in the first." And this he writes but a few sentences after—"Clearer conceptions of these matters would be reached if, instead of thinking in abstract terms, the physiological processes concerned were brought into the foreground."

Where shall we find a more daring misuse of "abstract terms," or a more glaring disregard of "the physiological processes concerned," than in the words "for a hundred generations an inactive organ may be partially denuded of blood all through life, and yet in the hundredth generation will be produced of just the same size as in the first"? The words imply that the *same* organ is "denuded of blood for a hundred generations," whereas the truth is that a hundred *similar* organs in a hundred successive generations are denuded of blood, which organs are only connected with preceding and succeeding like organs through germ cells, which are not in any way the products of the organs, but are co-descendants with the cells of the organs from a very remote cell-ancestry; at any rate the germ cells have not been proved to be products in part of the organs, and all theories as yet formulated which are founded on that supposition are fantastic and wild to a degree. If one thing is denuded of blood it does not follow that another thing will atrophy. If the eye of an animal be denuded of blood, much is taken for granted when it is assumed that its germ cell, situated far distant, will proliferate into an organism with an atrophied eye. It is precisely the physiological processes that Mr. Spencer fails to keep in the foreground, and, in this case, no one more than he fails to profit by the salutary advice; "Clearer conceptions of these matters would be reached if, instead of thinking in abstract terms, the physiological processes concerned were brought into the foreground."

CHAPTER IV

THE next question dealt with by Mr. Spencer is that of the co-adaption of animal parts (pp. 20—30). In the moose, for instance, the heavy horns would be useless, or rather a positive encumbrance, if *pari passu* with their evolution there had not occurred a co-adaptive evolution of almost all other parts of the body. In consequence of this evolution the bones of the skull have become massive; to sustain the heavy horns and head, the muscles, bones, and ligaments of the neck have increased in size and strength, and are supplied by larger blood-vessels and nerves; all the structures of the fore and hind limbs have also been correspondingly modified, as have those of the trunk, including the heart and lungs and other viscera. A thousand changes have occurred, all directly referable to the growth of the horns, and all directly co-adaptive. So with all other animals; with the evolution of the elephant's trunk there has proceeded such an evolution of all his other parts that, without the trunk, an animal so shaped could not maintain existence; with the evolution of the giraffe's neck has occurred a corresponding evolution of his limbs and trunk, whereby the animal is enabled to graze at a lofty height; the modifications in man's fore limbs have been accompanied by endless co-adaptive changes in the rest of his body. Mr. Spencer discusses the question in great detail, and comes to the conclusion that "either there has been inheritance of acquired characters or there has been no evolution."

Put shortly his arguments are as follows: In the individual increased functional activity in any structure is followed, generally speaking, by an increase in the size of that structure; this increase of functional activity and size puts a strain on, and is followed by a proportionate increase of activity and consequently of size in many structures co-ordinated with the first; these latter changes are the cause of changes in yet other structures; till, if the initial change be sufficiently important, the whole organism is co-adaptively modified. Now if acquired variations are transmissible, it is easy to understand how these acquired co-adaptive changes, dependent on functional activity, on use, accumulated during generations, may result in co-adaptive evolution of the most far-reaching kind in all the thousand co-ordinated parts of a complex and heterogeneous animal—*e.g.* mammal. But if they are not transmissible, by what means can we account for co-adaptive evolution? How can we explain the fact that a thousand structures are modified in the moose, the elephant, and the man, in obvious co-adaption to changes in the horns, the trunk, and the fore limbs respectively? For if we suppose that only inborn variations are transmissible, we must suppose that all the thousand structures varied favourably simultaneously and proportionately, and to suppose this is to suppose that which is incredible.

For instance, if during the evolution of the great horns of the moose an animal varied spontaneously in that he had larger horns than the ordinary, it is highly unlikely of the thousand structures, the evolution of which has been concomitant with and co-adaptive to the evolution of the horns, that all should have varied favourably, and if they did not the favourable variation of the horns would have been nugatory. With the larger horns it is as likely as not that weaker skull bones or structures in the neck, limbs, or trunk occurred. It is unreasonable to suppose that all the thousand co-ordinated structures did ever vary favourably in one and the same individual, and that they continued to vary favourably in a succession of individuals till the evolution of the moose was complete. On the contrary, it is only reasonable to suppose that some structures varied favourably, some unfavourably, and some varied not at all. A chain is only as strong as its weakest link. If with the larger horns there were associated co-ordinate structures in the head, neck, limbs, or trunk, weaker than the ordinary or only as strong as the ordinary, the larger horns would confer no advantage, but rather from their weight be a positive disadvantage. Consequently individuals with larger horns would not be the fittest, and therefore would not survive in increased numbers, and consequently there would be no evolution in horns. In Mr. Spencer's words—

"Other things equal, the blow given by a larger horn would be a blow given by a heavier mass moving at a smaller velocity; the momentum would be the same as before, and the area of contact with the body struck being somewhat increased, while the velocity was decreased, the injury would be somewhat less. That the horns may become better weapons, the whole apparatus which moves them must be so strengthened as to impress more force on them, and to bear the more violent reaction of the blows given. The bones of the skull on which the horns are seated must be thickened, otherwise they will break. To render the thickening of these bones advantageous the vertebræ of the neck must be further developed; and without the ligaments that hold together these vertebræ and the muscles that move them are also enlarged, nothing will be gained. Such modification of the neck will be useless, or rather will be detrimental, if its fulcrum be not made capable of resisting intense strains; the upper dorsal vertebræ and their spines must be strengthened, that they may withstand the more violent contraction of the neck muscles; and like changes must be made on the scapular arch; still more must there be required a simultaneous development of the bones and muscles of the fore-legs," &c.—*Principles of Biology*, vol. i.

Such, as briefly but as fairly as I am able to give them, are Mr. Spencer's arguments. They are set forth at great length in the pamphlet we have under consideration, and in his book, the *Principles of Biology*, from which the quotation given above is taken. Examined closely, we find that they are nearly the same arguments, but much amplified, as those already dealt with, whereby he sought to prove that accumulation of inborn variations is not alone sufficient to account for the evolution of any given quality (*e. g.* sight, scent, hearing, speed, &c., each of which depends on the functional activity of a group of structures), on the ground that if one animal survived because of superior keenness of sight, and others survived because of superior powers of scent, or hearing, or speed, or agility, or digestion respectively, then, since the qualities essential to survival are numerous, the several superiorities would be cancelled by interbreeding, and in the end there would be no evolution. He forgot, as we saw, that the essential qualities are not found dissociated, keenness of sight in one deer, keenness of hearing in another, and so forth; and moreover, that excellence in one quality is not unusually accompanied by excellence in other qualities, the vigorous in one respect being usually vigorous in other respects as well.

He forgot also that survival does not in general depend on superiority in any one quality, but on a general average of superiority in all essential qualities, and therefore, since the fit survive and the unfit perish, there may be, if the struggle for existence is sufficiently severe, interbreeding notwithstanding, evolution in all the essential qualities, by the accumulation of inborn variations alone. As formerly he argued that the evolution by the accumulation of inborn variations alone of all the numerous qualities necessary for survival is, owing to their multiplicity, impossible; so now he argues that the evolution of any particular quality—*e. g.* fighting power—nay, even of any structure—*e. g.* horns—by the accumulation of inborn variations alone is impossible, owing to the multiplicity of the structures implicated in the evolution.

Leaving for a moment Mr. Spencer's theoretical objections, let us consider some facts of nature which are within the cognizance of every one. The children of the same parents usually differ appreciably when grown in their physical powers, as also do the individual members of a litter of puppies, or the individual chickens hatched from a nest of eggs. These variations must be due to inborn variations, not to variations acquired by the parents and transmitted to the offspring, for, at any rate as regards the puppies and chickens, the circumstances attending the genesis of each individual in the litter or brood are such as practically to preclude the possibility of any one differing from the others except through differences in inborn variations, since in each case all the spermatozoa and ova, and the organisms which arose from them, were circumstanced exactly alike as regards the parent organisms; in other words, if acquired variations are transmissible, each individual under the circumstances must have acquired the same variations, and therefore differences between the individuals are explainable only on the theory that they are due to inborn variations. The extent to which the individuals of a nest or litter differ in powers and qualities is often quite remarkable, and in no respect is this difference more remarkable than in fighting power. This power in dogs and fowls, as in the moose, depends on an immense number of structures, in fact, on almost all the structures of the body, and therefore, when one of a litter or brood exhibits a marked superiority over his brothers in fighting power, we must suppose that his superiority results from a favourable co-adaptive development, due to inborn variations alone, of all the structures concerned in fighting. Now if the ability to secure mates, and therefore descendants, depends largely on the fighting power, as it actually does among so many wild creatures, including the canidæ, the gallinacei, and the cervidæ, are not the differences due to inborn variations alone, which, as we have seen, are considerable, sufficient to lead, by survival of the fittest, to the evolution of the fighting power, *i. e.* to the co-adaptive evolution of all the co-ordinate structures on which that power depends? Would a race descended from the best fighters in each litter or brood differ from a race not so descended, from a race as regards which nature has exercised no selection in this respect? I suppose that no one would maintain that it would not differ, for to maintain such a proposition would be to maintain that a race descended from the biggest and strongest individuals alone would not differ from a race the ancestry of which included smaller and weaker individuals. Yet here we would have just such a case of co-adaptive evolution resulting from the accumulation of inborn variations alone, and involving nearly all the structures in the body, as Mr. Spencer has declared is impossible.

A thousand similar examples can be found. For instance, among men, the great differences frequently observable among the children of the same parents, cannot be due to the parents having acquired and transmitted different traits at different periods of their lives, for if among the offspring of dogs or fowls there are great differences due to inborn variations alone, so also must there be among the offspring of men. Do we not all know families, some members of which are bigger and stronger than the others? In them we see a co-adaptive variation of all the structures, and if among men strength were important as a factor in survival, we should by the survival of the strong individuals get an evolution in strength. Do we not all know families, some members of which can run more swiftly than the others? In them we see a co-adaptive variation of all the structures which subserve speed; and if among men speed were important as a factor in survival, we should by the survival of the swiftest get an evolution in speed. Do we not all know families, some of the members of which are taller than the others? In them we see a co-adaptive variation of many structures; and if among men height were important as a factor in survival, we should by the continued survival of the tallest get an evolution in height. It is needless to multiply examples. Nothing can be more certain than that which is denied by Mr. Spencer, viz. that co-operative parts vary together.

But how shall we surmount the theoretical objection made by him, viz. that it is highly improbable that a thousand co-operative parts should vary, not only together, but proportionately as well? We shall surmount it by taking cognizance of that which has been so strangely overlooked by biologists, viz. that the action of Natural Selection has not only developed complex and heterogeneous structures in the higher animals whereby they are brought into greater harmony with a complex and heterogeneous environment, but that still further to increase this harmony with changeful surroundings, it has developed in the structures an immense power of varying in the individual under appropriate stimulation,[1] the amount of the power of varying as regards each structure being in proportion to the importance of its presence there as a factor in survival; so that some structures, such as teeth, have little or no power of varying under stimulation, since their function is such that in relation to it week by week and year by year the environment varies little; whereas other structures, such as skin or muscle, have greater powers of varying, since their functions are such that in relation to them week by week or year by year the environment varies more. By this power of varying along certain lines under appropriate stimulation, all the multitudinous structures of the high animal body are brought into exquisite harmony, each one with all the others; and therefore, should an animal vary in such a manner as to have a structure—say a certain muscle—larger than the corresponding structure in its progenitors, or rather, to speak more precisely, should an animal vary in such a manner that the structure develops under a "normal" amount of stimulation more than with the same amount of stimulation it developed in the progenitors, then, since this extra development puts an extra strain on all co-operative structures, there results a proportionate increase of size in them all. On the other hand, should an animal vary in such a manner that the structure develops under a normal amount of stimulation less than with the same amount of stimulation the corresponding structure developed in the progenitors, then, since the lesser development puts a smaller strain on all co-operative structures, there results a proportionate and

[1] *Vide* page 89, *et seq.*

lesser development of them all. But, of a thousand structures, some must vary in one direction and some in the other; the general result must therefore be according as the most vary, or as the most important vary; but whatever the general result, there is ever preserved a considerable degree of harmony and proportion between the structures in normal animals; in abnormal animals (*i. e.* in monsters, in deformed persons, and persons with tumours) the structures have not the power to vary proportionately; under stimulation either some develop more in proportion, or others less in proportion than is normal; but these animals being usually among the unfit, perish without leaving descendants.

The general effect of Natural Selection, at least as regards higher animals, therefore, is to produce in the individual organism the power of varying proportionately and harmoniously in all its parts along certain lines in response to stimulation, direct or indirect, from the environment; for instance, owing to direct stimulation the skin of the sole thickens and hardens, owing to indirect stimulation (*i. e.* in response to functional activity caused indirectly by events or objects in the environment), the muscles of a limb grow and strengthen. From which it follows, that the size any individual of a species attains, depends firstly on the power to vary under stimulation inborn in his structures, and secondly, on the amount of appropriate stimulation to which the structures are subjected, whereby this power to vary is called into activity; the structures in the limbs of an infant, for example, grow into an adult limb under appropriate stimulation, that of use; but if the stimulation be withheld, the limbs remain infantile, nay, if stimulation be withheld absolutely, even the infantile standard of development is not maintained, and the more active tissues, those most largely composed of living protoplasm, atrophy, and tend to disappear. Similarly the more active structures in an adult limb tend to disappear on withdrawal of stimulation. Therefore, not only has the action of Natural Selection caused the evolution in high animal organisms of complex and heterogeneous structures, whereby they are brought into harmony with a complex and heterogeneous environment, and not only has it endowed these structures with the power of developing proportionately and co-adaptively under appropriate stimulation; but yet more to increase the harmony with the environment, it has endowed them with the power of retrogressing in the absence of appropriate stimulation, whereby the organism is relieved wholly or partly from useless and burdensome structures. It should be observed also, that any structure, say a bone, is itself compounded of parts, which, like the structure as a whole, severally vary under stimulation; wherefore if the stimulation under which the parts develop be not proportionately the same as regards each of them as that under which they normally develop, then their development will be unequal, and the shape of the structure as a whole will be abnormal; for instance, if the muscles attached to one part of a bone be exercised more in proportion than the muscles attached to other parts of it, that part of the bone to which they are attached, in response to the extra stimulation, will develop more than the other parts, and as a result the shape of the bone as a whole will be abnormal; similarly, if the muscles attached to one part of a bone be exercised less in proportion than the muscles attached to other parts of it, that part of the bone to which they are attached, because less stimulated, will develop less than the other parts, and as a result, the shape of the whole bone will again be abnormal.

In higher species the structures of each individual receive much the same kind and quantity of stimulation, direct and indirect, from the environment, and are possessed of much the same power of varying in response to stimulation, as the structures of the other individuals of the species; as a result corresponding structures in different individuals in corresponding stages of the ontogeny closely resemble one another not only in general form, but also in size. Now since the structures develop mainly in response to stimulation, their development in the later stages of the ontogeny, at any rate, can only be regarded as acquired, not inborn: whence, if it be true that acquired variations are not transmissible, it follows that the later development cannot be transmitted; which is in accordance with the facts observed, for in each individual that later development must be acquired anew in response to stimulation, and cannot be achieved without it. That which is transmitted is the power to vary in response to stimulation, and it is inconceivable that this power can have resulted from aught but the accumulation of inborn variations.

It follows, moreover, that the adult individuals of any higher species differ among themselves as regards their structures, mainly through differences in this inborn power to vary in response to stimulation. Their structures are similar, but they do not develop to the same extent, or in the same proportion. Individuals differ in size mainly because there is inborn in some or all the structures of the bigger individuals, a greater power to vary in response to stimulation, than there is inborn in the structures of the smaller individuals; or, to a much less extent, they differ in size because the structures of the bigger individuals have received more stimulation than the structures of the smaller individuals: they differ in shape mainly because of differences in the power the structures had to vary proportionately; or to a much less extent, they differ in shape because the stimulation received by the structures is disproportionate.

Continuing the above line of argument, it follows that specific evolution in higher animals as regards size without change of shape, is due mainly to an evolution of the power to vary in response to stimulation proportionately in all the structures. Specific evolution as regards shape, whereby change of form is brought about, is mainly due to a disproportionate evolution in the structures of the power to vary in response to stimulation; thus as regards the giraffe, Natural Selection has resulted in the response to stimulation being greater in the structures of the fore than of the hind limbs, hence their greater size; whereas the reverse is the case as regards the kangaroo. In either case, notwithstanding the unequal evolution, there is perfect co-adaption of the parts—a co-adaption due in the individual to the strain which co-ordinated structures put on one another, whereby they are caused to develop proportionately; due in the species to the survival of those that possessed the power of varying co-adaptively, and the elimination of those which did not possess that power. In the giraffe the structures are much the same as in the kangaroo, that is, for each structure in the giraffe there is, generally speaking, a corresponding structure in the kangaroo, but the fully-developed animals differ vastly—differ not only as regards structural characteristics which are inborn, but also as regards structural characteristics which are acquired in the individual through stimulation acting during the ontogeny on the immense inborn power to vary in response along certain lines, the lines being different in the giraffe from what they are in the kangaroo.

It appears to me that the above considerations completely dispose of Mr. Spencer's objections. The co-adaption of animal parts is clearly due to, and maintained by, the inborn power the parts possess of developing in response to stimulation, so that the development or atrophy of any part, owing to increase or decrease of stimulation, is followed, owing to alterations in the strain put upon them, and therefore also to increase or decrease of stimulation, by the development or atrophy of all co-ordinated parts. It must be remembered that Mr. Spencer, while admitting that inborn variations are transmissible, attributes co-adaptive evolution to the accumulated effects of use or disuse, that is, he attributes it to the accumulation of those variations which are acquired in response to stimulation. But in the offspring these variations are not reproduced except in response to fresh stimulation, therefore, as regards any structure, that which is inherited is not the variation, but only the power to acquire the variation afresh in response to stimulation. Now there is no tittle of evidence tending to prove that stimulation increases the *power* of a structure to vary. The power to vary is inherent; it is called into activity by stimulation, but there is nothing to show that the inherent ability to vary is increased by stimulation. On the contrary, as animals grow older, their structures become less and less responsive to stimulation; the power of responding to stimulation in their long-stimulated structures actually declines, and the structures atrophy. Therefore as the power of varying is not increased or (presumably) decreased by stimulation or the lack of it, by use or disuse, it is clear, since variations of this power cannot be acquired, that acquired variations in it cannot be transmitted, and therefore if we hold that acquired variations are transmissible, we must not attribute the reproduction in the child of the parent's acquired variations to transmitted increase or decrease of this power.

But, as we see, variations can only be reproduced in the offspring by exercise of it, since without stimulation the variations do not appear. Therefore Mr. Spencer and those who follow him virtually hold that the acquirement of variations by the parent causes an increased power of varying in the offspring; *i.e.* that when one thing is acquired, another thing, which is not acquired and is therefore not transmitted, appears in the offspring.

CHAPTER V

The assumption that acquired traits are *not* transmissible is founded on the theory that a multicellular organism is compounded of unicellular organisms adherent together for the common benefit, or rather, as regards higher animals and plants, that a multicellular organism is compounded of unicellular organisms adherent for the better preservation of the race; which preservation is further subserved by the differentiation and specialization of the cells, the somatic cells being specialized for the performance of the various functions whereby the mass is brought into harmony with the environment, and the germ cells are placed at an advantage; the germ cells being so specialized that after conjugation they proliferate into organisms (*i. e.* adherent cell-communities) like to that of which they were cell-members. Thus a higher animal or plant is comparable to a swarm of bees, in which the workers correspond to the somatic cells, whereas the queens correspond to the germ cells; but there are these obvious differences, (1) that the workers cannot reproduce themselves, whereas the somatic cells are able to reproduce themselves for a limited, or in some cases for an unlimited, number of generations (*e. g.* in such plants as propagate by means of suckers or cuttings); and (2) that from the queens may be derived many swarms, whereas from the germ cell is derived only one organism. It is perhaps needless to remark, that the cells are not conscious agents, but that the evolution of multicellular organisms has resulted solely from the action of Natural Selection —from the natural selection of inborn variations alone— or, as Mr. Spencer holds, from the selection of inborn plus acquired variations.

If we hold, as I think we must, that the multicellular organism is really a compound being, a being compounded of unicellular organisms, then, as already explained, we must suppose that acquired variations are not transmissible, unless it can be shown that the reproductive cells receive from the somatic cells elements which so alter their constitutions as not only to cause them to proliferate into organisms different from those which would have otherwise arisen, but also into organisms which have inborn in them variations which were acquired in the cell-community of which the germ cells were members. This is the next question discussed by Mr. Spencer, but strangely enough he turns away from the actual point at issue, the question as to whether the elements (gemmules, physiological units, or what not) which are supposed to cause the change in the constitution of the germ cell, have any real existence; he assumes that they do exist, and appears to think that all he needs to prove is that, being existent, they are able to enter the germ cells, and this he does by pointing out that the microbes of disease as well as food molecules are able to enter them. He proceeds—

"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 the 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" (p. 32).

It is hardly necessary to argue the matter: of course if the gemmules or physiological units exist they must be able to enter the germ cells as do food molecules, as do microbes, and as do spermatozoa. This is not the thing denied. It is the very existence of the elements which cause the germ cells to proliferate into organisms with the acquired variations of the parent that is denied. The existence of gemmules or physiological units is assumed, that the transmission of acquired variations may be accounted for. There is no other evidence of their existence, except such as is furnished by the supposed transmission of such variations. But as I have just shown, acquired variations as regards higher animal organisms, to which alone Mr. Spencer directs his attention, are apparently not transmitted, but that only the power to vary in response to stimulation is transmitted; and variations in this power cannot be acquired in the sense that is increased by stimulation, by use, and decreased by the lack of it, by disuse. Therefore neither acquired variations nor an acquired power of varying can be transmitted; and therefore since there is nothing to show that gemmules or physiological units exist, it is vain until this is proved to discuss the possibility of their entrance into the germ cells. That is not, as I say, the point at issue.

But Mr. Spencer has another string to his bow. According to him, and in this he follows Mr. Adam Sedgwick, a so-called multicellular organism is not really a multicellular organism, but "a continuous mass of vacuolated protoplasm," a sort of gigantic unicellular organism.

He founds this opinion on some researches by the latter gentleman, which tend to show that cell-division is not absolute, but that cells are connected by "a fine protoplasmic reticulum, so that the alleged independence of the reproductive cells does not exist" (p. 41). Now as regards some cells, for instance nerve cells, it has long been known that they are connected by a fine protoplasmic reticulum, but until Mr. Sedgwick did it, it had never occurred to any one to call the brain for instance "a continuous mass of vacuolated protoplasm."

Nevertheless, let us concede the point, and agree, for the sake of argument, to regard a multicellular organism, *e. g.* an elephant, as a continuous mass of vacuolated protoplasm—as a sort of gigantic unicellular organism. How does the concession help Mr. Sedgwick? Does it aid him in proving, for example, that if changes are acquired in that part of the vacuolated mass which we call the elephant's trunk, that these changes will so affect that part of the vacuolated mass which we call the germ cell, that when the latter has developed into another elephant, there will spontaneously appear in the trunk of the descendant animal variations similar to those which were acquired by the parent—variations which arose in the parent in response to stimulation, and which arise anew in the offspring only in response to similar stimulation?

Mr. Sedgwick writes, and Mr. Spencer quotes—

"Finally, if the protoplasm of the body is primitively a syncytium, and the ovum until maturity a part of the syncytium, the separation of the generative products does not differ essentially from the internal generation 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. 40).

But how is the transmission of acquired characters thereby rendered less mysterious? Supposing an elephant exercises his trunk to such an extent that the muscles enlarge beyond the ordinary, do the molecular changes in this case differ essentially from the molecular changes which occur in the trunk of an elephant that uses his trunk to such an extent only that the muscles of it are maintained at the ordinary standard? And if they do differ, how can these molecular changes, spreading through the mass by way of many variously differentiated structures, which, being of different molecular constitutions, must be differently affected by the molecular changes, so affect the molecular constitution of the germ cell when they reach it, that it ultimately proliferates into an organism with enlarged muscles in the trunk? It must be remembered, that neither muscle cells nor any of the other kinds of tissue present in the trunk exist in the germ cell. These only appear in its very remote cell-descendants. If anything the mystery is deepened by Mr. Sedgwick's theory, and those who hold that acquired variations are transmitted will I think prefer Mr. Darwin's theory of gemmules, or Mr. Spencer's own theory of physiological units, as furnishing more probable explanations. Moreover, though Mr. Sedgwick may have established that other cells besides nerve cells are connected by fine protoplasmic processes, he has not as yet established that their nuclei are in any way affected by the connection; for example, he has not proved that the nuclei of the germ cells are in any way affected by the supposed partial continuity of their cell-bodies with the cell-bodies of adjacent somatic cells, and until this is done his researches, however interesting from a morphological point of view, are valueless as regards the matter under discussion, since modern research has rendered it practically certain that it is the nucleus, not the cell-body, that is the bearer of heredity.

To strengthen the case for the transmission of acquired variations, Mr. Spencer quotes from Lord Morton—

"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 George Ouseley, who has bred from her by a very fine black Arabian horse. I yesterday morning examined the produce, 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 forehand, and the dark bars across the back part of the legs" (p. 34).

Mr. Spencer quotes also the case of a sow, the offspring of which in the second and third litters exhibited traits characteristic of the father of the first, an animal which was drowned shortly after he had been put to the sow. He then remarks—

"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" (pp. 38-9).

The above passage is somewhat obscure. Its meaning, however, seems to be as follows:—Some of the germ-plasm of the first-formed reproductive cells passes into the somatic cells to the farthest limits of the organism. If then, any part of the organism acquires characters these are, by part of the germ-plasm there abiding, conveyed back to the germ cells subsequently formed, and thus acquired characters are transmitted. This Mr. Spencer thinks is proved by what he imagines is a fact, viz. that foreign germ-plasm is incorporated with the somatic cells, whence it enters reproductive cells subsequently formed, whereby the progeny of subsequent sires are infected with the characters of the first sire. If my interpretation is correct, it must be admitted that Mr. Spencer has here built a very pretty superstructure on remarkably slight foundations.

Professor Romanes (*Contemporary Review*, April 1894) in reply, suggests that it is possible that the immature ova are sometimes (*e.g.* in the cases cited) directly infected by the germ-plasm of the first male, a not improbable explanation, since we know that the spermatozoa do actually reach the ovaries, as is proved by many recorded cases of ovarian and abdominal fœtation. Mr. Spencer scouts his suggestion, and asks—

"Why in such a case each subsequent ovum, as it becomes matured, is not fertilized by the sperm cells present, or their contained germ-plasm, rendering all subsequent fecundations needless?" (p. 68).

Professor Romanes has, however, already by anticipation answered the objection by opining—

"That the life of ids" (*i.e.* groups of biaphors) "is not commensurate with that of their contained spermatozoa. After the latter have perished and disintegrated, their ids may escape in thousands of millions, bathing in a dormant state the whole surfaces of both ovaries. And if so, it is conceivable that when subsequent ova mature, *i.e.* come to the surface of their ovaries and rupture their follicles, these dormant ids adhere to their porous walls through which they may pass."

And therefore since the whole of a spermatozoon of a previous sire does not enter the ovum it is not entirely fertilized, and all his traits are not transmitted; but as only part of his spermatozoon enters, the ovum is only partially fertilized, and his traits are only in part transmitted. Mr. Spencer objects "that the surface of a mammalian ovarium is not a spermatheca" (p. 68), but when he speaks of "a foreign germ-plasm permeating the organism," does he not himself seem to consider the tissues in general of the female as a spermatheca? And if the tissues in general, why not the tissues on the surface of the ovary in particular? At any rate this is clear, that while there is not a particle of evidence to show that the female acquires and transmits traits from a previous sire, there is some evidence to show that her germ cells do.

We have now reviewed the whole of Mr. Spencer's articles so far as they deal with "The Inadequacy of Natural Selection," and taking the counter-arguments into consideration, I think he can hardly be said to have established his case. But in a foot-note (p. 33) he mentions a series of experiments which at first sight certainly prove, or seem to prove, that acquired traits may sometimes be transmitted. Brown-Sequard severed the sciatic nerve in some guinea-pigs; epilepsy thereupon supervened, and was apparently inherited by the offspring subsequently born, for they also were epileptic. Weismann's attempted explanation that certain hypothetical microbes entering the wound caused epilepsy in the parent, and infecting the unborn offspring, produced epilepsy after birth in them as well, appears to me improbable and far-fetched. However, Mr. Spencer himself does not attach very much importance to Brown-Sequard's experiments. He says of them in the pamphlet we have under consideration—"Let me say I do not commit myself to any derived conclusions;" and in a previous publication he says—

"Considerable weight attaches to a fact which Brown-Sequard discovered quite by accident in the course of his researches. He found that certain artificially-produced lesions of the nervous system, so small even as a section of the sciatic nerve, left after healing an increased excitability which ended in liability to epilepsy; and there afterwards came out the unlooked-for result that the offspring of guinea-pigs which thus acquired an epileptic habit, such that a pinch on the neck would produce a fit, inherited an epileptic habit of the kind. It has, indeed, been since alleged that guinea-pigs tend to epilepsy, and that phenomena of the kind described occur where there have been no antecedents like those in Brown-Sequard's case. But considering the improbability that the phenomena observed by him happened to be nothing more than phenomena which occasionally arise naturally, we may, until there is good proof to the contrary, assign some value to his results."—*The Factors of Evolution.* Williams and Norgate: 1887.

But if it is true that epilepsy usually supervenes in guinea-pigs when the sciatic is severed, and that this acquired epilepsy is generally or even occasionally inherited by the offspring, there appears to be strong grounds for supposing that the acquired variation in this case is transmissible. But before we could positively decide as to this we should have to still further extend our inquiry.

But here the reader must be reminded of that which was insisted on in a former page, namely, that in the last analysis all inborn variations in any organism must be due to acquired variations in the germ cell from which it arose. He must remember that it is not denied that a force acting on an organism may produce changes in its germ cells, and consequently in the descendant organisms; but that it is denied that the changes produced in the germ cell are usually such that in consequence of them they tend to grow into organisms in which appear changes similar to the changes produced by the force in the parent organism. He must remember that it is not asserted that a force acting on an organism cannot produce such a change in the germ as will cause the organism into which it develops to exhibit a variation similar to the variation produced by the force in the parent; but that it is asserted that this coincidence, this mere coincidence, must, from the nature of the case, be extremely rare—so very rare that, as factors in evolution, such apparent, but only apparent, transmissions of acquired traits may practically be ignored.

Epilepsy must depend on structural changes in nerve-matter, either gross changes, or molecular changes induced by alteration in the nutritive fluids, or by alterations in the nervous impulses received, or by spreading degenerative or inflammatory changes. If upon extended inquiry it were found that the structural changes in the nerve-matter of the offspring did not correspond to the structural changes in the nerve-matter of the parent, then this would be no true instance of the transmission of an acquired variation; for the epilepsy, though present in both parent and offspring, would be due to different causes. But if the structural changes in the offspring were found to be similar to those in the parent, it would have to be admitted that in this case an acquired variation had seemingly been transmitted; and the question would then arise as to the nature of the transmission, as to whether it was a true transmission of an acquired variation in the sense meant by those who contend that acquired variations are transmitted, *i. e.* as to whether it was transmitted because there is a tendency for acquired variations to be transmitted, owing to a power the organism possesses to so affect its germ cells that its acquired variations are reproduced in the offspring; or as to whether it was transmitted because this happened to be one of those very rare cases, those mere coincidences, in which the force which produced the acquired variation in the parent also produced directly or indirectly such changes in the germs, as caused in the organisms into which they proliferated inborn changes similar to the acquired changes in the parent.

This could only be determined by a further inquiry as to whether such clear and indubitable cases of reproduction by the offspring of the acquired variations of the parent, as the above, are common in nature. If it were found that they commonly occur, then the case for the transmission of acquired traits would be established beyond cavil, and we should have either to accept one of the current theories as to the way acquired variations are transmitted, or else to seek data whereon to found a theory which would present a greater appearance of probability than any of those yet formulated.

As a matter of fact, however, the phenomena of nature have already been ransacked for such clear cases of transmission, or apparent transmission, as the above, with the result that as regards the evolution of plants it is now admitted on all hands that the transmission of acquired variations can have played no part; as regards the higher animals alone is it generally claimed that such transmission has been important as a factor in evolution. Mr. Spencer says—

"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, characterized 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" (p. 45).

With the first part of this statement we may well agree, but there are strong grounds for demurring to the rest of it. So far from it being true that in the lower animal world acquired traits are not transmissible, it is precisely there, in the lowest, that they are distinctly transmissible. In that stratum of life which, in our imperfect knowledge, we at present regard as the lowest, where the plant and animal kingdoms merge, and whence both have arisen, every organism being unicellular is a germ cell, which, besides being extremely minute and relatively simple, is one on which external conditions act directly, whereby the constitution of the cell is so modified that a like variation is produced in the next generation. For this reason, as already indicated, bacteriologists, by means of their "attenuated cultures," are able to deeply modify the characteristics of microbic organisms. As regards higher animals, the high multicellular animals with which alone Mr. Spencer deals in his paper, nothing that he has said, and nothing that any one else has said, establishes a shadow of proof that in them acquired traits are transmissible—that the changes, which the action of the environment produces in their somatic cells, so affect their germ cells, that these latter, when they proliferate, reproduce in the new organism variations like to those which the action of the environment produced in the parent.

MENTAL EVOLUTION

CHAPTER I

THE older psychologists were not acquainted with the as yet undiscovered truths of evolution, and studied mind without their aid. Practically they dealt only with the fully developed thing as manifested in man. From that point of view, by "introspection," by observation of their own conscious states, they sought information regarding their own minds, and by "legitimate inferences" information regarding the minds of other men, and to a much less extent, information regarding the minds of lower animals. "Introspection" and "legitimate inferences" are still our only sources of information, but the "legitimate inference" that there is kinship between the human and the brute intellect, and that in the brute we may see the beginnings of that which we observe in man, enables us to make a better use of our available information. By observing mind at its beginning, and tracing it during its evolution, we are enabled to an extent undreamed of by former generations to understand of what it is compounded, to analyze it.

To Mr. Herbert Spencer beyond all others is due the credit of having applied the doctrine of evolution to the study of psychology, with the result that this science, which was formerly notoriously sterile, as Buckle observed, is now prolific in new achievement. But to this branch of biology, even more than elsewhere, has Mr. Spencer applied the doctrine that acquired variations are transmissible—that is to say, since mental evolution depends on structural evolution in the nervous system, he supposes that the nervous systems of man and other animals have arisen through the accumulation of acquired variations. It would be vain to discuss his writings in detail. The arguments already set forth against the theory that acquired traits are transmissible, apply in full force in this particular case. *À priori*, the transmission of acquired changes in nervous tissue seems impossible; it is unbelievable that acquired changes in nervous tissue can so affect the germ cell as to cause it after fertilization to proliferate into an organism with inborn variations similar to the acquired variations in the parent; *à posteriori*, though we frequently see psychical traits acquired, yet we never see acquired psychical variations transmitted, or so very rarely that such apparent transmissions may be set down as mere fortuitous coincidences.

The study of psychology is extraordinarily difficult, owing to the complexity and obscurity of the subject. We deal here with the intangible, non-material products of the functional activity of an organ, concerning which, in nearly all essential details, we are in ignorance. Though we are sure that every mental phenomenon has its physical side, yet we cannot express mind in terms of matter, and probably never shall be able to do so. Differences of structure in the nervous system, so minute as to be inappreciable to us, have commonly for their concomitants enormous psychical differences. Compare, for instance, the nervous systems on the one hand, and the mental traits on the other, of two allied species of insects, *e. g.* ants. Often, when the nervous systems appear precisely similar, we find that the mental traits as manifested in the actions of the animals differ vastly. Changes in nervous tissue seem magnified a thousandfold when examined through their psychical concomitants. But however obscure and difficult the subject, two facts may be made out: (1) *That acquired psychical changes are never transmitted, and* (2) *that, just as regards their corporal structures, so as regards their mental traits, the action of Natural Selection has been to develop in higher animals an immense power of varying, of individually acquiring variations, whereby the organism is brought into completer harmony with its complex environment.*

In animals low in the scale, such as the cœlenterates, almost all the reactions to stimulation from the environment are of the kind known as reflex. In them Natural Selection has brought about the evolution of particular reactions, whereby particular and generally oft-recurring events in the simple environment are provided against; the power of making these appropriate responses to particular stimuli being obviously inborn and transmissible. Higher in the scale occurs that kind of response to stimulation which is known as *Instinct,* and which Mr. Spencer has defined as "compound reflex action," but which Professor Romanes rightly insists is something more—is action into which has been imported the element of consciousness.

It may be defined as "*the faculty which is concerned in the conscious adaption of means to ends,*" *by virtue of inborn inherited knowledge and ways of thinking and acting.* For example, the young alligator or the young turtle instinctively seek the water on emerging from the egg, *i. e.* they seek it by virtue of their inborn and inherited knowledge and ways of thinking and acting. Instinct also is clearly transmissible.

Highest in the scale, and notably in man, occurs that kind of response to stimulation which is known as *Reason*, and which may be defined as "*the faculty which is concerned in the conscious adaption of means to ends," by virtue of acquired non-inherited knowledge and ways of thinking and acting.* Though powers of acquiring reason are transmissible, reason itself is obviously never transmitted.

I am aware that the above definitions of instinct and reason are very different from those ordinarily accepted, but I think it will be found on consideration that they are more accurate, that they more completely include within their limits all cases of instinct and reason respectively, and that they more clearly separate that which is instinctive from that which is rational than any other definition as yet advanced. Professor Romanes, for example, defines reason as—

> "The faculty which is concerned in the conscious adaption of means to ends. It therefore implies the conscious knowledge of the relation between means employed and ends attained, and may be exercised in adaption to circumstances novel alike to the experience of the individual and to that of the species."—*Mental Evolution,* p. 318.

But it appears to me that reason so defined includes nearly all those actions which we commonly term instinctive. For instance, by what term shall we designate the action of the spider when he builds his web? Does the animal not know for what purpose he constructs it? Was there ever a web building in which there were not "circumstances novel alike to the experience of the individual and to that of the species"? Or, when he runs along a thread to capture his prey, or cuts loose a dangerous captive, does he not consciously adapt means to ends, just as much as a man who runs to secure a snared bird, or who builds a "golden bridge" for a flying enemy? I think that Professor Romanes would have designated the actions of the spider as instinctive, and the actions of the man as rational. Yet, in what respect do they differ? Only in that the actions of the spider result from knowledge which is inborn and inherited, whereas the actions of the man result from knowledge which is neither inborn nor transmissible, but has been acquired. Again, in what particular does the action of the young alligator, that seeks the water and snaps at an opposing walking-stick, differ from the actions of the man who seeks a refuge and strikes at an intervening enemy? Only, I think, in that the one acts by virtue of inborn transmitted knowledge, whereas the other acts by virtue of acquired, non-transmissible knowledge. If we take the words "exercised in adaption to circumstances, novel alike to the experience of the individual and to that of the species," as separating that which is instinctive from that which is rational, by what term shall we designate the action of a man who speaks, or even of one who builds a hut, actions novel neither to the experience of the individual nor to that of the species? Wherein do such actions differ from those of a bird, which utters cries of rage or fear or warning, or which builds a nest? Once again, only, in that the man acts by virtue of acquired, non-inherited knowledge, whereas the bird acts by virtue of knowledge which is inborn and inherited.

In very low animals, such as the cœlenterates, almost all actions are apparently reflex. Higher in the scale reflex action becomes manifestly associated with instinct. Higher yet, reflex action and instinct become associated with reason. But though reflex action and instinct successively lose their positions of commanding importance as factors of survival, yet even in the highest animals, *e. g.* in man, for whom the environment is most complex and heterogeneous, both are present as essential factors of survival. In them reflex action by means of particular reactions still provides against particular and generally oft-recurring events, of such a kind as, if not provided against, would result in the destruction of the organism or of the species. As examples of such reflex actions present in man may be quoted, the movements of various hollow viscera, the acts of breathing, swallowing, and coughing, and in infants, that of sucking. Instinct is represented by various emotions, the existence of which is necessary for the preservation of the individual, or of the race, *e. g.* sexual and parental love, love of life, jealousy, rage, fear, hate, &c.[1] Generally speaking, reflex action and instinct provide reactions against such events as invariably happen to every individual, and are of such a nature as, if not provided against by unfailing machinery, would lead to destruction of the individual, or at any rate to cessation of the race; and this machinery, as I have said, is obviously inborn and transmissible.

But reason is obviously neither inborn nor transmissible. It is obviously acquired anew by every individual, only the power of acquiring it being inheritable; and the extent to which it is acquired in each organism depends (1) on the organism's power of varying, of

[1] Sexual and parental love, fear, and love of life are clearly necessary for the preservation of the race or of the individual, on whose preservation depends that of the race. As regards hate, that individual who has not the instinct is less likely to survive and have offspring than the individual who, having it, seeks to injure or destroy those that seek to injure and destroy him; so also with the instinct of rage; as regards jealousy, an individual, who views with indifference the advances made to his mate or mates by individuals of his own sex, is likely to have a smaller share in continuing the race than an individual who resents such advances.

developing mentally in response to appropriate stimulation, and (2) on the amount of appropriate stimulation supplied—as is clearly the case in man, in whom beyond all other animals there has been an evolution of this power.

In higher animals therefore the power of individually acquiring reason, of varying, of developing mentally, in response to appropriate stimulation, is strictly analogous to their power of individually acquiring corporal traits, of varying, of developing physically in response to appropriate stimulation. But just as in lower animals, such as the sponges and cœlenterates, the physical structures develop in the absence of all stimulation other than that of sufficient food, so also do such mental traits as they exhibit, reflex action and even instinct, develop in higher animals in the absence of all other stimulation; and therefore the development of the reflexes and instincts is strictly analogous to the development of the physical structures of lower animals; that is, appropriate stimulation does not cause variations and developments in reflexes and instincts, but merely calls pre-existing reflexes and instincts into activity; they develop quite apart from stimulation.

Reflexes and instincts, like the structures of lower animals, and like what I may call the groundwork of many of the structures of higher animals, then, have been developed by Natural Selection to provide reactions against actions invariably occurring in the simple environment; but reason, like that part of the physical development of higher animals which is achieved only as a reaction to stimulation, is achievable only as a reaction to appropriate stimulation acting on the inborn power to vary, to develop mentally; by means of it, as by means of their variable physical structures, higher animals are brought into completer harmony with a variable and complex environment, in which occur events too numerous and heterogeneous to be provided against by an unvarying machinery.

The entire passage from reflex action in its lowest manifestations, to reason in its highest manifestations, is therefore a process of increasing adaption to environments increasing in complexity. Low in the animal scale a few heterogeneous structures and reflexes provide against the few heterogeneous events that occur in the simple environment. Higher in scale the greater heterogeneity of the events to be provided against is met by the evolution, through Natural Selection, of a greater number of heterogeneous structures and reflexes. Higher still, when the environment becomes yet more complex, reflex action is supplemented by instinct, which, though undeviating in its promptings, is associated with consciousness and controlled to some extent by volition, whereby the element of choice is introduced, and the adaptability of the organism to its environment vastly increased. Highest of all, increasing still more the adaptability, when the events in the environment to be provided against have become so heterogeneous and multitudinous that by no evolution of new physical structures, accompanied by new reflexes and instincts, can they be provided against, there occurs that physical variability, in adaption to circumstances, to which I have so persistently called attention, and that accompanying mental variability on which I as strongly insist, and which we call reason.

Now there is no vestige of proof that instincts are increased by stimulation, *i. e.* that they are not only called into activity by stimulation, but are sharpened by it. Indeed, if our definitions are correct, such a supposition involves a contradiction in terms, since whatever is mentally acquired pertains to reason, not to instinct. Again, it is obvious that reason is not transmissible, since in each generation it must be acquired anew in response to stimulation, only the *power* of acquiring it being transmissible. As regards this power also, just as regards instinct, there is not a vestige of proof that it is increased by stimulation, by use. On the contrary, it is a matter of common knowledge, that as animals grow older their long stimulated powers of acquiring mental traits become more feeble and tend to cease. The conclusion we are forced to arrive at, then, is this—that since whatever mental that is transmissible (instincts and powers of acquiring reason) does not vary under stimulation, therefore no acquired mental traits can be transmitted, for no transmissible mental traits can be acquired—an *à priori* conclusion capable, as will be seen, of ample *à posteriori* confirmation.

It may be objected that, since reflexes and instincts appeared earlier in the phylogeny than reason, they ought to appear earlier in the ontogeny, whereas some reflexes and instincts, *e. g.* those pertaining in man to the sexual functions, appear later in the ontogeny than reason. The answer to this is threefold: (1) some reflexes and instincts which appear late in the ontogeny, without doubt have had the period of their appearance in it shifted by the action of Natural Selection—have been removed to a later date by the accumulation of favourable variations; (2) others again may exist in a latent state from an early period, not becoming apparent till stimulation is supplied, which stimulation is not supplied till late in the ontogeny; (3) others again may not have been developed till late in the phylogeny, just as some structures (*e. g.* horns and teeth), which do not vary under stimulation, were not developed till late in the phylogeny.

In defining instinct as "the faculty which is concerned in the conscious adaption of means to ends by virtue of inborn inherited knowledge and ways of thinking and acting," and reason as "the faculty which is concerned in the conscious adaption of means to ends by virtue of acquired non-inherited knowledge and ways of thinking and acting," it will be perceived that I have sharply marked off all that is instinctive from all that is rational, leaving no border-space where the one merges into the other. In this, however, I believe I am at variance with all other writers who have dealt with the question from the standpoint of evolutionists. These, with whom alone we are here concerned, generally derive the one faculty from the other, in which case there must of course be a border-space. "Spencer regards instinct as 'compound reflex action, and the precursor of intelligence' (*i.e.* reason), while Lewes regards it as 'lapsed intelligence,' and therefore necessarily the successor of intelligence. Thus while Lewes maintains that all instincts must originally have been intelligent, Spencer maintains that no instinct need ever have been intelligent." Professor Romanes, from whom I have quoted, is in partial agreement and disagreement with both Mr. Lewes and Mr. Spencer, thinking that in some cases the one is right, and in some cases the other. All three authors base their theories on the assumption that acquired mental variations are capable of transmission and therefore of accumulation.

It is a matter of common experience that the performance of any complex action becomes more easy for frequent repetition, till, if the action be repeated frequently enough, the performance of it becomes automatic; that is, the performance of it is accompanied less and less by a sense of mental effort, till at length no sense of such effort is present in consciousness. Thus we learn to walk, to speak, to read, to write with difficulty, but in time constant practice makes these complicated actions so easy that we perform them with scarcely any, if any, sense of mental effort. In common but erroneous parlance, we perform them instinctively. Mr. Lewes supposes that some of this acquired facility in performance is bequeathed by successive parents to successive offspring, whereby it is accumulated during generations to such an extent that the remote descendant has inborn the facility which the remote ancestor acquired only with effort and difficulty. In this manner, according to him, do actions, at first intelligent and accompanied with mental effort, become ultimately mechanical and instinctive, passing first through a border-space where they are neither quite instinctive nor altogether intelligent, but partake of the nature of both.

But setting aside for a moment the conclusion we have arrived at, that acquired traits are not transmissible, this theory totally fails to account for the formation of many important instincts. For instance, many insects at the ends of their lives lay their eggs in a particular place, and in a particular way, some in such a manner as to cause the aggregate of eggs to resemble an inedible substance, *e.g.* a twig. The action is performed only once, and at a time when the eggs have ceased in any true sense to be integral portions of the parent organism. Under the circumstances, not only can no facility in performance be acquired by the individual, but, even were it acquired, none could be transmitted. Yet Mr. Lewes' theory of the formation of instinct presupposes both the acquirement of facility and the transmission of it. Therefore, while it is conceivable that this instinct arose by the survival of the fittest during a severe process of Natural Selection, it is quite inconceivable that it should have arisen through a lapsing of intelligence. Moreover, were it true that instincts had such origins as Mr. Lewes supposes, they should be most numerous and best developed in higher animals, and intelligence

—*i. e.* reason—in the lower, whereas the reverse is the case; for in the highest animals—*e. g.* man—reason predominates, and instinct is at a minimum, while in lower animals—*e. g.* fish—instinct predominates, and reason is at a minimum. Very plainly, therefore, in higher animals there has been retrogression, not evolution, as regards instinct. Again, instinct and reason do not merge at any point, as they would were one derived from the other. The fact that one is inborn and the other acquired separates them sharply, and if, in the case of any given action, which is the outcome both of instinct and reason, as, for instance, a man's choice of a wife, we are unable to say how much of it is instinctive, and how much rational, our difficulty is due to our ignorance, not to there being no dividing line, but a border-space.

According to Mr. Spencer, "rational action arises out of instinctive action when the latter grows too complex to be perfectly automatic" (*Principles of Psychology*, vol. i. p. 458). His hypothesis, like Mr. Lewes', necessarily involves the supposition that there is a border-space where the instinctive and the rational merge. We have seen that he defines instinct as compound reflex action. In his view, therefore, rational action is reflex action still more highly compounded. Concerning instinct he says—

"In its higher forms, instinct is probably accompanied by a rudimentary consciousness. There cannot be co-ordination of many stimuli without some ganglion through which they are all brought into relation. In the process of bringing them into relation, this ganglion must be subject to the influence of each—must undergo many changes. And the quick succession of changes in the ganglion, implying as it does perpetual experiences of differences and likenesses, constitutes the raw material of consciousness. The implication is, that as fast as Instinct is developed, some kind of consciousness becomes nascent."—*Principles of Psychology*, vol. i. pp. 434-5.

It seems, therefore, that he regards consciousness as an accidental accompaniment of instinct (and presumably of reason also), not as an essential part of it. But some instinctive (and rational[1]) actions which result from consciousness, such as winking the eye when in fear of injury, are exceedingly simple, whereas some reflex actions, which do not result from consciousness, are highly complex. "No one thinks of sneezing, or the convulsions produced by tickling, as examples of instinctive actions. Yet they are compound reflex actions to a degree of compounding not easily paralleled, and certainly much more so than any of the non-psychical adjustments which are given by Mr. Spencer as illustrations of instinct."[2] So also some rational actions are very simple, whereas some instinctive actions are highly complex—*e. g.* web-spinning by spiders. Rational action cannot therefore be correctly described as arising out of "instinctive action which has grown too complex to be perfectly instinctive." Moreover, as I say, instinct does not merge into reason, but is sharply divided from it; there is therefore no more cause to suppose, as Mr. Spencer does, that reason arises out of instinct, than for supposing, as Mr. Lewes does, that instinct arises out of reason.

It is to be noted also that instinctive action is as sharply divided from reflex action by the fact that it results from consciousness, as it is divided from rational action by the fact that it is due to inborn, not acquired, knowledge and ways of thinking and acting. Here also

[1] Winking the eye is probably instinctive in the chick; it is not so in man, for a new-born infant does not wink when the hand is suddenly brought near its eye.

[2] *Mental Evolution*, Romanes.

there is no border-space, where the one merges into the other, but a sharply dividing line. The truth, therefore, appears to be, that reflex action, instinct, and reason are not derived the one from any other, but that each one is distinct from and has arisen independently of the others—is not a more or less complex form of the others. Concerning the latter two faculties, I think we have no choice but to believe that, when, by the action of Natural Selection during the phylogeny, a nervous system was evolved, then, in consequence of the high compounding of reflex action, another and totally distinct faculty, instinct, was evolved, and was superimposed on reflex action, the evolution of it being rendered possible by the developed state of the nervous system. But this faculty was an entirely new thing, as was also reason, which was subsequently evolved, its evolution being rendered possible by a still greater development of the nervous system. To illustrate the subject, however faultily, we may compare the evolution of the nervous system with these three faculties to the evolution of the fore limbs of mammals with three faculties which may be possessed by them. Reflex action may be compared to locomotion, to which the limbs were primarily devoted; instinctive action to the power of delivering blows with the fore limbs as in bears, a thing totally distinct from locomotion, yet only possible to the fore limbs in consequence of the evolution of the organs of locomotion; rational action to the power of carrying objects, as in man—a thing distinct both from locomotion and the delivering of blows, yet only possible because of still further evolution in the organs which subserve locomotion and the delivering of blows.

CHAPTER II

THOUGH reflex action and instinct persist in higher animals, yet reason does not merely supplement them; to a great extent it supplants them, and the extent to which it does so is greater the higher placed the animal is in the scale of life. We are in almost complete darkness as to the mental processes, if I may here use the term, of such low animals as the cœlenterates, but it is highly improbable that any of their actions, except to a slight extent in the highest members of the group, are instinctive, and wildly improbable that any of them are rational. The late Professor Romanes, to whose works I am greatly indebted, says—

"Some species of medusæ—notably *Sarsia*—seek the light, crowding into the path of a beam, and following it actively if moved. They derive advantage from so doing, because certain small crustaceæ on which they feed likewise crowd into the light."—*Animal Intelligence*, p. 23.

Professor Romanes considers this merely a reflex action, but it seems at least debatable whether it is not a "reflex action into which there has been imported the element of consciousness," whether, in fact, it is not an instinctive action. The following, however, is clearly a case of instinct.

"It was said by the late Dr. Robert Ball, that when the common *Sargartia parasitica* is attached to a stone, and a hermit crab is placed in its vicinity, the anemone will leave the stone and attach itself to the hermit's shell."—*Animal Intelligence*, p. 234.

Instinct is certainly present in worms, as Darwin proved.

"Seeing that they always lay hold of the part of the leaf (even though an exotic one) by the traction of which the leaf will offer least resistance to being drawn down."—Quoted by Professor Romanes in *Animal Intelligence*, p. 24.

"This animal is of a timid disposition, darting into its burrow like a rabbit when alarmed."—Quoted by Professor Romanes in *Mental Evolution in Animals*, p. 344.

Instinct is certainly present also in the molluscs, and, as every one knows, it is developed to an astonishing degree in the annulosa—*e. g.* insects; and here also may we detect the first glimmering of reason, if my definition of it as "the faculty which is concerned in the conscious adaption of means to ends by virtue of acquired non-inherited knowledge and ways of thinking and acting" be correct.

"Even the headless oyster seems to profit from experience, for Dicquemase (*Journal de Physique*, vol. xxviii. p. 244) asserts that oysters taken from a depth never uncovered by the sea, open their shells, lose the water within, and perish; but oysters taken from the same place and depth, if kept in reservoirs, where they are occasionally left uncovered for a short time, and are otherwise incommoded, learn to keep their shells shut, and then live for a much longer time when taken out of the water."—*Animal Intelligence*, p. 25.

"These animals (snails) appear also susceptible of some degree of permanent attachment. An accurate observer, Mr. Lonsdale, informs me that he placed a pair of land-snails (*Helix pomatia*), one of which was weakly, into a small and ill-provided garden. After a short time the strong and healthy individual disappeared, and was traced by its track of slime over a wall into an adjoining well-stocked garden. Mr. Lonsdale concluded that it had deserted its sickly mate; but after an absence of twenty-four hours it returned, and apparently communicated the result of its successful exploration, for both then started along the same track, and disappeared over the wall."—*Descent of Man*, pp. 262, 263, quoted by Professor Romanes in *Animal Intelligence*, p. 27.

Both the oyster and the snail appear capable of acting, in however limited a degree, by virtue of acquired knowledge. That some insects possess this power to a great extent is evident from the fact that enslaved ants acquire mental traits vastly different from those exhibited by the free members of the same species. It should be noted that adult ants are never enslaved, the pupæ alone being carried off, so that whatever traits the slaves acquire are acquired under the tuition of their masters.

"When the pupæ hatch out in the nest of their captors, the young slaves begin their life of work, and seem to regard their master's home as their own; for they never attempt to escape, and they fight no less keenly than their masters in defence of the nest. *F. sanguinea* content themselves with fewer slaves than the *F. rufescens;* and the work that devolves upon the slaves differs according to the species which has enslaved them. In the nest of *F. sanguinea* the comparatively few captives are kept as household slaves; they never either enter or leave the nest, and so are never seen unless the nest is opened. They are then very conspicuous from the contrast which their black colour and small size present to the red colour and much larger size of *F. sanguinea.* As the slaves are by this species kept strictly indoors, all the outdoor work of foraging, slave-capturing, &c., is performed by the masters; and when for any reason a nest has to migrate, the masters carry their slaves in their jaws. *F. rufescens,* on the other hand, assigns a much larger share of labour to the slaves, which, as we have already seen, are present in much larger numbers to take it. In this species the males and fertile females do no work of any kind; and the workers, or sterile females, though most energetic in capturing slaves, do no other kind of work. Therefore the whole community is absolutely dependent upon its slaves. The masters are not able to make their own nests or to feed their own larvæ. When they migrate, it is the slaves that determine the migration, and, reversing the order of things that obtains in *F. sanguinea,* carry their masters in their jaws. Huber shut up thirty masters without a slave, and with abundance of their favourite food, and also with their own larvæ and pupæ as a stimulus to work; but they could not feed themselves, and many died of hunger. He then introduced a single slave, and she at once set to work, fed the surviving masters, attended to the larvæ, and made some cells.

"In order to confirm this observation, Lespès placed a piece of sugar near a nest of slave-makers. It was soon found by one of the slaves, which gorged itself and returned to the nest. Other slaves then came out and did likewise. Then some of the masters came out, and, by pulling the legs of the feeding slaves, reminded them that they were neglecting their duty. The slaves then immediately began to serve their masters with the sugar."—*Animal Intelligence,* pp. 65-6.

It is extremely doubtful whether the lowest vertebrates have any power of acquiring mental traits, of varying mentally so as to place themselves in greater harmony with the environment. But, a little higher in the scale, reason certainly appears, and gradually overshadows reflex action and instinct, its growth being synchronous and related to that of the higher portions of the brain, the cerebrum and the cerebellum, especially the former. The lower vertebrates, like the lower insects, and unlike the higher vertebrates and insects (*e. g.*) ants and bees, do not generally tend their young, and therefore can teach them nothing. By virtue of a knowledge which is inborn and hereditary, and which cannot possibly have been acquired, they deposit their eggs under circumstances favourable for survival (*e. g.* most fish), and by virtue of a similar knowledge, the young when they emerge from the egg are fully equipped mentally for the battle of life. Some fish (*e. g.* sticklebacks), however, protect their young for a few days after they are hatched, during which time the latter may acquire from their parents such traits as watchfulness against enemies, knowledge of shelter, &c. That little is acquirable by fish is known to every keeper of an aquarium, but that they have some small power of mentally varying, so as to place themselves in harmony with an environment which has become more complex, is proved by such facts as that they may be tamed to a limited extent, and that the wariness of trout increases in a much-fished stream.

Batrachians show distinct powers of acquiring mental traits. Many frogs and toads have been tamed. The following is an extreme case.

"I used to open the gate in the railings round the pond, and call out 'Tommy' (the name I had given it), and the frog would jump out from the bushes, dive into the water, and swim across to me—get on my hand sometimes. When I called 'Tommy,' it would nearly always come, whatever the time of day, though it was only fed after breakfast; and it seemed quite tame."—*Animal Intelligence*, p. 255.

In this case the instinctive knowledge which impelled the frog to fly from a human being was quite overcome by the rational knowledge, which dispelled its fear of its mistress. The cerebral lobes of the brain are relatively larger in frogs than in fishes, but in the higher reptiles there is a still greater relative and absolute increase of size in these structures, and in them also are developed greater powers of reason, as the following examples prove.

"But a most singular instance of attachment between two animals, whose natures and habits were most opposite, was related to me by a person on whose veracity I can place the greatest reliance. He had resided for nine years in the American States, where he superintended the execution of some extensive works for the American Government. One of these works consisted in the erection of a beacon in a swamp in one of the rivers, where he caught a young alligator. This animal he made so perfectly tame that it followed him about the house like a dog, scrambling up the stairs after him, and showing him much affection and docility. Its great favourite, however, was a cat, and the friendship was mutual. When the cat was reposing herself before the fire (this was at New York), the alligator would lay himself down, place his head upon the cat, and in this attitude go to sleep. If the cat was absent the alligator was restless, but he always appeared happy when the cat was near him. The only instance in which he showed any ferocity was in attacking a fox, which was tied up in the yard. Probably, however, the fox resented some playful advances which the other had made, and thus called forth the anger of the alligator. In attacking the fox he did not make use of his mouth, but beat him with so much severity with his tail, that, had not the chain which confined the fox broken, he would probably have killed him. The alligator was fed on raw fish, and sometimes with milk, for which he showed a great fondness. In cold weather he was shut up in a box, with wool in it; but having been forgotten one frosty night, he was found dead in the morning. This is not, I believe, a solitary instance of amphibia becoming tame, and showing a fondness for those who have been kind to them. Blumenbach mentions that crocodiles have been tamed; and two instances have occurred under my own observation of toads knowing their benefactors, and coming to meet them with considerable alacrity."—*Animal Intelligence*, p. 258.

"In one of these cases the tortoise would come to the call of the favoured person, and when it came would manifest its affection by tapping the boot of this person with its mouth; 'but it would not answer any one else.' A separation of some weeks did not affect the memory of this tortoise for his friend."—*Ibid.* p. 259.

"I happen to know the gentleman and lady against whom a complaint has been made because of the snakes they keep, and I should like to give a short account of my first visit to them.

"Mr. M——, after we had talked for a little time, asked if I had any fear of snakes, and after a timid 'No, not very,' from me, he produced out of a cupboard a large boa-constrictor, a python, and several small snakes, which at once made themselves at home on the writing-table among pens, ink, and books. I was at first a good deal startled, especially when the large snakes coiled round and round my friend, and began to notice me with their bright eyes and forked tongues; but soon, finding how tame they were, I ceased to feel frightened. After a short time Mr. M—— expressed a wish to call Mrs. M——, and left me with the boa deposited on an arm-chair. I felt a little queer when the animal began gradually to come near, but the entrance of my host and hostess, followed by two charming little children, put me at my ease again. After the first interchange of civilities, she and the children went at once to the boa, and, calling it by the most endearing names, allowed it to twine itself most gracefully round about them. I sat talking for a long time, lost in wonder at the picture before me. Two beautiful little girls with their charming mother sat before me with a boa-constrictor (as thick round as a small tree) twining playfully round the lady's waist and neck, and forming a kind of turban round her head, expecting to be petted and made much of, like a kitten. The children over and over again took its head in their hands and kissed its mouth, pushing aside its forked tongue in doing so. The animal seemed much pleased, but kept turning its head continually towards me with a curious gaze, until I allowed it to nestle its head for a moment up my sleeve. Nothing could be prettier than to see this splendid serpent coiled all round Mrs. M——, while she moved about the room, and when she stood to pour out coffee. He seemed to adjust his weight so nicely, and every coil with its beautiful marking was relieved by the black velvet dress of the lady. It was long before I could make up my mind to end the visit, and I returned soon after with a friend (a distinguished M.P.) to see my snake-taming acquaintance again. . .

"These (the snakes) seemed very obedient, and remained in their cupboard when told to do so.

"About a year ago Mr. and Mrs. M—— were away for six weeks, and left the boa in charge of a keeper at the Zoo. The poor reptile moped, slept, and refused to be comforted, but when his master and mistress appeared he sprang upon them with delight, coiling himself round them, and showing every symptom of intense delight." —Severn, a distinguished artist, in a letter to the *Times*, quoted by Professor Romanes. *Ibid.* pp. 260-1.

We see, then, that animals so low in the scale of life as oysters, snails, and insects exhibit some power of acquiring mental traits, of varying mentally in response to stimulation, so as to place themselves in harmony with an environment which has become more complex, and that higher in the scale this power, of which reason is the outcome, increases more and more, till in amphibians and reptiles, the traits acquired by virtue of it sometimes contradict and overpower the dictates of instinct. But even the highest reptiles, with few exceptions, are provided with an equipment of reflexes and instincts sufficient to enable them to enter on the battle of life unaided immediately on emerging from the egg. It is far otherwise with birds and mammals, which, to an increasing extent, as they are higher placed in the scale, depend for survival less and less on instinct, on inborn inherited knowledge and ways of thinking and acting, and more and more on reason, on acquired knowledge and ways of thinking and acting; and therefore to an increasing extent are helpless and unfit for the battle when hatched or born, and for an increasing length of time are protected by, and receive tuition from, one or other of their parents, whereby they acquire such knowledge and ways of thinking and acting as enable them to enter on the battle with advantage.

In the lower birds and mammals, in which the cerebrum is least developed, instinct still predominates over reason. A young chick, for instance, emerges from the egg the possessor of a large amount of hereditary knowledge,[1] supplemented later by an amount of acquired

[1] "The late Mr. Douglas Spalding, in his brilliant researches on this subject, has not only placed beyond question the falsity of 'that all the supposed examples of instinct may be nothing more than cases of rapid learning, imitation, or instruction,' but also proved that a young bird or mammal comes into the world with an amount and a nicety of ancestral knowledge that is highly astonishing. Thus, speaking of chickens, which he liberated from the egg and hooded before their eyes had been able to perform any act of vision, he says that on removing the hood after a period varying from one to three days, 'almost invariably they seemed a little stunned by the light, remained motionless for several minutes, and continued for some time less active than before they

knowledge, small as compared to the acquired knowledge accumulated in subsequent life by such a bird as a

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were unhooded. Their behaviour, however, was in every case conclusive against the theory that the perceptions of distance and direction by the eye are the result of experience, or of associations formed in the history of each individual life. Often at the end of two minutes they followed with their eyes the movements of crawling insects, turning their heads with all the precision of an old fowl. In from two to fifteen minutes they pecked at some speck or insect, showing not merely an instinctive perception of distance, but an original ability to judge, to measure distance, with something like infallible accuracy. They did not attempt to seize things beyond their reach, as babies are said to grasp at the moon; and they may be said to have invariably hit the objects at which they struck—they never missed by more than a hair's breadth, and that too when the specks at which they aimed were no bigger, and less visible, than the smallest dot of an i. To seize between the points of the mandibles at the very instant of striking seemed a more difficult operation. I have seen a chicken seize and swallow an insect at the first attempt; most frequently, however, they struck five or six times; lifting once or twice before they succeeded in swallowing their first food. The unacquired power of following by sight was very plainly exemplified in the case of a chicken that, after being unhooded, sat complaining and motionless for six minutes, when I placed my hand on it for a few seconds. On removing my hand the chicken immediately followed it by sight backward and forward, and all round the table. To take, by way of example, the observations in a single case a little in detail:—A chicken that had been made the subject of experiments on hearing, was unhooded when nearly three days old. For six minutes it sat chirping and looking about it; at the end of that time it followed with its head and eyes the movements of a fly twelve inches distant; at ten minutes it made a peck at its own toes, and the next instant it made a vigorous dart at the fly, which had come within reach of its neck, and seized and swallowed it at the first stroke; for seven minutes it sat calling and looking about it, when a hive bee coming sufficiently near was seized at a dart and thrown some distance, much disabled. For twenty minutes it sat on the spot where its eyes had been unveiled without attempting to walk a step. It was then placed on rough ground within sight and call of a hen with a brood of its own age. After standing chirping for about a minute it started off towards the hen, displaying as keen a perception of the qualities of the outer world as it was ever likely to possess in after life. It never required to knock its head against a stone to discover that there was "no road that way." It leaped over the smaller obstacles that lay in its path and ran round the larger, reaching the mother in as nearly a straight line as the nature of the ground would permit. This, let parrot or a jackdaw, which begins life with a comparatively slender equipment of instinct. So also a young

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it be remembered, was the first time it had ever walked by sight.'

"Further, 'When twelve days old one of my little *protegés*, while running about beside me, gave the peculiar chirr whereby they announce the approach of danger. I looked up, and behold a sparrow-hawk was hovering at a great height overhead. Equally striking was the effect of the hawk's voice when heard for the first time. A young turkey, which I had adopted when chirping within the uncracked shell, was on the morning of the tenth day of its life eating a comfortable breakfast from my hand, when the young hawk, in a cupboard just beside us, gave a shrill chip, chip, chip. Like an arrow the poor turkey shot to the other side of the room, and stood there motionless and dumb with fear, until the hawk gave a second cry, when it darted out at the open door right to the extreme end of the passage, and there, silent and crouched in a corner, remained for ten minutes. Several times during the course of that day it again heard these alarming sounds, and in every instance with similar manifestations of fear.'

"Again referring to young chickens, Mr. Spalding continues—'Scores of times I have seen them attempt to dress their wings when only a few hours old—indeed, as soon as they could hold up their heads, and even when denied the use of their eyes. The art of scraping in search for food, which, if anything, might be acquired by imitation—for the hen with chickens spends the half of her time in scratching for them—is nevertheless another indisputable case of instinct. Without any opportunities of imitation, when kept quite isolated from their kind, chickens began to scrape when from two to six days old. Generally, the condition of the ground was suggestive; but I have several times seen the first attempt, which consists of a sort of nervous dance, made on a small table.'

"In this connection I may here insert an interesting observation which has been communicated to me by Dr. Allen Thomson, F.R.S. He hatched out some chickens on a carpet, where he kept them for several days. They showed no inclination to scrape, because the stimulus supplied by the carpet to the soles of their feet was of too novel a character to call into action the hereditary instinct; but when Dr. Thomson sprinkled a little gravel on the carpet, and so supplied the appropriate or customary stimulus, the chickens immediately began their scraping movements.

"But to return to Mr. Spalding's experiments, he says:—'As an example of unacquired dexterity, I may mention, that on placing four ducklings a day old in the open air for the first time, one of them almost immediately snapped at and caught a fly on the wing. More interesting, however, is the deliberate art of catching flies practised by the turkey. When not a day and a half old I observed the young turkey already spoken of pointing its beak at lamb or a fawn, which is able to recognize and follow its dam immediately after birth, displays much more knowledge of the world than does a young dog, and vastly more than a new-born infant, which cannot even seek the breast; but the mental traits acquired subsequently by the lamb are small as compared to those acquired by the pup, and infinitely little as compared to those acquired by the infant. It is to be noted, however, that though the most intelligent animals are generally the most helpless at birth, yet their helplessness is not always due entirely to Cessation of Selection, whereby, owing to the substitution of reason for instinct as a chief factor in survival, the instincts are lapsed; but that reversed selection must often have played some part in the elimination of instinct, for often the helplessness at birth of many of the more intelligent animals is an advantage to them, either owing to the situations in which they are born, or to the manner in which their parents procure food. For instance, the kind of intelligence displayed by a young chick or lamb would be distinctly unfavourable to the survival of young jackdaws, parrots, or puppies. Nevertheless the general truth, that as reason increases instinct decreases, manifestly holds.

It follows as a corollary from the above, and as a proof of it, that animals in which reason predominates, which have little knowledge of the world at birth, but acquire much knowledge subsequently, must, when removed from the ancestral environment, display in a

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flies and other small insects without actually pecking at them. In doing this, its head could be seen to shake like a hand that is attempted to be held steady by a visible effort. This I observed and recorded when I did not understand its meaning. For it was not until after, that I found it to be the invariable habit of the turkey, when it sees a fly settled on any object, to steal on the unwary insect with slow and measured step until sufficiently near, when it advances its head very slowly and steadily till within an inch or so of its prey, which is then seized by a sudden dart.'"—Romanes' *Mental Evolution in Animals*, pp. 161-4.

new environment a greater divergence in mental traits from their ancestors than animals in whom instinct predominates. That this is true we have ample evidence. For example, a chicken or lamb, removed from the care of its mother and reared by hand, differs mentally more from the rest of its species than does a fish or a frog similarly reared, but not nearly so much as a parrot, jackdaw, or puppy, and immeasurably less than does an infant reared even by members of its own species, but of a different race, *e.g.* a child of savage parentage reared by civilized people. A fish, which starts life with such a large equipment of instinct, can never be tamed to the same degree as a chick or a lamb, which starts life with a smaller equipment; a chick or a lamb can never be taught the many things that a jackdaw or a dog are capable of learning; as to the extraordinary power of acquiring mental traits possessed by the human infant, which starts life with such a small equipment of instinct, it need not be dwelt on, as it is known to all.

Mental traits, like physical traits, when once acquired, are more or less persistent. By withholding appropriate stimulation, that of use, from the limb of an infant, *e.g.* by paralyzing it, we are able to prevent its development into an adult limb; so, by withholding appropriate stimulation from the mind of an infant, we are able to keep it in a more or less infantile condition, as has happened in the case of various individuals incarcerated from infancy in Eastern prisons for political reasons; but as, when once a corporal structure has attained its full development, we cannot reduce it to its infantile condition by withholding stimulation, or greatly alter it by altering the kind of stimulation; so, when once a mind, whether human or brute, has attained its full development, we cannot reduce it to an infantile condition, or greatly alter it, by withholding or by altering the stimulation. The acquired characteristics of the fully-developed mind, like those of the body, possess a certain fixity, which, since mind is in a sense the product of a physical structure, the brain, is exactly what was to be expected. Therefore it is that adult animals do not so readily change their acquired mental traits as young animals; therefore do slave-holding ants capture only the pupæ of the servile species; therefore do we usually fail in taming adult wild animals, except in some few cases (*e. g.* monkeys and elephants), when the power of acquiring mental traits is so great, that new traits may be superimposed even in the adult animal on traits previously acquired in youth; and therefore is the right training of young human beings so highly important.

It is within the knowledge of every one that many animals, like men, afford tuition to their offspring, that they endeavour to develop in them the traits that conduce best to survival. The higher the animal, the smaller its equipment of instinct, the greater its power of acquiring mental traits, the more elaborate is the system of education to which it is subjected. The instinct which impels the human mother to teach her offspring is developed in bird and beast as well. Poultry may be seen in every farmyard instructing their broods in the arts of discovering food or of avoiding enemies. In our homes the cat plays a game of hide-and-seek with her kittens, mimicking the hereditary warfare against mice. Books of sport and travel tell how wild dogs, bears, lions, tigers, and especially elephants and monkeys, instruct their young. All higher creatures, in fact, instinctively seek by example to educate the next generation, which by virtue of strong, imitative instincts is able to profit by the lessons. Thus, the instinct of the parent assists in the development of reason in the offspring, but in this the instinct of the offspring also assists. The sports in which all young mammals *instinctively* indulge are obviously adapted to this end, as well as to enable them to acquire fit corporal traits, as is beautifully illustrated by the differences in the sports of kittens and puppies.

We constantly meet passages in the writings of various authors, wherein they attribute this or that mental change in lower animals or men to "centuries of domestication," or "centuries of civilization," or "to centuries" of this or that change in the environment, the implication being that acquired mental traits are transmitted and accumulated, for otherwise the word "century" would not be applied to creatures whose lives are much shorter. The evidence, however, is overwhelmingly against such a supposition. Our various breeds of house dogs, for example, have all lived and evolved under much the same conditions, and therefore if acquired mental traits are transmissible, should exhibit similar qualities. The difference between the dispositions of bulldogs and lapdogs, for instance, can only have arisen (1) through artificial selection, or (2) through differences in individually acquired (and not transmissible) traits; the latter being secondary to physical conformation, which in the bulldog must tend, through success in conflict, to produce a bold, resolute, and confident spirit, and in the lapdog a spirit much the reverse. And here I may remark, that in all animals capable of acquiring mental traits, the character of these traits must in every case be profoundly influenced by the physical conformation. *À priori*, very many of the traits exhibited by domesticated animals must be acquired, not inborn, since they are mentally so malleable as to be capable of domestication, of adapting themselves to an "abnormal" environment; *à posteriori*, these traits are proved to be acquired and non-inheritable, since every one of our domesticated animals,—dogs, cats, horses, sheep, goats, pigs, poultry, &c.,—when relieved of man's presence, exhibit, after a single generation, all the traits of their wild congeners, including extreme dislike and dread of man. On the other hand, many wild animals, especially those who have a minimum of instinct of inborn mental traits, and a maximum of reason, of acquired mental traits, such as monkeys, ichneumons, parrots, jackdaws, &c., exhibit in the first generation of captivity as thorough a subjection to man as the species longest domesticated, especially if during growth, and even during adult life, they are allowed no commerce with, and so can acquire no traits from, the wild members of the same species.

As regards man also, the inborn variations caused in him by "centuries of civilization" can only be such as are due to Natural Selection, acting under somewhat changed conditions. These, as will be seen, are often very important physically, but much less important mentally. It will be found that civilized man differs mentally from savage man almost exclusively in acquired traits, the difference in inborn traits being practically inappreciable, except in one instance, which, however, is of capital importance.

CHAPTER III

Of all young creatures, the human infant is the most helpless, the least equipped with instinct for the battle of life; of all grown creatures the adult man is the most helpful, the best equipped with reason for the strife. Beyond all other creatures, the human being possesses the power of mentally varying in response to stimulation from the environment, and thereby bringing himself into completer harmony with it. And always, owing to the imitative instinct, as strong in him as in lower animals, his variations tend to reproduce those of his immediate predecessors. At birth his mind, mainly the product of that great mass of nerve tissue, the cerebrum, is an unploughed field, which, ploughed by experience, brings forth, according to the seed that is sown; a blank unwritten page on which chance shall write; a mass of clay ductile to the hands of the moulder.

If we bear steadily in view the two cardinal facts, (1) that the mind of man is compounded almost entirely of acquired traits,—acquired knowledge, acquired ways of thinking and acting, acquired likes and dislikes, &c.; and (2) that the action of Natural Selection has plainly been such as to cause in him, not an evolution of this or that mental trait, but mainly of a power of acquiring mental traits in response to stimulation, we are able to understand many things which at first sight seem puzzling.

For instance, there are present in adult man many highly-developed faculties, which cannot possibly have contributed in an appreciable degree to survival, and therefore cannot have been evolved through the action of Natural Selection, more especially as these faculties vary in their development immensely in different races of mankind. Such are the musical, the mathematical, the artistic, the devotional, and many other faculties, none of which can greatly, if at all, have affected the survival rate. The existence of these faculties has supplied many thinkers with arguments against the doctrine that the organic world has arisen solely by the accumulation of inborn variations. It is maintained by some that they have resulted, and can only have resulted, from the accumulation of acquired variations, from the accumulation during generations of the effects of use. Mr. Wallace, who rejects this latter theory, maintains that they supply proofs of spiritual interference.[1] But it appears to me that there has been no evolution of these faculties at all, but only an evolution of the power to vary mentally in response to stimulation, by virtue of which man is enabled individually to acquire a thousand traits, these among others, and so to adapt himself to an environment which has become immensely complex and heterogeneous. For example, the musical and mathematical faculties have been apparently growing for many centuries, and recently, as we know, at a very accelerated rate. Are we to suppose that they have increased *pari passu* with the advance in musical and mathematical knowledge, and therefore that the modern school-boy, whose knowledge of music and mathematics surpasses that of ancient masters, possesses greater faculties? Assuredly not. It would be as reasonable to attribute the growth of our knowledge

[1] *Darwinism*, by Alfred Russel Wallace, p. 474.

of steam-power to the growth of a faculty for understanding the uses of steam. His greater knowledge is certainly due solely to the circumstance that he has been the recipient mentally of appropriate stimulation which was wanting to the ancients; in other words, that he has been taught that which they more painfully discovered. When therefore we read in books of travel that the individuals of this or that tribe of savages possess no knowledge of numerals or of music, we are not to suppose, as travellers commonly do, that they are incapable of acquiring the knowledge, but only that the appropriate stimulation, which has developed the knowledge in our minds, was wanting to them. Many negroes, when instructed, have shown themselves capable of high mathematical attainments, and the whole race in America is notoriously musical. I myself have heard, in Honolulu, a Kanaka band, which I was told was of great excellence, and the Maoris of New Zealand appeared to me fully equal in powers of mind to average Europeans, though far surpassed by the latter in actual knowledge.

As evidence that the mathematical, musical, and artistic faculties have not resulted from the action of Natural Selection, Mr. Wallace points out that individuals vary to an extraordinary extent as regards these powers, a small minority having a thousand-fold greater endowment than average humanity. The powers of these gifted beings, he says, are plainly not attributable to the survival of the fittest, and must therefore be attributed to some other cause, which he thinks can only be spiritual. We have seen that when the nervous systems of two allied species of insects—*e. g.* ants—appear precisely similar, there is often a vast difference mentally in some one or other particular; on the whole, their nervous systems subserve much the same purposes —*i. e.* the products of the functional activities of the several nervous systems are much alike. In love of life, in desire for food, in social feeling, &c., the different species resemble one another, but in this or that particular, in the nest-building, the fighting, the slave-getting, or other instincts, they differ. In them, a difference in nervous tissue, so minute as to be inappreciable to us, results in mental products that differ greatly. Now the huge brain of man, in which, as we cannot doubt, various tracts subserve definite functions, is subject to great variations in size (as shown by differences in the cranial capacity) and in the complexity and amplitude of the convolutions. Surely, if minute differences in the nervous systems of other animals result in mental differences so vast, it is possible to account for much less fundamental and important differences in human faculties, as manifested in different individuals, by supposing that they likewise are due to more or less slight differences in the systems, especially when we remember how small a mass of nerve tissue —*e. g.* the thermal and breathing centres—may subserve important functions.

Much, very much, has been written concerning man's moral nature. Numerous authors, especially theologians, have assured us that there is inborn in him a knowledge of good and evil, that, in fact, this knowledge is instinctive, not acquired. But all the evidence points the other way. On both *à priori* and *à posteriori* grounds we are forced to the conclusion that man's moral nature is acquired, not inborn. In the first place, it is impossible to conceive how the possession of a high moral tone can have so affected the survival rate as to secure to the possessors of it such advantages in the struggle for existence, that there was thereby brought about an evolution in moral tone by the survival of the morally high. In the second place, young infants certainly give no indication of a moral nature, for whatever is moral in an individual appears later, at a time when the imitative instincts have come into play. In the third place, morals are evidently affairs of time and place—that which we now hold to be right was in other times held to be wrong, that which we now hold to be wrong is in other lands held to be right: our pagan ancestors persecuted the Christians; our Christian ancestors persecuted the Jews and Pagans; we hold religious persecution to be the most heinous of crimes. The Thugs approve of murder, the American Indians of robbery, the Chinese of infanticide, the Japanese of suicide, the Africans of cannibalism, the Turks of polygamy, some Indian hill tribes of polyandry, the Masai of promiscuous sexual intercourse, and so forth. On the other hand, we habitually do, without any sense of wrong-doing, that which other peoples esteem highly criminal; for instance, we eat flesh, and in particular beef and pork.

In the medley of moral systems two things are positively clear. First, that no system is inborn, but all are acquired; and second, that persistence of a moral system, during any number of generations, does not cause it to become inborn, *i. e.* acquired moral natures do not become transmissible no matter how often acquired; they are handed from parent to child as language is, or, as I may even say, as property is, not as eyes and teeth are; as is proved by the indubitable fact that the children of any race (*e. g.* Anglo-Saxons), if reared by another race (*e. g.* American Indians), develop the moral nature of the educators, not of the progenitors. Perhaps there is nothing more characteristic of the English than corporal modesty; there are among us women who would rather die than expose portions of their persons to the public gaze, or even to the gaze of accredited individuals, as every surgeon knows to his discomfiture, resembling in this the modesty of Arab ladies as regards their faces.[1] Yet the English infant exhibits no corporal modesty, and the English-born adult reared among savages has none of it. Nothing indeed can be plainer than that moral natures, or so-called moral natures, are acquired, not inborn. But because in man, the highest and mentally (perhaps physically also) the most variable of animals, the most responsive mentally to appropriate stimulation, that which is mentally acquired so greatly predominates over that which is mentally inborn, because in him instinct is so overpowered by reason (or perverted reason), his acquired traits often possess all the strength that instincts do in the lower animals. How many instincts, for example, are overmastered and set at nought by the abstemious monk or nun?

Speaking of moral systems the historian Buckle observes—

"Applying this test to moral motives, or to the dictates of what is called moral instinct, we shall at once see how extremely small is the influence those motives have exercised over the progress of civilization. For there is unquestionably nothing to be found in the world which has undergone so little change as those great dogmas of which moral systems are composed. To do good to others, to sacrifice for their benefit your own wishes, to love your neighbour as yourself, to forgive your enemies, to restrain your passions, to honour your parents, to respect those who are set over you—these and a few others are the sole essentials of morals; but they have been known for thousands of years, and not one jot or one tittle has been added to

[1] I have read somewhere, though I cannot lay hands on my authority, of a traveller, I think in the island of Socotra, who unexpectedly met an Arab woman. He relates that she immediately covered her face with her petticoats, thus completely exposing the lower half of her body.

them by all the sermons, homilies, and text-books which moralists and theologians have been able to produce."—*History of Civilization*, vol. i. p. 180.

But of these fundamental parts of almost all moral systems which Mr. Buckle instances, it is to be observed that they are precisely those which would naturally be inculcated, for their own good, by the normal instructors of youth. They are nearly universal, not because they are inborn, but because the conditions under which they are acquired are almost universal.

Here I cannot refrain from observing, though the remark is foreign to the purpose of this work, that the great diversity of moral systems among the peoples of the world should render each individual human being cautious in believing, that the moral system which he has acquired from his progenitors is an absolute guide as to right and wrong, is a compendium to which he may safely refer moral questions with absolute confidence. Other moral systems which sanction rape and murder are plainly wrong, and so may be our own system in some particulars. Conscience alone is evidently an unsafe guide, since its promptings manifestly vary with time and place, since conscience prompted the Crusades and the horrors of the Inquisition, and prompts at this day the West African to torture before his Fetish sentient animals, valuable to him, but sacrificed because of his sense of right. That only should we regard as right which we have valid reason for thinking so; that only as wrong which, not for ancient prejudice or superstition, but for valid reason, we perceive to be so. Reasoning by analogy from other peoples, we may be sure that much that we abhor is not abhorrent, and much that we revere is not reverend, but the reverse.

In a species of ant,

"*F. rufescens*, the structure of the animal is such as to render self-feeding physically impossible. Its long and narrow jaws, adapted to pierce the head of an enemy, do not admit of being used for feeding, unless liquid food is poured into them by the mouth of a slave. This fact shows of how ancient an origin the instinct of slave-making must be; it has altered in an important manner a structure which could not have been so altered prior to the establishment of the instinct in question."—*Animal Intelligence*, p. 66.

When we consider that, though our knowledge of ants is yet in its infancy, we have already sufficient information to warrant us in placing these minute insects above all animals except man—I had almost said civilized man—in regard to their powers of reason, we may be permitted to question whether the habit of slave-making should not be classed as an act of reason rather than one of instinct. The fact that the slaves, in their new homes, so readily adapt themselves to the changed environment, so readily exhibit knowledge and ways of thinking and acting which must be acquired and cannot possibly be instinctive, for the reason that their ancestry can never have been subjected to the influence of a like environment, proves how great a share reason has in all that is mental in them. And since the slaves clearly acquire mental traits which fit them for their duties as servants, it is not unreasonable to suppose that the slave-holders in like manner individually acquire the mental traits which fit them for their functions as masters, *i. e.* that the slave-holding habit in them is not instinctive but rational. The question is capable, I imagine, of experimental investigation. Should it prove that the slave-making habit is an individually acquired, not an inborn trait, we should then have, in the highly modified structures of *F. rufescens*, an example of physical structures profoundly modified in consequence of the acquirement by generation after generation, through innumerable generations, of certain mental traits; we should have an example of physical structures profoundly modified, because the traits acquired caused the survival of individuals different from those which would otherwise have survived. In that case the acquired habit of slave-making, transmitted from preceding generations to succeeding generations, as men transmit language or property, has been the cause of the survival of those ant communities in which the individuals were most fit mentally and structurally for slave-making, for fighting, rather than of those communities in which the individuals were most fit for maintaining existence by their own industry, and therefore the cause of the evolution of the enormous jaws and their co-ordinated structures, and the concomitant retrogression of many other structures.

In man occur many examples of structural evolution and retrogression traceable to the persistent acquirement by generation after generation through many generations of various traits. His hearing and notably his sense of smell have retrogressed, because, owing to his growing powers of acquiring reason, these senses less and less influenced the survival rate. His teeth, and all the structures co-ordinated with them (jaw-bones, muscles, nerves, blood-vessels, &c.), have retrogressed, as has also his whole digestive apparatus, owing to his acquired habit of cooking his food; the survival rate being here beneficially influenced by an acquired trait, which rendered him above all other animals omnivorous, which enabled him to use for food a greater number of things than any other animal; and therefore, though man is no longer able to digest the raw food on which his remote ancestors subsisted, yet his acquired habit brings him into completer harmony with the environment than did his lost powers. His acquired habit of bi-pedal progression, a habit acquired anew by every individual, has resulted in immense and obvious structural changes. But certainly, above all other acquired traits, his acquired trait of speech has most influenced his survival rate, and has therefore been the cause of the most far-reaching evolution and retrogression. To this acquired trait very much of the evolution and retrogression directly traceable to other acquired traits is indirectly traceable. By means of it, more than by means of any other of his acquired traits, has he been placed in harmony with the environment. For it is clear, that those individuals who had the best powers of speech, *i.e.* of communicating information, and the best powers of understanding speech, *i.e.* of acquiring information, had the best powers of placing themselves in harmony with the environment. Owing to the acquirement by generation after generation through innumerable generations of language, man has retrogressed so far in structure and instincts that, but for speech, and all that is communicated by one generation to another through the medium of speech, he is as certainly unfit for existence as *F. rufescens* is without its slaves. If for a single generation this all-important trait were not acquired, and man, were it possible, survived, he would at once be reduced to the condition of a brute, and, his equipment of instinct being small, a very helpless brute. His highest faculty, his enormous power of varying mentally in response to stimulation, of acquiring fit mental traits, would be rendered nugatory, since it would no longer receive appropriate stimulation. Under the new condition of things, the fit who survived would no longer be the same as heretofore, and since the power of acquiring the higher mental traits would no longer be a principal factor in survival, that power would necessarily decline by the lapsing of ancestral inborn variations, and man would degenerate towards that ancestral form in which it did not exist.

Now of all the traits acquirable by man, articulate language is the most difficult to acquire. It is certain that it can only have been perfected by slow improvements occupying long ages, during which also occurred a corresponding and contemporaneous evolution of the structures which subserve speech and thought. If, then, the knowledge of articulate language were lost and man survived, before it could be re-invented and perfected, the structures which subserve it (tongue, brain, &c.) would probably have retrogressed so far, that the acquirement of articulate language, in any form approaching perfection, would be impossible, until, by a slow process of evolution, man had once more emerged from the brute.

The slow evolution of speech, and the slow concurrent evolution of the structures which subserve speech, during innumerable generations, the one generation transmitting that which it acquired from the preceding generation, with slight improvements, to succeeding generations, which, by the survival of the fittest, were enabled not only to acquire the speech with the improvements, but to make further improvements, all in like manner to be transmitted to descendants of slightly larger powers, the constant repetition of this process, till speech as we know it was developed, furnishes us with the means of learning by analogy the process by which some of the more complex acquired traits in man and lower animals have been developed. Like language, these are individually acquired by each generation, and like language have been developed and perfected during many generations, and this especially when the acquired trait is one which is only acquirable by the individual slowly and with difficulty as speech is.

The acquired mental traits of the lower animals are often nothing more than mere extensions of their instincts. Thus the cat and the tiger instinctively know how to hunt their prey, but they do it better for the instructions given them by their progenitors. Young birds know how to build nests, but evidently learn by experience, since older birds are the better architects. Such traits as these, which are easily acquired, can have been little improved on since they were first superimposed on instinct. But such traits as language in man, and probably also many traits in ants, which are acquired with difficulty by the individual, the acquirement of which occupies a considerable time during the ontogeny, and which considerably supplement such instincts as the animal possesses, must have arisen only by small accretions, slowly and painfully added during the phylogeny. They are found only in mentally the highest animals, and betoken in them great powers of varying mentally in response to stimulation, of acquiring reason. Every such trait is one which considerably increases the harmony with the environment, and is one therefore which very favourably influences the survival rate; as is easily proved *à posteriori*, and of which *à priori* proof is furnished by the fact, that notwithstanding the time spent and the mental toil endured by each individual in acquiring it, it yet remains persistent in the species.

Savage man differs from lower animals chiefly in that he possesses the power of acquiring articulate speech, and the knowledge he thereby acquires enables him to place himself in harmony with an environment of far greater complexity than that of any other animal. Civilized man differs from savage man chiefly in that he has invented, and more or less perfected, certain artificial aids to speech, by virtue of which he is enabled to acquire or store in an available shape vastly more knowledge than the savage, and is therefore able to adapt himself to an environment of vastly greater complexity. By means of written symbols, representing words or thoughts, he is not only able to store in a form easily available to himself, his fellows, and his descendants accumulations of knowledge so immense that no memory could contain them, but by means of these symbols he is able (*e. g.* in the mathematics) to perform feats of thinking utterly beyond the powers of the unaided mind; just as by means of tools, machinery, and other mechanical contrivances he is able to perform physical feats utterly beyond the unaided powers of his body. To written symbols representing words or thoughts is due, practically speaking, his whole advance beyond the savage in the past, and to them are due also his vast potentialities for future advance.

CHAPTER IV

No opinion is more universally entertained than that the races of mankind differ mentally, each one from all others, by reason of inborn traits; and though opinions differ as to the mental peculiarities of this or that race, though each race has usually a very flattering opinion of its own mental proclivities and capabilities, and a very unflattering opinion of the mental proclivities and capabilities of other races, yet seldom is it doubted that such proclivities and capabilities are inborn. For instance, to take the English point of view, few Englishmen doubt that their own race has "by nature" a genius for empire and colonization, is brave, resolute, law-abiding, cool-headed, liberty-loving, industrious, enterprising, &c.; that "by nature" Frenchmen are fickle, frivolous, and ardent; that, for the same reason, Germans and Dutchmen are phlegmatic and thoughtful; Russians dull and barbarous; Spaniards proud, indolent, and superstitious; Italians musical, excitable, and violent; Bengalese cowardly, servile, and subtle; Afghans fanatical, fierce, and treacherous; Chinese, in some things abnormally clever, in others abnormally stupid; West Africans, almost beyond belief, foolish and cruel; Fingoes braggart and timid; Zulus brave and honest; that this race has great artistic tendencies and that race none at all; that this race has considerable capacity for civilization, that race less, and that other none at all; that the Irish have "by nature" one kind of temperament, and the Scotch another.

Almost alone among historians, Buckle has questioned whether races differ inherently as regards their mental peculiarities. He says—

"I cordially subscribe to the remark of one of the greatest thinkers of our time, who says of the supposed differences of race, 'Of all vulgar modes of escaping from the consideration of the effect of social and moral influences on the mind, the most vulgar is that of attributing the diversities of conduct and character to inherent natural differences' (Mill's *Principles of Political Economy*, vol. i. p. 390). Ordinary writers are constantly falling into the error of assuming the existence of this difference, which may or may not exist, but which most assuredly has never been proved. Some singular instances of this will be found in Alison's *History of Europe*, vol. ii. p. 336, vol. vi. p. 139, vol. viii. pp. 525, 526, vol. xiii. p. 347, where the historian thinks that by a few strokes of his pen he can settle a question of the greatest difficulty connected with some of the most intricate problems in physiology."—Buckle's *History of Civilization*, vol. i. p. 40.

"Whatever, therefore, the moral and intellectual progress of men may be, it resolves itself not into a progress of natural capacity, but into a progress, if I may say so, of opportunity; that is, an improvement in the circumstances under which that capacity after birth comes into play. Here, then, lies the gist of the whole matter. The progress is one, not of internal power, but of external advantage. The child born in a civilized land is not likely as such to be superior to one born among barbarians; and the difference which ensues between the acts of the two children will be caused, so far as we know, solely by the pressure of external circumstances; by which I mean the surrounding opinions, knowledge, associations, in a word, the entire mental atmosphere in which the two children are respectively nurtured."—*Ibid.* p. 178.

Now, since so little in man that is mental is inborn, and so much is acquired, deductively it seems probable that Mr. Buckle is right. It may, however, be argued, that since survival of the fittest has caused in different environments the various races of mankind to differ as regards their physical characters, it is highly likely that it has caused them to differ as regards their mental characters also. But here crops up that old and fertile source of error, the idea that the mind is an organ, not merely the product of the functional activity of an organ. The mind is the product of the functional activity of the nervous tissue, mainly of the brain, and therefore is comparable, not to other organs, but to the products of their functional activities, using the words in their widest sense. The brains of various races differ in size and shape, and may differ structurally in many other respects, not as yet observed by us, and these differences have possibly or probably for their concomitants mental differences of more or less importance, but it is doubtful whether these latter are of the kind commonly supposed.

We have seen that organic evolution consists essentially of a process of gradual structural modification, whereby evolving species are brought into harmony with environments gradually increasing in complexity. Counting from unicellular organisms, the process began with the evolution of multicellular organisms, was continued by the evolution of complex and heterogeneous shapes and structures in multicellular organisms, and is proceeding by the evolution, in the higher of them, of an immense power of varying in response to stimulation from the environment, by means of which they are placed in closer correspondence with it. Contemporaneous with the evolution of the higher portion of the brain, and certainly dependent on that evolution, has been an evolution of mental variability, which has still further increased the correspondence. In man this power of developing mentally in response to stimulation from the environment has grown to be so great, and is of such importance in his struggle for existence, that his survival is determined mainly by virtue of it, not by his inborn mental traits (instincts), which have therefore undergone great retrogression. It follows that nations, the individuals of which have on the average large brains, which have the cerebrum and the cerebellum much developed, differ from races the individuals of which have smaller brains, which have the cerebrum and cerebellum less developed, mainly in powers of varying, of developing mentally, by acquiring mental traits in response to appropriate stimulation; not mainly in inborn traits, such as are implied when it is said that this or that race differs inherently from this or that other, in that it possesses a greater genius for empire or colonization, or music, or art, &c. Indeed, while it is conceivable that survival of the fittest has caused the evolution of the instincts properly so called (love, fear, rage, &c.), or rather, to speak more precisely, of the nervous structures which subserve these instincts; and while it is conceivable that it has caused the evolution of the higher portions of the brain, and, as a consequence, the evolution of the power of varying mentally in response to appropriate stimulation, of acquiring fit knowledge and ways of thinking and acting from the environment; it is not conceivable that it can have produced such inborn mental differences among races, as are meant when it is said that the Englishman is "by nature" resolute, the Frenchman vain; the Italian excitable, the German phlegmatic, the West African cruel, and so forth. For it can hardly be, that the environment in which the English have evolved has so differed from the environments in which the French, the Italians, the Germans, and the West Africans have evolved, that survival of the fittest, in the case of the Englishman, has caused in them a greater evolution of the nervous structures which subserve resolution than it has caused in the other nations named; it can hardly be that the Frenchman is *inherently* vain because the environment of his ancestry was such that superior vanity caused a greater survival or a lesser elimination than it caused in the other nations; there is no conceivable reason why phlegm in Germanic surroundings should have conduced to survival more than in West African surroundings; or why cruelty should have been more beneficial or less deleterious to the ancestors of the West Africans than it was to the ancestors of the Germans.

We must suppose, therefore, that differences in the sizes and shapes of the brains of various races imply, not differences in inborn mental traits, but differences in the power of acquiring traits; we must conclude that the African Bushman, with his small brain, differs mentally from the Englishman with his large brain, not mainly in inborn mental traits, but mainly in the traits which he acquires, and *in his powers of acquiring them*. Reasoning from the analogy of lower animals, it is certain that the man with the smaller and less developed brain differs inherently from the man with the larger and better developed brain, mainly in that he has a smaller power of developing in response to stimulation, a lesser power of acquiring fit mental traits, not mainly in that he has different inborn mental traits, for these latter (*i.e.* instincts), as I say, have been so replaced and overshadowed in man by acquired mental traits, that they have undergone great retrogression, and except as regards certain instincts, common to all races of men as well as to lower animals (sexual love, &c.), are of very subordinate importance.

It is possible, however, since, as seems probable from the above considerations, the Bushman's power of acquiring mental traits is smaller than the Englishman's, that his instincts have undergone less retrogression, and therefore that, while the latter do not differ in kind in the two races, they may differ in degree. They cannot differ in kind, or at least it is highly improbable that they do, because though there is much evidence that man has undergone great retrogression in many directions as regards instinct, there is no evidence that he has undergone evolution in any direction as regards this faculty; his entire mental evolution apparently having been in the direction of the power of acquiring mental traits. This becomes evident when we consider that not only are his powers of instinct on the whole less than those of any other mammal, and very much less than those of any reptile, but that even in those particular instincts which have survived in him, and are essential to his persistence, he shows no evolution beyond lower types. Not one of these instincts is peculiar to him, in not one does he surpass inferior animals.

If the above be true, it follows, since the evolution of instinct has ceased in man, that no new instincts have been evolved either in the Bushman or the Englishman, and therefore that they can differ as regards instinct only in that one race may have more and others less of this or that instinct. But because, even in the Bushman, that which is mentally acquired so greatly overshadows and replaces that which is mentally inborn, practically the whole mental difference between him and the Englishman must consist (1) in the powers of acquiring mental traits, and (2) in the traits acquired, which differ according to differences in the environment, the second factor being of vastly greater importance than the first. In other words, the Bushman differs mentally from the Englishman almost entirely because the stimulation he individually receives from his surroundings is different, a chief factor in producing his peculiar traits being his progenitors, who rear him and cause him to acquire mental traits similar to their own.

Lastly, it should be noted that this power of developing mentally in response to stimulation may not, and probably does not, proceed on lines entirely parallel in races differently circumstanced as regards the environment; for instance, the circumstances under which the ancestors of the Bushman have lived are such, that possibly he is able more readily and to a greater extent to acquire habits of watchfulness against enemies than the Englishman, whose ancestors, on the other hand, have perhaps been so circumstanced, so dealt with by Natural Selection, that possibly he is able more readily and to a greater extent to acquire the higher mental traits. But mental differences arising from this cause must likewise be slight, when compared to the differences produced by the direct action of the environment on the individual; for, on the whole, the evolution of the power of developing mentally in response to stimulation must have proceeded on lines nearly parallel in all races of mankind. There does not perhaps exist a potential Wrangler among the Bushmen, but judging from such children of savage parentage as have been reared in civilized communities, the average Bushman can be brought under fit training to closely resemble in his mental traits the average Englishman.

Numerous facts inductively support the deduction arrived at above, and considering that the bearings of many of these facts are extremely obvious, it is remarkable that philosophers, historians, and other thinkers and writers have almost universally been of the opinion that the mental differences exhibited by the various peoples and nations of men are racial (*i. e.* inborn), not educational (*i. e.* acquired); or perhaps it would be more correct to say, that it is remarkable that generally they have not perceived, when considering aggregates of men, that the units of the aggregate are like mentally to one another, and to their progenitors, mainly because they have all received after birth, like mental stimulation from the environment, have all been subjected to like educational influences, not mainly because their racial or inborn traits are similar; and that they differ mentally from the units of other aggregates of men mainly because their educational influences have been different, not mainly because their inborn mental traits are different.

The enormous importance of educational influences as regards individuals is very generally and fully recognized, and therefore parents in all countries exercise the most anxious and vigilant care in the bringing up of their children; but seldom is the fact recognized, that the educational influences which affect in common the individuals of a whole race may be good or bad, or a combination of the two, and that by them is determined the material and intellectual progress or stagnation, the weal or woe of the whole race; seldom is the fact recognized, that just as different races of men differ in the languages they acquire from their progenitors, so also do they probably differ in other acquired traits of great importance; never is the fact appreciated that the young human being, unlike the young of lower animals, is born with a mind which is practically blank, and that nearly all that is mental in him is subsequently acquired in response to stimulation from the environment, the chief factor in that environment being the influence exercised on him by his progenitors. It is generally realized that a child reared by the uncleanly will probably be uncleanly; that a child reared by the slothful will probably be slothful; that a child reared by the energetic will probably be energetic; that a child reared by the dishonest and immoral will probably be dishonest and immoral; that a child reared by the superstitious will probably be superstitious; that a child reared by the brave and enterprising will probably be brave and enterprising; that a child reared by the timid and cautious will probably be timid and cautious; that, in fact, the mind of one generation imprints itself on the mind of the next generation, not racially but educationally; but there is a general failure to realize that the aggregation of individuals called a race or a nation is governed in this respect by the same laws and conditions as an individual, and therefore if one generation be slothful the next will also be slothful; if one generation be active the next also will be active, and so forth; but herein lies the key to the distinguishing peculiarities of nations and races, and to much of the history of the world.

Diverse educational influences produce diverse knowledge, modes of thought, and motives for acting, and since races are aggregations of individuals, and the aggregation of individual efforts moulds the destinies of the race, I can best explain what I mean by taking as examples three individuals, one of whom is a scientist, the second a manufacturer, and the third a monk. Though they may all be of equal powers, yet their different knowledge, ways of thinking, and motives for acting will lead to very different results. The labours of the scientist will result in an increase of knowledge, the manufacturer will produce an increase of wealth, whereas there will be no tangible result, at any rate on earth, to the labours of the monk. But if early influences had caused the scientist to be a monk, the monk to be a manufacturer, and the manufacturer to be a scientist, then in each case acquired and educational traits would have led to knowledge, ways of thinking, and reasons for acting, and thence to practical results widely different to those which other influences produced. And if the educational influences which act on all the individuals of a race in common, are such as to produce in them a general tendency to the pursuit of knowledge, or of wealth, or to the practice of asceticism, it is abundantly evident that these educational influences will so affect the acquirement of knowledge, of modes of thought, and of motives for acting in the whole race, that the resulting state of society will be quite different in any one instance from what it would be in any other.

CHAPTER V

A COMPARATIVE study of the effects of different religious systems on the human mind lends powerful support to the doctrine that nations and races differ mentally not so much in that which is inborn, as in that which is acquired. Obviously all that arises in the developing mind of a young human being in consequence of the inculcation by his progenitors of the beliefs, the morals, the ways of thinking, the motives for acting, &c., peculiar to any religious system, is acquired, not inborn; and therefore if the individuals of a group of nations which have a religion in common are mentally alike to one another, but mentally different from the individuals of another group of nations, which profess a different religion, the presumption is that their mental similarity to the individuals of the same group is due to the community in religion—*i. e.* in acquired traits—and their mental dissimilarity from the individuals of a different group to the difference in religion, especially when individuals of different religious groups are circumstanced more alike as regards the rest of the environment, than the individuals of the same group; or when individuals of the same race, but of a different religion, resemble mentally their co-religionists more than their compatriots. For instance, the presumption is, that the mental likenesses to one another, and mental unlikenesses to peoples of other religions which Mahomedans display, are due to the influence of their religion, and therefore to traits which are acquired, not inborn.

Now there is probably nothing in the environment of man that has more mightily affected his knowledge, and ways of thinking and acting, than his religious systems. They have been the greatest of his educational influences, and therefore there is nothing that so certainly gives a nation its status in the scale of civilization, progress, and prosperity as the faith it professes; for every religion, by teaching beliefs and ideas peculiar to itself, imposes on its adherents a state of society resulting from those acquired beliefs, and ideas quite distinct and different from that which is imposed by any other religion, and thus produces a mental uniformity which is much greater than the mental uniformity produced by racial, climatic, or any other influences. We may expect to find, therefore, that all communities that have any given religion in common, have also mental characteristics so much in common that the resulting state of society in any one of the communities is such as is more or less common to all the other communities—that they all have a nearly similar civilization, attain to a nearly similar degree of prosperity and enlightenment, and exhibit a nearly similar degree of respect for law and order, &c. That this is true there is unlimited evidence; for throughout the world, whatever community of whatever race, in whatever climate, professes a given religion, it invariably has a civilization nearly similar to that of all the other communities professing the same religion. Other educational influences may of course modify the civilization which is associated with any religion, but so great and so dominant is the influence of the latter, that the modification is in all cases surprisingly slight.

The example of the Mahomedans, since they include in their ranks a great number of races, dwelling in a variety of climates, under very diverse physical conditions, admirably illustrates my argument. It is manifest that the Mahomedan civilization is everywhere much the same. The Malay of the Eastern Archipelago is about as high in the scale of civilization, but no higher than the Moor of Fez, than the Turcoman raider on the Oxus, or the Arab slave-trader on the Congo. All these widely separated peoples, of such diverse races, have much the same degree of prosperity and enlightenment, and practically they all think and act much alike. In other words, being in common under the influence of a dominant educational factor, their acquired knowledge, modes of thought, and motives for action, and the state of society resulting therefrom, are much the same. The Turk alone has received a thin veneer of the civilization of the Christians. Forced by the exigencies of his position, he has adopted the quick-firing rifles, the big guns, the ironclad ships, and the public debt of his Western neighbours; but everything that he has in excess of other Mahomedans is imported, purchased by the superior command of money, which the extent and populous condition of his territories bestows. At the bottom he remains the man that Mahomed made him, and, if the pressure of the Christian civilization were removed, would lapse back to what he was in the days of Osman. All the swarming millions of the Buddhists think and act much alike to one another, but differently from the peoples of other religions; their strange civilization is unlike all other civilizations, and their various communities have about an equal degree of prosperity. The same is true of the Hindoos, and of the adherents of all other religions.

We have here a law of civilization which has all the force of a law of nature, so uniform is its action; namely, that a civilization or a state of society invariably conforms to the religion with which it is associated. The amount of civilization achieved is in fact the amount of progress permitted by the associated religion, which, in most cases, sets such limits to further progress as cannot be passed unless the religion be first abandoned. Thus Fetishism, by teaching that which is false and wrong, and thereby preventing an apprehension of that which is true and right, sets such limits to the progress of its adherents, that it is quite impossible for them to attain to a higher civilization without first abandoning their faith. But as the action of a natural law may be modified by external causes, as, for instance, the action of the law of the expansion of gases is modified by pressure, so the action of the law that connects a civilization with a religion may be modified by the pressure of other civilizations: as, for example, the Mahomedan civilization has been modified in the case of the Turks; and as, far more profoundly, owing to more intimate admixture, the Roman Catholic civilization has been modified by the Protestant;[1] and as, in a lesser degree, the Greek Church civilization has been modified.

This modification, however, is, in a great measure, more apparent than real, and consists rather in an acceptance of the things of the modifying civilization,

[1] I speak of the Protestant civilization, but the term is not strictly correct. Our modern civilization should properly be described as one that is disassociated from religion. Thus our literature and notably our various sciences—Biology, Geology, Geography, Astronomy, Ethnology, Etymology, Sanitation, Political Economy, &c.—on which the character of the civilization so largely depends, are quite divorced from the religion, and are even more or less in conflict with it; a result due in great measure to the conflict of opinion among the numerous Protestant sects, which, combined with free intercourse among their members, has caused such a degree of intellectual freedom and activity that the civilization has been changeful and progressive to an extent unknown in lands and ages in which this conflict of opinion, with its resulting freedom of thought, has been unknown —*i.e.* in which the weight of religious opinion has been solidly against the recognition of any newly-discovered truth.

the material products of it, than in an acceptance of the civilization itself: thus the Turks borrow our big guns, but not our ideas; our ironclad ships, but not our modes of thought and motives for action; thus savages accept our rifles and alcohol, but are not thereby civilized. A change from a lower to a higher civilization consists, in fact, in a mental change, a change in knowledge and in ways of thinking and motives for acting, not in material things, for a savage who possesses a bottle of rum, a quick-firing rifle, a silk hat, a mirror, and a piano, is just as much a savage as he is without them; whereas the Englishman or the Frenchman, who is cast ashore, destitute of all the things of civilization, on a desert island, is just as much a civilized being as the dweller in London or Paris.

The supreme importance of the mental traits acquired under the influence of religion is extremely well shown in the case of conquered nations. To take once more the example of the Mahomedans: all those nations which were conquered by them, and which adopted the Mahomedan religion, have received the impress of the Mahomedan civilization; but all those which did not adopt the Mahomedan religion, have quite different modes of thought and motives for action, quite a different civilization—such are the Greeks, the Bulgarians, the Servians, and the Roumanians, who adhere to the religion, and, though of diverse races, possess the civilization of the Greek Church.

History affords numerous examples of races changing their characteristics, and with their characteristics their civilization; but in every case such change was associated with and preceded by a change of religion. For instance, much of Asia Minor and Northern Africa anciently held the Pagan Roman religion and possessed the splendid Roman civilization. Subsequently the inhabitants became Christians, or, to speak more precisely, Catholics, producing vast numbers of ascetics and such celebrated saints as Athanasius and Augustine of Hippo, and their civilization became typically Catholic —that is to say, mediæval. Lastly, they were converted to Mahomedanism, and since then their civilization has been as typically Mahomedan. We have here an example of many races being forced into a uniformity of mental characteristics, and therefore of civilization, wealth, and enlightenment, by a religion held in common. The negroes at the present day furnish an example of a single race professing several religions, and possessing as many different civilizations. Allowing for other educational influences, the black Methodists and Baptists of America differ little in knowledge (or what they conçeive to be knowledge), and in modes of thoughts and motives for action, from their white co-religionists, but markedly from the Catholic negroes of Haiti, who resemble in many of their characteristics the whites of South America, while the Mahomedan negroes of Africa are typical Mahomedans—fierce, fanatical, predatory, non-progressive,—differing greatly in their turn from their neighbours, the Fetish-worshipping negroes.

The form, therefore, which the civilization of any race assumes, is due in great measure to the character of the associated religion, which largely moulds the character of the race; and though the pressure of other civilizations may introduce modifications (more apparent than real, as I have shown), yet in the main a civilization invariably remains "true" to the religion with which it is associated, and if modified at all, tends to resume its original form when the external pressure is removed; as in the case already cited of the Christians of the Balkan Peninsula. But if, while the external pressure is in operation, we wish to observe, in its purity, the civilization which is associated with any religion, we must turn to a time and place at which the civilization was not modified by the pressure of other civilizations—a condition which, as regards the civilization associated with the Roman Church, was fulfilled in the Middle Ages.

This subject might be pursued much further with very interesting results, but it is somewhat foreign to the general purposes of this work, and besides, I have no desire to incur at present the *odium theologicum.* Suffice it therefore to point out, that even the adherents of the three great Churches into which Christendom is divided, differ profoundly as regards their mental traits, especially in districts in which the sects intermix but little. Almost all the intellectual and material progress of the last century or more is due to the adherents of one of the Churches alone, or at least to them and to actual if not declared seceders from the other two; and if we compare the respect shown for law and order as measured by the statistics of crimes of violence—brigandage, murder, conspiracies against the Government, rebellions, civil wars, &c.—we find here also a marked difference. If, notwithstanding all the foregoing, it be maintained that this is due to racial—*i. e.* inborn—not to educational—*i. e.* acquired—traits, let the reader lay down this work, and for a little while endeavour to call to mind as many as possible of the distinguished men, good or bad, of the first rank, who have made the last one hundred and twenty years the most remarkable in human history—naval and military heroes, statesmen, scientists, philosophers, historians, poets, novelists, inventors, travellers, accumulators of wealth, thinkers, and men of action of all sorts; he will find that all, or nearly all, whose names occur to him have belonged to one only of the three Churches, or else have been seceders (actual if not declared) from the other two.

Therefore, while it is clear that the races which adhere to that one Church have not a monopoly of genius, it is clear that in that Church alone is it allowed free play, or comparatively free play.

I have now finished the introductory portion of my work, and may in comfort proceed with my proper theme, the Present Evolution of Man. The ground is cleared, and I have by anticipation met some objections which would otherwise have been raised. The three main facts I have endeavoured to drive home have been—(1) that every species must necessarily undergo retrogression, unless that retrogression be checked by selection; (2) that in such a high multicellular organism as man acquired variations cannot be transmitted; and (3) that in such an organism living amidst immensely complex and heterogeneous surroundings, the action of Natural Selection has been mainly to develop so extraordinary a power of varying in response to appropriate stimulation, direct or indirect, from the environment, such a remarkable power of individually acquiring fit variations, that very much, indeed by far the greatest part of the characteristics of such a high organism are due to stimulation acting on this power to vary, are variations acquired by the individual, but variations which are not transmissible. Therefore, though acquired variations are not transmissible, yet the variations acquired by every "normal" individual have a magnitude and importance far beyond that which is commonly attributed to them by biologists, who usually measure a variation by the amount of its deviation from the "normal" standard, forgetting that the normal standard of development itself is only attained as a result of long-continued stimulation acting on the inborn power to vary, and therefore that the structures, physical and mental, of normal individuals, being in great measure a bundle of acquired characters, are no criteria whereby to decide whether this or that additional variation is an acquired variation or not. If we bear these three cardinal truths well in mind during the ensuing discussion we shall find that while they supply means of solving many problems of the highest importance, and explaining many mysteries hitherto held to be inscrutable, they are themselves amply and often beautifully confirmed by the study in its deeper aspects of the Natural History of Man.

PART II

THE PRESENT EVOLUTION OF MAN

PHYSICAL EVOLUTION

CHAPTER I

It is a common opinion that, in consequence of man's very extensive conquest over the forces of nature, he is no longer under the operation of that law which, in the vast majority of instances, bids species change in adaption to a changing environment, or perish, and that therefore his evolution has ceased. Dr. Moxon expressed this opinion when he said, in the passage quoted at the beginning of this work, that "now the plan is so turned about by the arrival of man on the scene, and by his civilization, that you cannot watch Darwin and Huxley themselves without seeing that the struggle that they and other good men wage is no struggle for existence." It is true that civilized man no longer contends against wild beasts, and that, generally speaking, he is safe from cold and hunger and thirst; no longer does he necessarily perish if he be weak of limb or deficient in cunning, or because he cannot endure prolonged exposure or privation. Nevertheless, it is also true that the strong in mind and body are still the more successful in securing mates, and in providing suitable food and shelter for them, and for their offspring, who in the next generation likewise wage a successful strife.

That the evolution of physical and mental powers in the ancestral direction has not entirely ceased seems to be proved by the apparently well-ascertained fact, that moderns have on the whole larger bodies and brains than had their ancestors of five hundred years or more ago. The ancient Greeks and Romans were certainly of extraordinary mental prowess, but it is more than probable that they surpassed our less remote ancestors only because the environment in which they lived was more favourable than the mediæval to the acquirement of fit mental traits; because, in their free intellectual atmosphere, they were trained to the performance of intellectual feats which were impossible to the fettered minds of our forefathers, who could hardly achieve greatness except as priests, or warriors, or as painters, sculptors, architects, musicians, or as other labourers in such arts as subserved the grandeur of the Church or the Throne. The splendour of the Greek and Roman achievements, therefore, does not constitute a proof that the Greeks and Romans were splendidly endowed, but only that the traits which they acquired from their progenitors enabled them to use their endowments splendidly. In judging of the mental capabilities of a people as a whole, as in judging of physical powers, it is safer to take as a test their corporal structures, their bodies and brains, rather than their physical and mental feats, for whether these latter be great or little depends upon circumstances which may be favourable or the reverse.

What awaits the human races in the future it is difficult to forecast. It may be, as modern tendencies seem to indicate, that as our social organization approaches perfection the labourer will receive according to the effort put forth, not according to the result achieved, and therefore that the weak and feeble in body and brain will be rewarded as richly as the strong and capable. In which case the evolution of man on his ancestral lines will probably cease and retrogression ensue; unless, indeed, in a more enlightened era than our own, the Natural Selection of man is replaced by an Artificial Selection, which shall permit only the happy in mind and body to perpetuate the race.

But side by side with this evolution of brain and body along ancestral lines, which has engrossed the attention of biologists, but which now threatens to cease, man, during the last few thousand years, during which he has largely dwelt in towns and cities, has been undergoing another and a vastly important evolution in a different direction, an evolution which has escaped the observation of biologists, but which threatens by the survival of the fittest and the elimination of the unfittest to advance at accelerated speed in the near future; an evolution none the less real because it is intangible. If we would discover in what direction the evolution of an evolving species is proceeding, it is a good plan to note which members of it survive and which perish. We shall then, by observing the difference in qualities between the fit and the unfit, be able to discover which traits favour survival, and therefore which traits are undergoing evolution by the accumulation of inborn variations. Applying this test to man, it is manifest that the survivors in his species, they who attain to maturity and have offspring, who continue the race, are not, as in the remote past, necessarily the strong in limb and mind, but necessarily the strong against disease. The present evolution of man is therefore not mainly an evolution of physical or intellectual strength, as in his remote ancestry, but mainly an evolution against disease, and wherever men are crowded together, and can take disease from one another, or there are other unfavourable circumstances, especially against zymotic disease—that is, disease due to or produced by living micro-organisms.

Zymotic diseases, both in man and brutes, are due to the presence of living ferments, minute animals or vegetables, very low in the scale of life, which permanently or temporarily take up their abode in the bodies of higher animals, drawing thence their sustenance, and, either by themselves or by their poisonous secretions, causing death or disablement, which latter may be more or less complete and more or less permanent, and which, therefore, may more or less completely and permanently place the individual attacked by them among the ranks of the unfit. In this category are included all infectious and contagious diseases, small-pox, cholera, scarlatina, measles, influenza, syphilis, &c. In some of them the organism, which causes the death or disablement, has been seen and studied under the microscope, as in tuberculosis and cholera; in all the others we are able by analogy to infer with certainty the existence of pathogenic germs, as in small-pox and measles.

Concerning any disease prevalent in any given area—*e. g.* scarlatina in England—experience teaches that some individuals of the community are much more susceptible to its action than are others. For instance, in England, where scarlatina is prevalent, there are people so constituted that they take the disease in a severe form and perish of it, whereas there are others who are totally immune to its action, or who take it in so mild a form that they do not suffer more than a passing inconvenience. Now it is manifest that, in the presence of such a disease, those individuals who are liable to be attacked by it in its severer forms are at a disadvantage, and tend to be eliminated and to leave no offspring, or if by chance they leave offspring, these, inheriting the peculiarities of their parents, tend in turn to be eliminated; whereas those individuals who are immune against the disease, or are so constituted that they take it only in a mild form, are at an advantage, and tend to survive and leave offspring, who, inheriting their peculiarities, are likewise able to survive and continue the race in spite of the presence of the disease. It follows, therefore, that the presence of any fatal or serious disease during generations must tend to call forth in the race attacked a gradually increasing power of resistance to it, just as the presence of carnivora in any country calls forth in weaker creatures—*e. g.* hares and antelopes—increasing powers of evading attack. On the other hand, just as the powers of attack in the carnivora must, as a condition of their survival, undergo evolution *pari passu* with the evolution of the powers of evading attack in their prey, so it is probable, that as the powers of resisting microbic disease undergo evolution in man and brutes, the powers of attack in the pathogenic organisms must undergo corresponding evolution; at least in the case of all those pathogenic organisms to which a human or brute prey is essential to existence,—*e. g.* the micro-organisms of syphilis, rabies, and tuberculosis—if not in the case of those micro-organisms which, having their normal sources of subsistence elsewhere, only accidentally or occasionally attack the higher animals—*e. g.* the micro-organisms of malaria.

For reasons which will be considered presently, microbic diseases are often limited in their areas of distribution, and therefore, as a further deduction from the foregoing, it follows that the races dwelling within any such areas must become, "by nature," more resistant to the action of the prevalent disease than races dwelling outside the area, for the reason that they are descended from an ancestry which was rendered increasingly resistant to the disease by the survival of the fittest; and therefore if individuals, whose ancestors lived outside the area, migrate into it, they should, if we have reasoned correctly, be much more liable to death or disablement from the disease than the normal inhabitants of the land; on the other hand, should the disease from any cause overpass its normal boundaries, we ought to find that the inhabitants of the newly-invaded territory are much more susceptible to its attacks, and to death or disablement from them, than the dwellers within its normal habitat. That we have reasoned correctly may be proved, *à posteriori*, by a great mass of evidence, but the consideration of it also may conveniently be deferred while we turn our attention for awhile to the question of immunity.

Two kinds of immunity, or partial immunity, from the attacks of zymotic disease are distinguishable, the inborn and the acquired. The first, which is transmissible to the offspring, plainly results from the survival of the fittest; the second, which is not transmissible, results indirectly from the same cause; between them they afford a beautiful example of the universal truth that in higher organisms, while inborn traits are transmissible, acquired traits are not transmissible, but die with the individual that acquired them. As regards the first kind of immunity from zymotic disease, the inborn inherited kind, so long as, during the phylogeny, any such disease frequently causes death or serious disablement, the evolution of the race afflicted by it towards complete immunity will continue, and will do so notwithstanding that the law of retrogression will cause at the same time many individuals to revert towards the ancestral condition of non-immunity and to perish. But when the evolution of the species towards complete immunity has proceeded so far, that an attack of the disease does not in the great majority of cases result in death, or in serious or long-continued disablement, the evolution will cease; an equilibrium will be established between the two warring forces of evolution and retrogression, and the species in respect to inborn immunity will remain stationary. For which reason it is that complete inborn immunity is not attained against such very common diseases as measles and chicken-pox, which almost every individual in Europe suffers from, but which are only fatal to a very few—to those, speaking generally, who have reverted, by the lapsing of inborn variations, to the ancestral condition of non-immunity.

As regards acquired immunity: it is a well-known fact, that duration of the attack in many zymotic diseases is limited in the individual; that is, if the individual attacked does not die, he recovers within a certain pretty definite period of time, which is short in some diseases,—*e. g.* measles,—longer in others,—*e. g.* typhoid,—and longer still in a third class—*e. g.* syphilis; but certain other diseases—*e. g.* tuberculosis and leprosy—may persist in the individual for an indefinite time. It is also well known that in all diseases which have a limited duration in the individual, one attack confers for a shorter or longer time complete or partial immunity against subsequent attacks. This is the second kind of immunity, the kind which is not transmissible, but is acquired anew by each individual; for example, one attack of small-pox is usually followed by complete or partial immunity from subsequent attacks of that disease, the individual attacked, in the vast majority of cases, not taking the disease again, or taking it only in a mild form a considerable time after the first attack. Never, or so very rarely that it is regarded as a medical curiosity, is one attack closely followed by another.

Now it is plain, if such a disease as small-pox or measles persisted for an indefinite time in an individual, or if an individual were subjected to repeated attacks of the disease, that either condition would be equivalent to one of permanent disablement, and would place the individual so attacked in the ranks of the unfit, and therefore it is evident that this power of acquiring immunity, of varying in a fit direction in response to appropriate stimulation, like all other powers of acquiring fit traits, whereby the individual is brought into harmony with a more complex environment, has resulted from Natural Selection. This power is, in fact, a short cut, by means of which is brought about in every individual that takes the disease, or at least in all such individuals as have not reverted to an ancestral condition, in which the power was not developed, or was less developed, a condition of complete or nearly complete immunity; a condition of immunity more complete than could possibly have been attained by the survival of the inherently immune alone, if only for the reason already stated, that in the latter case, whenever the immunity approached perfection, the consequent relaxation of the stringency of Disease Selection would cause a racial lapsing back to an ancestral condition of less immunity. It should be noted here, that in all microbic diseases, which run a definite course of limited duration in the individual, immunity, for a greater or lesser length of time after a first attack, must necessarily follow the cessation of that attack, for, since the cessation of the disease is due to the death or departure of the organisms which cause it, that which causes the death or departure of the organisms will for a greater or lesser length of time operate against and cause the death or departure of any fresh organisms of the same kind which may happen to invade the body. Hence it is that whoever has had an attack of small-pox or diphtheria, is for a greater or lesser length of time immune to those diseases.

The facts that many zymotic diseases run a definite course, which is limited in point of time, and that one attack of every such disease for a greater or lesser length of time confers immunity against subsequent attacks, are known to all; but the actual cause of the recovery and the subsequent immunity is still in dispute. Evidently the body of a man who has had cholera, for instance, and is immune, differs in some important respect from his body before he had the disease, and was not immune; but what is the difference? Why is it that the cholera microbes are able to flourish in him during one period and not during the other? The most elaborate microscopic, the most delicate chemical examinations furnish no information. Judged by them the tissues have undergone no change.

"There has for long been the utmost anxiety on the part of physicians and others to obtain some explanation of these remarkable facts. Klebs and Pasteur explained them on the assumption, that during the first attack of a disease some material that was essential for the nutrition of the pathogenic organisms, that had by this time been found to be associated with some of these diseases, had been used up, and the supply being cut off, the organisms were no longer able to exist in the body, and exhibit the characteristic evidences of their presence. This substance must have been present in exceeding small amount, as no alterations in the composition of the blood, or other fluids of the body, could be determined by any methods of chemical analysis that could be applied. Then came the theory advanced by Chauveau and others: that just as micro-organisms, when growing in artificial media, produced excretory products, the presence of which was inconsistent with the continued life of the organism; so in the body, bacteria, during the course of the disease, gave rise to some material which might act deleteriously on their own protoplasm, and which, remaining in the body for a considerable length of time, interfered with the growth of any similar organisms that might in future be introduced. Here again these special chemical products could not be detected in the blood, and must have been present in such infinitesimally small quantities, that it is difficult to see how they could exert any very marked influence on the activity of the bacteria; whilst, as Flugge points out, our knowledge of the actions of the tissues on foreign bodies of various kinds would lead us to the conclusion that any such material would be very rapidly eliminated. Then Grawitz suggested that in any battle between the cells and the bacilli that may occur in the body during the course of a disease, if the cells can but manage to obtain the upper hand and to destroy the bacteria, they should become hardier, as it were, through the training of the contest, their vital energy and assimilating power should be increased, and they should thus become able to deal in a more summary manner with any organisms with which they might afterwards be brought in contact. Then came Buchner's theory of the inflammatory cause of immunity, which offered another explanation. He argued that bacteria made their way into the body at certain special points, these points, seats of infection, differing in different diseases, and that in consequence of the development of the bacteria, there was a reactionary alteration, inflammatory in its nature, in the tissues, which fitted them for the future to resist the special organism that had previously made the attack; this minute alteration in the function of the special cells at the seat of the invasion enabling them to resist the further action and invasion of the same organism even at a considerably later period. Again, based on the same principles as Grawitz's theory, came the now celebrated Metschnikoff theory. Metschnikoff holds that the protection against the attacks of micro-organisms on the body is entirely due to the action of the amœboid cells of the body; that these cells are living pieces of protoplasm; that they are constantly taking into their own substance all foreign particles which find their way into the body; that wherever there is an extra demand on their energies, a large number are attracted to the point at which the work is to be done; and that these cells, acting on the micro-organisms just as they do on foreign bodies, take them up into their substance, digest and convert them partly to their own uses, and gradually throw into the circulating fluids of the body small quantities of effete substances, which are removed by the ordinary physiological channels. Some observers think, however, that the process is not so simple as it would appear; certain bacteria secrete substances which appear to exert a paralyzing effect on the cells, and may so alter them that they are unable to perform their proper functions; whilst on the other hand, the cells secrete in the performance of their work a material which has an unfavourable influence on the activity of the bacteria. This at first sight is an extremely feasible explanation, but when we come to consider more carefully the conditions under which immunity against diseases is conferred, we find that, although in certain cases an attack of one disease protects against an attack of a more serious and deadly malady, this occurs only within certain definite and well-defined groups of diseases; there appears, therefore, to be something more than a mere general protective influence generated within the body. We must have specific powers of resistance developed in or by the cells in order that they may be able to resist specific bacterial activities, and the effects of specific bacterial poisonous products."—*Bacteria and their Products*, p. 369, by Dr. Sims Woodhead.

It is not necessary for the purposes of this work that the question in dispute—*i. e.* as to how acquired immunity is acquired—should be correctly decided, for, since the power of acquiring immunity is manifestly a most important factor in man's present struggle for existence, our subject only demands that we shall note that it must be undergoing evolution in him. But nevertheless, since the matter is of the highest interest and importance, and since it sheds a clear light on the much controverted question of the transmission of acquired traits in very high and very low organisms, we may, with profit, pause to consider it, always remembering, however, that the main tenor of our argument is not affected by this side issue, decide it how we may.

If we hold, as I think we must, that the multicellular organism is a being compounded of unicellular organisms, the cell-descendants, generally speaking, of a pair of conjugated unicellular organisms, which cell-descendants during the ontogeny, unlike the cell-descendants of a pair of conjugated infusorians, remain adherent and undergo morphological and physiological differentiation along definite lines of descent, we may well believe that along certain of these lines the differentiation is such, that the cells belonging to them become functionally adapted to resist and repair any injuries which may befall the organism, whether at the surface or in the interior, whether in the solid tissues or in the fluids such as the blood which nourish and cleanse those tissues. Marvellous as such a functional adaption may appear at first sight, it does in fact occur, and is, after all, less marvellous than the adaption to various other functions of certain other kinds of cells, as, for example, skin, gland, muscle, and nerve-cells which have departed farther from the ancestral amœboid type.

The cells which perform the reparative and scavenging functions are capable of independent movements, and are known as white blood corpuscles when occurring in the blood, and as wandering connective tissue cells when found in the tissues, the two designations probably signifying identical things in different situations, for the white blood corpuscles apparently differ in no respect—neither morphologically nor physiologically—from the wandering connective tissue cells, and have been seen migrating from the blood-vessels into the tissues.[1] When a solid organ is injured—*e. g.* the skin by a cut or a bruise—these cells appear in great numbers at the damaged spot, infiltrate the surrounding tissues, proliferate rapidly, adhere together, and undergo gradual differentiation, till a greater or lesser mass of dense connective tissue, a scar, is formed and healing is completed. Should the injury be severe or prolonged, more or less of them perish from various causes,—*e. g.* the poisons secreted by bacteria,—and the clear lymph is rendered turbid by their dead bodies; that is, pus is formed as in an abscess or on the surface of a sore. Should the blood be injured by the presence of solid impurities non-living—*e. g.* artificially introduced particles of vermilion or carbon—or living—*e. g.* bacteria or protozoa—the leucocytes, called phagocytes from their function, here also perform reparative functions. The impurities, living or non-living, are taken up by them into their substance, and an attempt (successful or unsuccessful as the case may be) is made at digestion; that is, to convert the impurities into food for the cells, or into soluble substances capable of being excreted by such organs as the skin and kidneys. Solid non-living foreign impurities, from the nature of the case, must under normal circumstances be rare in the blood. Living foreign impurities occur in many of the zymotic diseases—*e. g.* anthrax—when the bacteria producing these diseases may often be seen enclosed in the cell-substance of the phagocytes.

The pathogenic micro-organisms of some zymotic diseases—*e. g.* diphtheria and tetanus—do not enter the blood stream, and so pervade the whole body, but remain limited to a definite area of the inner or outer

[1] Other allied but fixed mesoderm cells perform like functions, but it is needless to particularize them.

surface. The "local lesion" caused by them may be, and often is, trivial in depth and extent, yet the whole organism may be powerfully affected. Moreover, it is not necessarily the tissues in the near neighbourhood of the lesion that are most affected, but frequently tissues far distant. For instance, the microbes of tetanus cause in the neighbourhood of the lesion—*e. g.* a wound in the hand or foot, to which they have gained entrance—an inflammation of no great extent, and indistinguishable from inflammations due to other causes; but notwithstanding that the local lesion is so slight, the muscles of the body, beginning with those of the head and neck, and passing to those of the trunk and limbs, are thrown into violent contraction, so that the sufferer, resting perhaps on his head and heels only, lies rigid, bent like a bow by the spasm of his strong posterior muscles. The main, the distinctive pathological effect, is therefore not due to any direct action of the micro-organisms, but to a soluble poison (toxin) produced by them, which enters the blood stream at the point of the lesion, and pervades and poisons the whole body, acting probably, like strychnine, chiefly on certain nerve-cells which have for their functions the control of the muscles. Similarly as regards most other zymotic diseases, whether the micro-organisms enter the blood stream or not, the pathological effects produced are directly traceable, not to the micro-organisms, but to their toxins, as is proved by the fact that if these micro-organisms are cultivated, as in many instances they have been, in a suitable artificial medium outside the body, and then killed or separated—*e. g.* by filtration—from the medium, the latter, since it contains the specific toxins, when injected into the body produces all the distinctive symptoms of the disease.

Now, it has been found in the case of various zymotic diseases of which one attack confers immunity, that if the bacteria of any of them be injected into the tissues of an animal—*e. g.* man—that has not had the disease, and is susceptible to it, the phagocytes of the host appear to be poisoned or paralyzed by the toxins of the micro-organisms, and seem incapable of taking up the latter, or, if they do take them up, of digesting and destroying them, being themselves destroyed instead. This continues till the death of the host occurs, in cases in which a fatal termination ensues; but if recovery follows, it is seen that the phagocytes, becoming habituated to the toxins, gradually acquire the power of attacking and overcoming the bacilli, which are thus destroyed, and the organism freed from them. If now a fresh injection of bacilli be made, it is found that they are at once attacked and destroyed by the phagocytes, in other words, it is found that the individual experimented on has become immune. The duration of this state of immunity, as already explained, is different in different diseases, being short in some diseases—*e. g.* diphtheria—and usually lifelong in others—*e. g.* small-pox. In the latter case it is clear that the cells, which acquired the power of attacking the micro-organisms in the presence of their toxins, transmit this power to their descendants, acting therein like simple unicellular organisms; whereas in the former case—*i. e.* when the immunity is of short duration—the acquired trait is not transmitted, or is transmitted in a rapidly diminishing extent for a few generations only; or when the duration of the immunity is very short, it is even lapsed by the very cells which acquired it.

The above explanation of acquired immunity—that it results from a fit variation, in response to appropriate stimulation, of the cells (phagocytes) belonging to certain lines of the cell-descendants of the conjugated ovum and spermatozoon, whereby they are (and as a consequence the whole organism is) brought into completer harmony with an environment that has been rendered more complex by the presence of pathogenic microbes—is in full accordance with the theory of evolution set forth in the preceding pages. The variations which the phagocytes undergo, when they acquire powers of attacking pathogenic micro-organisms, which were before invulnerable to them, are strictly comparable to the variations undergone by other kinds of cells under appropriate stimulation; for example, to the variations undergone by skin-cells when subjected to oft-repeated heat or friction, as in the blacksmith's hand, whereby they are able the better to resist heat and friction; to the variations undergone by muscle-cells, when, as a result of training, they acquire the power of contracting long and vigorously; to the variations which the eye-cells undergo in an educated man, who without inconvenience can for hours scrutinize small print, a proceeding that would render the eyes of an uneducated man dim and painful in a few minutes; to the variations undergone by the cells of various other tissues, whereby toleration is established against nicotine, arsenic, &c.; in short, to the variations undergone by this or that kind of cell in every variation by means of which the organism is brought into harmony with the environment, for any such variation is after all nothing other than a variation, quantitative or qualitative, or both, of certain of its component cells. Moreover, the theory incidentally affords a valid explanation of the otherwise inscrutable fact, that acquired immunity against any zymotic disease protects against that disease only and against no other. *À priori*, we might suppose that the acquired powers which enable the phagocytes to attack and overcome the micro-organisms of any disease, would be available, in some degree at least, against the micro-organisms of all other diseases, but experience proves that this is not to any extent the case. An attack of measles, for instance, does not at all protect against scarlatina. But when we learn that acquired immunity against any disease is due to a power acquired by the phagocytes of withstanding the toxins of that disease, and when we remember, in this connection, that the toxins of any disease must be different from the toxins of every other disease, since their effects on the organism are so different, we are able to understand why acquired immunity against any one disease holds good against no other.

CHAPTER II

There is, however, an objection to the phagocytic theory of acquired immunity, which at first sight appears fatal, but which I think is capable of being overcome. Pasteur found that in a man bitten by a rabid animal, the onset of hydrophobia, or at any rate its fatal termination, could in a great proportion of cases be prevented by injecting into the tissues of the patient emulsions made from the spinal cords of animals dead of that disease, beginning with an emulsion from a cord that had been dried for fifteen days in a closed vessel over caustic potash, whereby it was in a great measure bereft of moisture, and the pathogenic organisms were probably quite destroyed, continuing day by day with emulsions from cords of animals more and more recently dead, from which, therefore, the moisture was less and less thoroughly abstracted, and in which the pathogenic life was less and less completely destroyed, and ending with a cord which was absolutely fresh and therefore virulently infective, as could be proved by inoculating a susceptible animal with it and producing fatal disease. It is to be presumed that the pathogenic organisms were absent or almost absent from the first injected, the old and thoroughly dried cord, but that their toxins were present in it, and that in the fresher cords, in direct proportion to their freshness, the organisms were present as well as their toxins.

But when the treatment is begun the infected individual has already within him the disease germs and toxins; by adding to these germs and toxins we might expect on *à priori* grounds that the rapidity of the disease would be increased, and the fatal result rendered more certain, since the phagocytes would have less time wherein to vary fitly—to acquire the power of destroying the microbes of rabies in the presence of their toxins. The contrary, however, is the case, for under such treatment the infected individual usually recovers. This, then, is the difficulty which must be surmounted before the phagocytic theory of immunity can be accepted.

When under fit conditions yeast is introduced into a solution of grape sugar, that micro-organism multiplies rapidly. Concurrently with its multiplication, and in proportion to it, the sugar disappears, and is replaced by alcohol. Whence we may fairly conclude, that the sugar is used as food by the micro-organism, and that the alcohol is an excretion, an effete waste product, comparable to the waste products present in the excretions—*e.g.* urine—of higher animals. Like those waste products, it is more or less poisonous to the organism that produces it, as is proved by the fact that, when the alcohol reaches a percentage of fourteen in the solution, the yeast ceases to multiply and perishes. It was at one time thought the behaviour of yeast in a solution of sugar afforded an explanation and an illustration of the way in which immunity against microbic disease is acquired. The micro-organisms of disease were believed to produce waste products, poisonous to themselves, which were retained as in a bottle by the host, and which, therefore, when they attained a certain strength, wrought the destruction of the micro-organisms that produced them, and rendered the infected individual immune for the future, or at least for so long as the waste products were not excreted and were retained unchanged in the blood and tissues. Pasteur's memorable experiments on rabies were held to confirm this view. It was thought that his thoroughly desiccated cords contained the waste products, but not the living organisms of hydrophobia, and that the rationale of his treatment of that disease lay in disproportionately increasing the waste products as compared to the microbes in the blood and tissues of the infected person.

There is perhaps a modicum of truth in this theory, for the blood serum of animals which have acquired immunity through illness against certain diseases—*e. g.* cholera, erysipelas, anthrax, diphtheria, &c.[1]—has greater bactericidal power than the serum of animals yet susceptible; but that the waste product theory is utterly insufficient to account wholly for acquired immunity is decisively proved by the fact that the micro-organisms of certain diseases are able to flourish in the serum withdrawn from immune animals, which should of course contain the waste products if they exist in them. The anthrax bacillus is a case in point, and it has been found, moreover, that this micro-organism will live and grow in the anterior chamber (in which there are normally no phagocytes) of the eye of an immune animal, where also the waste products if present in the animal should likewise be present. A different interpretation of Pasteur's experiments must therefore be sought.

Pasteur furnished the key to the problem by some other experiments. He found that the virus of rabies is of constant strength in dogs.

"And inoculations made from dog to dog kill the animal with the same incubation period, the same

[1] *Vide* Kanthack, *British Medical Journal*, February 20, 1892.

symptoms, and practically in the same time. When inoculated from the dog to the monkey, however, the virus becomes less virulent; it is said to be attenuated or weakened, the attenuation becoming more and more marked in successive inoculations from monkey to monkey; the course of the disease becomes longer and longer, until eventually there may come a time at which the virus, when introduced under the skin or into the cranial cavity, is not sufficiently active to cause the death of this species. If this attenuated fluid be now inoculated into a rabbit, a dog, or a guinea-pig, it still remains comparatively weak for a time, through successive inoculations on these animals—*i. e.* at first it does not kill, then it kills, but only after a considerable time; but gradually the virulence returns, until at last it reaches its original level of malignancy, whilst if the successive inoculations are made in rabbits with primary fluid from either the dog or the monkey, the virulence may become so exalted that it is considerably greater even than that of the virus taken from the street dog, which at one time was supposed to be the most virulent form except that of hydrophobic wolves, which has always been known to be specially fatal; the virulence is doubled as the inoculation period is reduced to one-half."—Woodhead, *Bacteria and their Products*, p. 320.

"Pasteur . . . subjected the bacilli" (of anthrax in a pure culture, *i. e.* in an artificial nutrient medium free from other kinds of micro-organisms) "to a temperature of from 42° to 43° C.; these were found to have lost all their vitality at the end of about six weeks, this loss of vitality during the six weeks going on progressively in proportion to the rise of temperature. It is stated that at the outset the pure culture had all the virulence of anthrax blood; whilst only half of the sheep inoculated with the culture that had been heated for ten days succumbed to anthrax. On the twenty-fourth day of heating, the culture, when inoculated, although giving rise to mild febrile disturbance, did not cause the death of a single animal. It was found, too, that if now, twelve days after the first inoculation, these surviving animals were inoculated with a culture from the twelfth day, which before had killed half the animals, there was still a slight febrile disturbance, and none of the inoculated animals died. Virulent anthrax blood might, after a further interval of twelve days, be introduced into animals that had been subjected to the double inoculation, without giving rise to anything more than a slight febrile condition similar to that noticed as resulting from the inoculation of the modified virus. If, however, virulent anthrax blood were introduced into animals, in which only the first protective inoculation had been made,—*i. e.* with material that had been cultivated for twenty days,—a large proportion of the animals died. It was evident, therefore, that it was absolutely necessary to use both a first and a second vaccine if this protection was to be complete. This attenuation was not confined to the generations of the bacilli that were directly acted upon. If the temperature were lowered to about 35° C., vegetative activity was immediately set up, rods in enormous numbers were formed, and eventually spores might be observed in these rods. Now comes the interesting fact: the attenuated properties of the original bacilli were handed on to the spores; these spores might be kept in a latent condition for a considerable length of time, and on being introduced into media suitable for their growth, they sprouted out, *not into virulent anthrax bacilli, but into modified anthrax bacilli,* so that the conservation of the vaccine (on silk threads) became a comparatively easy matter."—*Bacteria and their Products,* pp. 372-3.

Judged then by the most delicate of our tests, the effects of its toxins on living creatures, the anthrax bacillus is not only modifiable by a changed environment, but its acquired traits are transmissible to its descendants. We must admit this, or hold as the only alternative, that the progressive modification of the microbe in the changed environment is due to a process of natural selection among them, causing evolution in a changed direction by the accumulation of inborn variations of a kind that in the former environment did not conduce to survival. It is highly improbable, however, that the latter theory correctly explains the facts, (1) because the time in which the modification is obtained is apparently too short to admit of its arising as a result of the survival of the fittest, and (2) because we have no reason to doubt that acquired traits are transmissible in the microbes, since they are all very low unicellular organisms, each one of which, since it is capable of continuing the race, is to be regarded as a germ cell on which the environment acts directly. The point, though of great interest, is, however, of theoretical importance only. The fact remains, that the bacilli of anthrax, whether by the transmission of acquired variations or by the accumulation of inborn variations, or both, are easily to be modified by changing the environment; and this has been proved true, not only of the bacilli of anthrax, but of the microbes of numerous other diseases as well, including the microbes of rabies, as was shown by the experiments above cited, in which Pasteur demonstrated that while these microbes, when cultivated through a succession of dogs, are fatal to the dog and other susceptible animals, when cultivated through a succession of monkeys induce only a transitory illness in dogs, and when cultivated in a slowly drying cord do not prove fatal to human beings.

We are now in a position to attack the difficulty we encountered a few pages back—to explain how it is that in a man bitten by a rabid dog, hydrophobia is not intensified, but cured by a treatment which begins with the injection of material from the desiccated cord of an animal dead of that disease, proceeds with the injection of fresher and fresher infective material, and ends with the injection of the most virulent material.

We have seen that the bacilli of anthrax when exposed to a temperature of from 42° to 43° C., gradually lose their virulence and at length cease to cause death; in other words, the toxins which they manufacture gradually become less and less poisonous, till they become so little poisonous that the phagocytes of the infected animal are able to vary fitly in response to appropriate stimulation, that from the toxins, and become capable of destroying the invading bacilli in spite of the presence of those toxins. We have seen also that if we proceed step by step, first injecting into the tissues of a susceptible animal bacilli of little virulence, then after an interval bacilli of greater virulence, we are able at length to confer immunity against the bacilli of the greatest virulence—of virulence so great that they would have infallibly destroyed the animal experimented on had it not been protected by the previous inoculations. The rationale of the process is then clear; the stimulation from the feeble toxins of the first injected bacilli induce such a reaction, such a fit variation in the phagocytes of the host, that they are thereby placed in a position of advantage, owing to which they are able to vary fitly in response to stimulation from the stronger toxins of the second injection, which, but for their position of advantage, would destroy or paralyze them. In like manner, from this second position of advantage they are again able to vary fitly still farther in response to the stimulation of the toxins of the third and most virulent injection. So skin-cells when subjected to long-continued heat or friction vary in response in a fit direction, till they are able to withstand degrees of heat and friction which would be fatal to them had they not attained a position of advantage. So cells of other kinds by an analogous process of slow training are able to withstand nicotine, opium, alcohol, arsenic, &c., when present in their nutrient fluids in a degree of concentration that destroys or paralyzes untrained cells of the same kinds. When Pasteur for his first inoculation for the cure of rabies uses materials from an old desiccated cord, the essentials of his injection are not "waste products," but extremely attenuated toxins—toxins so attenuated that the phagocytes, far from being destroyed or paralyzed, react in response to the stimulation from them, and are thereby placed in a position of such advantage, that instead of being destroyed they are able to react in a similar manner to the stimulation of the more virulent toxins and the microbes producing them of the next inoculation, whereby they attain a position of still greater advantage, and are able to withstand and react to the stimulation of the toxins of the third injection, and so on in succession till the toxins of the most virulent injection, which would otherwise have destroyed or paralyzed them, to which they could not have reacted at one effort, produce no other effect than another reaction of like kind, by virtue of which they are able to withstand those toxins, and to attack and destroy the micro-organisms that produce them. Rabies, as we know, is of slow development in man—*i. e.* the microbes increase slowly in him, or at the least they produce their toxins slowly in him, one or both, for it is not till weeks or months after infection that he exhibits the characteristic symptoms of the disease. This long incubation period renders possible the success of the scientist in his endeavours to save life—affords time during which he is able to train the phagocytes to successful battle.

Syphilis resembles rabies in its slow development, and therefore as regards it also we may hope or expect that means will be found of attenuating the virus, perhaps by passing it through a series of resistant animals, or better still, by subjecting a pure culture, if such can be made, to the influence of an altered environment in an artificial medium; but great difficulties will lie in the way of success by the latter method, since syphilis, so far as we know, is a purely parasitic disease, never a saprophytic one; that is, the micro-organisms which produce it are capable of existence in a living host, but not, so far as we know, in non-living nutrient media. The difficulty in the way of success by the former method lies in the fact that we know of no animal capable of taking the disease and attenuating it.

In diseases which have a shorter incubation period than rabies—*i. e.* in which the toxins are more rapidly produced—but in which the micro-organisms do not pervade the body, but are localized in a definite area of limited extent—*e. g.* in diphtheria and tetanus—it is only reasonable to assume that the toxins elaborated by them, however much they may paralyze or destroy the phagocytes in the immediate neighbourhood of the local lesion, where they are in a state of concentration, however much in the concentrated state they may ward off the attack of the phagocytes on the microbes, do not at first destroy or paralyze the phagocytes in the blood and tissues at a distance where they are in a state of great dilution; and therefore, as regards these diseases also, we may hopefully attempt to cause recovery and produce acquired immunity by introducing, before the virulent toxins reach such a degree of concentration as to destroy or paralyze the phagocytes, less virulent toxins obtained from animals—*e. g.* horses—which "attenuate" their microbes, as the microbes of rabies are attenuated in the monkey, and as the microbes of small-pox are attenuated in the calf, or else from artificial cultures, in which the microbes are attenuated, as the microbes of rabies are attenuated, in a slowly drying cord, and as the microbes of anthrax are attenuated when subjected to a temperature of from 42° to 43° C.

But in diseases in which the incubation period is short, and in which the microbes pervade the whole body, any attempt to produce immunity by inoculation with a weaker virus, *after* the host has been infected by the stronger virus of the normal disease, must necessarily be vain. In such a case the phagocytes, paralyzed or destroyed by the stronger toxins, are unable to react in response to the stimulation from the weaker toxins, and death ensues; or if death does not ensue it is because the phagocytes have been able to react from a position of no acquired advantage to the stimulation of the strong toxins. So skin-cells in the presence of great heat disregard and do not react to the stimulation of lesser heat (which then but adds to the effect of the other), or if they do react at all and are not destroyed, react in response to the stimulation of the greater heat. So other kinds of cells in the presence of large doses of opium, tobacco, alcohol, &c. (toxins in fact) disregard the action of smaller doses given shortly after (which indeed but supplement the action of the greater doses), and perish, or if they do not perish, but react, react to the stimulation of the stronger doses. Therefore in such a disease as small-pox, in which the toxins are rapidly produced, and in which the microbes pervade the whole body, protective inoculation after infection is useless. To be effectual it must be made at a time antecedent to infection, which is what is done in vaccination against small-pox; at the least if made after infection it must be made at a time antecedent to the appearance of symptoms of illness—*i. e.* during the incubation period, before the virulent toxins have been elaborated,—and then it will be successful only if the attenuated microbes elaborate their toxins much more rapidly than do the virulent microbes, a thing most unlikely to happen.

As regards a fourth group of zymotic diseases, we can hardly hope anything from the protective inoculation of attenuated microbes, either before or after infection. Included within this group are tuberculosis and leprosy, which run a prolonged indefinite course in the infected individual, and against the microbes of which the phagocytes never seem to acquire markedly increased powers, behaving in this respect like other kinds of cells when subjected to the action of certain vegetable and mineral poisons—*e.g.* digitalis and mercury—which they are never able to tolerate the better for training. In leprosy and tuberculosis the toxins, judged by their systemic effects, apparently play a very minor part, at least when compared to the part played by them in other diseases—*e. g.* diphtheria and anthrax. The battle here is not carried on, so far as has been ascertained by the pathogenic organisms, at long range, but is to all intents and purposes a physical struggle between them and the phagocytes, for at all stages of the disease the former may be seen enclosed in the cell-substance of the latter. In leprosy the microbes are invariably or almost invariably the victors, since never, or very rarely, does a sufferer recover from that disease. In tuberculosis the phagocytes are often victorious, since recovery from this disease is common. But victory does not endow the cells with increased power, as it does in such diseases as small-pox, in which the toxins are abundant; stimulation does not cause them here to vary in a fit direction, for he who has once suffered from tuberculosis is as liable as ever to be attacked by the disease. Immunity from tuberculosis and leprosy is therefore entirely or almost entirely of the inborn inherited kind. Some individuals seem immune under any circumstances; others are ordinarily immune, but are liable to be attacked when from any cause their vitality is lowered, when their phagocytes are less fit for the battle; others again seem quite unable, even under the most favourable circumstances, to resist infection; their phagocytes succumb easily, and in them the disease usually runs a rapid course, and systemic death quickly ensues.

The fact that, in such a country as England, where tuberculosis has been prevalent for unnumbered generations, so many people are inherently immune, is obviously related to the fact that immunity cannot be individually acquired against it, for, unlike the case of such a disease as measles, against which immunity may be acquired, inborn immunity is here pushed to its extreme by Natural Selection as the sole means to survival in the presence of the disease. It appears, therefore, that in the case of diseases the micro-organisms of which do not paralyze the phagocytes with powerful toxins, but engage from the outset in a physical struggle with them, that immunity cannot be individually acquired, and, therefore, that protective inoculation is useless; but in the case of diseases, the less hardy microbes of which, engaging at long range, produce powerful toxins by means of which they paralyze and keep at a distance the phagocytes, immunity may be acquired, if under normal circumstances the reaction, whereby the phagocytes acquire the power of disregarding the toxins, and so of attacking the microbes, is not too great to be accomplished at one effort; and even when, under normal circumstances, the reaction is too great to be accomplished at one effort (as in rabies), it may be brought about by one or more protective inoculations, by means of which the phagocytes are enabled to accomplish in a shorter or longer series of efforts that which they are unable to accomplish at a single effort.

Against these diseases protective inoculations of attenuated microbes or of their toxins are therefore useful. But owing to the circumstance that the duration of acquired immunity is different in different diseases, from practically life-long immunity in some diseases —*e. g.* scarlatina and small-pox—to immunity which endures for but a few days—*e. g.* relapsing fever—the degree of utility that may be hoped for from protective inoculations varies. In small-pox and kindred diseases, in which the duration of immunity is long, it may be most useful; in relapsing fever and kindred diseases, in which the duration of immunity is short, it is much less useful, except, indeed, in the cases of such dangerous diseases as diphtheria, which saturate the host with their toxins slowly, when inoculation by attenuated microbes or their toxins helps the infected individual to tide over a dangerous crisis, and enables his phagocytes by a series of efforts to accomplish a reaction which they may be unable to perform at a single effort. The quickness and completeness with which the immunity is produced, though not the duration of it, seem in direct proportion to the quickness with which the toxins are produced, and to their degree of virulence. To put it in another way: Natural Selection has so dealt with the micro-organisms of some diseases—*e. g.* small-pox, measles, &c.—that there has been an evolution in them of the power of producing toxins as their chief means of combating the phagocytes; in which case the phagocytes must react quickly or perish; if they do react quickly they find the feeble microbes an easy prey. But as regards diseases in which the toxins are neither quickly produced nor virulent—*e.g.* tuberculosis—Natural Selection has so dealt with the pathogenic micro-organisms that there has been an evolution in them of what may be called personal vigour as their chief means of combating the phagocytes. In such diseases the struggle between the microbes and the phagocytes is always prolonged, and may last for years. Victory is decided for or against the phagocytes accordingly as Natural Selection has developed in them or in the micro-organisms the greater personal vigour. When the personal vigour of both parties is evenly balanced the result of the combat is decided by circumstances, which raise or depress the vitality of the phagocytes, such as an improvement or deterioration in the blood plasma, their nutrient fluid. In the later stages of disease this must be so much deteriorated that the phagocytes are at the greatest disadvantage.

As regards relapsing fever, the phagocytes quickly react to the stimulation of the toxins and destroy the bacilli, but the acquired power is soon lapsed, hence the relapses that occur in this disease, and also the fact that one attack does not afford immunity against subsequent attacks. Here also we have therefore nothing to hope from protective inoculation. As, however, the disease only attacks individuals whose vitality has been reduced to a low ebb (by starvation, for instance), it appears that the untrained phagocytes of the healthy body are fully able to cope with the bacilli. It is probable that during the intervals of convalescence which alternate with the relapses, the pathogenic organisms are not entirely absent from the infected body, but that some of them at least are present as harmless but highly resistant spores, which regain their pathogenic but lose their resistant properties when the relapse occurs—*i. e.* when the phagocytes lapse their acquired powers of tolerating the toxins.

Of late Behring and others have advanced the theory that acquired immunity is due to the production by the host of anti-toxins, that is, of chemical substances which neutralize or destroy the toxins, and which presumably persist in the blood and tissues of the host as long as the immunity lasts. It is, however, highly improbable, not to say impossible, that this theory correctly explains the phenomenon, because, since the toxins of every disease differ from those of every other disease, as is proved by the difference of their effects on the organism, this would mean that the host is capable of elaborating separate anti-toxins, highly complex chemical substances, exactly antagonistic to the different toxins of all the separate zymotic diseases to which he is susceptible, and against which he is able to acquire immunity; a thing wholly incredible, especially when we remember that these anti-toxins may, if the theory is true, be manufactured by individuals whose ancestry can have had no experience of the disease, and in whose race therefore there can have been no evolution of the power of such manufacture; for instance, the horse is resistant to diphtheria, and is able to acquire immunity against it, yet we have no reason to suppose that the equine race has in past ages been afflicted by the disease; again, the natives of Australia and the islands of the Mid Pacific are able, though less able than the inhabitants of the Old World, to acquire immunity against such Old World diseases as measles, scarlatina, small-pox, &c., yet, as we know, these diseases are new to their races. Moreover this theory of immunity by the production of anti-toxins is unnecessary, since the theory of immunity by fit variations sufficiently explains the phenomena. We can well believe that in the horse and in the Samoan the amœboid leucocytes have retained the phagocytic functions of their remote amœboid unicellular ancestors, and that in these cells there has been an evolution of the power of varying in a fit direction in response to stimulation, whereby they are able to react to stimulation from the toxins, even though these toxins are strange to them and to their ancestry, just as other cells of the body are able to vary in a fit direction when stimulated, even when the stimulation is strange, this being the especial means by which higher organisms are brought into harmony with an exceedingly complex environment; but it is highly incredible that phagocytes or any other cells, when acted on by this or that toxin, should suddenly develop the power of producing this or that anti-toxin, a complex chemical substance which exactly antagonizes another highly complex chemical substance, the particular toxin presented, but no other. It would be as reasonable to attribute the resistance offered by trained skin-cells to heat to the production of an anti-pyretic substance, or the resistance offered by other kinds of trained cells to the action of nicotine, alcohol, opium, arsenic, &c., to the production of specific anti-toxic substances.

When therefore the toxins (freed from the micro-organisms) of diphtheria are injected into the horse, it is highly improbable that there is elaborated in that animal an anti-toxic substance which exactly antagonizes the poison, and which, dissolved in his blood-serum, when injected into man, for that reason renders the latter resistant to the disease. On the contrary, it is probable, since the injection of immunized blood-serum apparently does tend to produce increased powers of resistance in man, that the toxins are altered in the horse, perhaps by intracellular digestion, as suggested by Dr. Hunt;[1] are rendered less virulent, are so altered that the phagocytes of the man are able to react to them, and thus to attain a position of advantage, whence they are able to react to the unaltered virus, to accomplish by successive efforts that which they are unable to accomplish at a single effort. It is very improbable that the animal body is a sort of "magic bottle" which produces particular anti-toxins at need, but very probable, judging by analogy, that its cells are able to vary gradually so as to adapt themselves to that change in

[1] *British Medical Journal*, September 16, 1893.

their environment which is caused by the presence of this or that toxin, and that this adaption is more readily achieved when it is accomplished by degrees—*i. e.* by a series of efforts.

Of course it is possible that skin-cells, muscle-cells, nerve-cells, leucocytes, &c. become adapted to changes in the environment through a process of Natural Selection, through the survival of the fittest among them, and that they do not really react to stimulation, but that the unfittest among them perish, leaving in each succeeding cell-generation the more and more fit to continue the race—*i. e.* that they do not transmit acquired traits any more than multicellular organisms do, but undergo evolution only through the accumulation of inborn variations. But this also is highly improbable, since the adaptive change is often accomplished in a time which is apparently too short to admit of such evolution among the cells; for instance, it is hardly possible that acquired immunity against small-pox is due to evolution among the phagocytes owing to the continued survival of their fittest. On the other hand, it is possible that this adaptive change in the cells is due to the survival of the fittest among their biaphors; that is, to the survival of the fittest among those hypothetical units which are supposed to stand in the same relation to the cell as the cell stands in relation to the multicellular organism.

CHAPTER III

We may now resume the main thread of our argument, and seek to confirm inductively the conclusion we have already arrived at deductively; namely, that the present evolution of man is chiefly against zymotic disease. We have not, however, quite done with deduction.

Adopting a different classification from that which we have heretofore found useful, zymotic diseases may be divided into three classes.

The first class includes those diseases of which the pathogenic organisms are entirely parasitic, and are capable of existence only in living tissues, or for a short time, before morbid changes of importance occur, in the previously infected tissues of dead animals, including man—*e. g.* rabies; practically, however, since the death of the microbes so soon follows that of the host, the former may be considered as capable of existence only in living tissues. They are, therefore, never earth, air, or water-borne, but are acquired by the healthy only through actual contact with the diseased, and then only under special circumstances, since the cells at the surface of the body are normally quite able to repel the invasions of their microbes. They are therefore contagious in the strictest sense of the word, and therefore—since conditions of temperature, moisture, &c. in the bodies of men are much the same all the world over—they are quite independent of climatic and telluric influences, and may prevail anywhere. Of these diseases only syphilis need concern us, since it alone is sufficiently prevalent to be a cause of evolution.

The second class includes those diseases of which the pathogenic micro-organisms are entirely parasitic, but are capable of maintaining existence for a limited time, as resting spores or otherwise outside the living tissues. Tuberculosis and the acute exanthemata are examples. Normally they are always earth, air, or water-borne, though they may be communicated by actual contact,—*e.g.* by a kiss,—and therefore, since their microbes are incapable of maintaining existence for an unlimited time outside the living tissues, it follows that they are essentially diseases of crowded populations. In sparsely inhabited countries they must tend to die out under normal circumstances from failure of their food supply, and even when the infection is periodically renewed from outside, the inhabitants of such territories must enjoy a greater freedom from infection than the inhabitants of more populous lands where these diseases are endemic. This freedom from infection, other things equal, will be greatest when the country is inhabited by barbarous tribes hostile to one another, as in Australia at the present day, or in North America during the past, for this mutual hostility gives rise to what is practically a very strict system of quarantine. A very important point to be noted in connection with disorders of this class is that, since the microbes, before they can infect a fresh host, normally must exist for a time outside the body, such diseases cannot be prevalent unless the external environment present conditions favourable to the existence of the microbes as well as for their conveyance to other hosts, conditions which are not always satisfied even in populous countries. For instance, as regards tuberculosis, the conditions favourable to its prevalence are not well satisfied in countries where the heat renders necessary the admission of large volumes of fresh air into the dwellings of the inhabitants, nor in colder countries, where the art of building has so little advanced that large volumes of air enter the dwellings in spite of the mistaken endeavours of the inhabitants. Again, as regards scarlatina, undiscovered causes in the environment are unfavourable to the existence of the virus in Arabia; the disease is therefore almost unknown in that country, though its inhabitants are not immune when they travel abroad.

In the third class are included all zymotic diseases of which the microbes are able to subsist for an indefinite time outside the living body on non-living organic matter, the microbes of which, in fact, are essentially saphrophitic, and have their normal habitat outside the living body, but are capable of existence, should occasion serve, within it. Examples of such are the malarial fevers. Unlike the diseases of the first two classes, the prevalence of their germs in any territory is quite independent of the human population, except in so far as it affects the outside conditions through drainage, deforestry, &c., and therefore the inhabitants of the most sparsely peopled lands may be afflicted by them.

These three classes of diseases are not sharply defined from one another, but shade, the first into the second, and the second into the third. From diseases, the germs of which are practically incapable of existence outside the living body, to diseases, the germs of which have their normal habitat external to it, every degree of ability to exist outside the body may be observed. The microbes of some diseases,—*e. g.* syphilis,—practically speaking, cannot exist outside the living body; the microbes of others—*e. g.* tuberculosis—can exist, but cannot multiply outside it; the microbes of others—*e. g.*

cholera—can not only exist but can multiply, though apparently only to a limited extent, outside it, while the microbes of yet others—*e.g.* malaria—can not only exist and multiply outside it, but can multiply there to an unlimited extent.

Of the microbes of cholera, and probably also of typhoid, it is interesting to remark, that though it seems they are able to multiply for a period in contaminated fluids, yet in time they always perish, unless they find a home again in a human host, as appears to be proved by the fact that travellers are never infected by them in long-deserted districts. That they are truly parasitic when in man, in the sense that they find their nutriment essentially in his living tissues, not in the non-living contents of his bowel, is proved by the circumstance that he is able to acquire immunity against them; or rather, it is proved by the fact that after he has once acquired immunity the microbes of the disease are no longer able to flourish in the contents of his bowel even should they gain fresh entrance. Reasoning from the analogy of other diseases, acquired immunity in this disease can only be due to the phagocytes acting from a "position of advantage" on the microbes within their reach, which of course would not be those that lay free in the bowel, mixed with its contents; the only other rational theory here being one which supposes that it is due to the secretion, by the cells lining the intestinal tract of an immune person, of a substance poisonous to the microbes; an untenable theory when we take into consideration the fact that immunity may be acquired by individuals whose ancestry can have had no experience of the disease, and therefore cannot have developed the power of secreting this particular complex chemical substance, this species of anti-toxin—not only exactly when it was first wanted, *i.e.* when infection first took place in the individual, but ever after.

The purging which accompanies the disease may possibly quite clear the bowel of one recovering from it of the bacilli, but after recovery such a one would be as likely as any other again to swallow infected material, when purging would not again occur; at least it has not been found that persons recovered from cholera, and immune to it, are liable to diarrhœa during an epidemic, and that their discharges are infective. If then an immune person swallowed infected material, and if the contents of his bowel afforded all-sufficing nutriment, in which the bacilli were able to multiply to an unlimited extent, his bowel would become the home of a colony of bacilli, which, owing to the constant renewal of their food supply, and the removal of their waste products, would multiply perpetually, and would perpetually infect his alvine discharges, to the danger of the community; unless indeed, which seems improbable, when we consider the enormous rate at which the bacilli multiply, the churning movements of the intestine so certainly brought all the bacilli within reach of the phagocytes, now invulnerable to them, in the wall of the bowel, that the latter were able to seize and destroy them. But we know that no person who has recovered from cholera ever becomes a source of perpetual danger to the community, and we must conclude therefore that, even if the bacilli are capable of multiplying to a limited extent in the non-living contents of the bowel, that, unlike some harmless species of micro-organisms that normally inhabit it, they are not capable of multiplying to an unlimited extent there, but must periodically pass a time and find their nutriment in the living tissues of susceptible individuals.

The truth, therefore, appears to be, that the microbes of cholera find their nutriment in the living tissues of the host, but when the conditions of existence become unfavourable there—*i. e.* when the phagocytes react to the stimulation of the toxins, and acquire the power of attacking the microbes—these latter, owing to a trait which the survival of the fittest has developed in the species, leave the host in the discharges, and maintain, if they do not perish, a purely saprophytic existence outside him till such time as, if fortune serve, they are taken into the body of another and a susceptible host. It should be remembered, moreover, since a human host is essential to the persistence of the cholera bacilli, that if they found their nutriment in the contents of the bowel merely, if they were purely saprophytic, it would be against their specific interest to secrete a poison which would cause the death of the host that supplied the means of subsistence. The law of the survival of the fittest would, in that case, cause them to become as innocuous to the host as the other micro-organisms which normally inhabit his alimentary tract. We may conclude then, that the cholera bacilli are not saprophytic, in the sense that they find their nutriment solely in the non-living contents of the bowel, but that they are parasitic, in the sense that they find their nutriment in the living tissues lining the bowel. The point, however, is capable of being tested experimentally, for if the bacteria are saprophytic in the sense that they find their nutriment in the *contents* of the bowel, then the alvine discharges of an immune person should have bactericidal powers as regards them, whereas the alvine discharges of a susceptible person should form an excellent culture medium; but if they are parasitic, if they find their nutriment in the *living tissues* lining the bowel, then the alvine discharges from immune and susceptible persons should have equal bactericidal or nutritive powers.

But while we may safely assert that the contents of the bowel do not normally present all the conditions under which the cholera bacilli are able to multiply to an unlimited extent, and while I think we may safely assert the same of other nutrient media normally present in the outside world, it would be too much to assert that no nutrient medium exists, or can exist, in the outside world, in which they are able to multiply to an unlimited extent. Indeed Dr. Koch believes that the mass of decaying organic matter accumulated in the delta of the Ganges presents such a nutrient medium, in which they do multiply to an unlimited extent, and whence they issue forth to cause those epidemics which afflict more favoured regions. It is, however, a question whether, in the total absence of a human population in the basin of the river and its tributaries, the pathogenic organisms would persist, as the microbes of malaria persist under similar circumstances.

It is noteworthy that diseases, the microbes of which are little capable of existing outside the human body, are much more stable in type than diseases, the microbes of which are more capable of so existing.[1] For instance, syphilis at one end of the scale, for people of the same race, does not vary much in relation to time, except when such time is of very long duration; nor in relation to space, even when such space includes the most distant parts of the world. But malaria, at the other end of the scale, varies much in relation to both time and space, since, for people of the same race, in the same locality, it may be of a mild type in one year and of a severe type in the next; or for people of the same race and at the same time, it may be mild in one locality and severe in another. Of diseases intermediate in the scale we find the same to be true. Scarlatina, small-pox, and measles, the microbes of which are little capable of existing outside the body,

[1] White, *British Medical Journal*, Feb. 25, 1893.

vary little in type for people of the same races in different times and places; whereas diphtheria, typhoid, and cholera, the microbes of which are more capable of existing outside the body, vary much more in relation to time and space. This is of course exactly what might be expected on *à priori* grounds. For since the microbes of disease are unicellular organisms, and as such presumably capable of transmitting acquired traits in whole or part to descendants, it follows that diseases, the microbes of which exist under the more stable conditions, should be more stable in type than diseases, the microbes of which exist under less stable conditions; and therefore that such a disease as syphilis, the microbes of which exist solely in the human body, under conditions of temperature, moisture, &c. that are constantly nearly the same, should be more stable in type than such a disease as malaria, the microbes of which normally exist outside the body under conditions of temperature, moisture, &c. that are constantly changing; so also diseases intermediate in the scale—*e.g.* measles and scarlatina, cholera and diphtheria—should approach syphilis in stability, or malaria in instability, accordingly as the environment in which the pathogenic organisms exist resembles that of the microbes of syphilis in its equability, or that of malaria in its changefulness.

But, so far as we are able to institute comparisons, all zymotic diseases, even the most stable, show differences of type when attacking different races of men, always being least severe as regards each disease in races that have had much and prolonged experience of it, and most severe in races that have had little or no previous experience of it. Thus measles, scarlatina, small-pox, tuberculosis, syphilis, malaria, &c., are comparatively mild in type when attacking races that they have long afflicted, but comparatively severe in type when attacking races that they have little or not at all afflicted. Judging by current literature, this difference of type is generally regarded as something altogether inexplicable, or sometimes it has been said that this or that race has become acclimatized to this or that disease, when the common and very foolish error has been made of imagining that a phenomenon named is a phenomenon explained. To say that a race has become acclimatized to a disease—*i.e.* that it has become more resistant to it—does not explain how that increase of resisting power arose. When in rare cases an attempt has been made to probe deeper, it has generally been on lines of the Lamarkian doctrine of the transmissibility of acquired traits[1]; and as often as not it has been made by thinkers who possibly never heard of Lamark, and who would repudiate the logical outcome of their own arguments—the idea of evolution. Starting with the fact, that in many zymotic diseases one attack confers immunity against subsequent attacks, it has been tacitly assumed, if not expressly asserted, that this acquired immunity, or some of it, is transmitted from parent to child, each successive individual of the line beginning life with more resisting power than his parent began with, and transmitting more to his offspring than he received. But besides the general considerations which lead us to deny the transmissibility of

[1] "The opinion has been expressed that syphilis becomes milder in communities in which it has long been present. It is reputed to have become in Portugal a much enfeebled malady from this cause. An English physician practising there has expressed his belief that, owing to the habitual neglect of efficient treatment, the whole community has become influenced. We know that this law of transmitted partial immunity prevails in the other specific fevers. If small-pox be introduced to a new soil, it is far more severe and fatal than when it occurs in a community which through many generations has been accustomed to its prevalence ; so also with measles and scarlet fever."—J. Hutchinson, *Syphilis*, pp. 389-90.

acquired traits in multicellular organisms, and therefore of the transmissibility of the effects of disease, there are certain facts, drawn from the study of disease itself, which sufficiently negative the opinion that traits acquired under such influence are transmissible. No immunity can be acquired against tuberculosis and malaria, the two most death-dealing of all diseases; or at least no immunity can be acquired against tuberculosis, and if any is acquired against malaria it is quickly lapsed, as in relapsing fever, in the intervals between the paroxysms. Experience by the individual of either of these diseases weakens rather than strengthens him against them. If the above, the Lamarkian theory, be correct, it follows, therefore, that races which have had a long and disastrous experience of tuberculosis and malaria should be less resistant than races that have had little or no experience of them. Each successive individual of the line should begin life with less resisting power than his parent began with, and should transmit less than he received to his offspring. The contrary, however, is the case, for races that have had a long and disastrous experience of tuberculosis and malaria are more resistant than races that have had little or no experience of them; thus the Englishman who migrates to the West Coast of Africa falls an easier victim to malaria than does the negro, whereas the negro who migrates to England falls an easier victim to tuberculosis than does the Englishman. In both cases, not only is the disease more readily acquired by the stranger, but, when acquired, it is much more deadly to him than to the man whose race has had an extended experience of it. In both cases, though experience of the disease by the individual rather weakens than strengthens his power of resistance to it, yet prolonged experience of the disease by the race does not result in a weakening of its resisting power, but in a conspicuous strengthening of it. Here, then, the Lamarkian theory breaks down utterly.

Conformably then to the *à priori* conclusion which we have already arrived at, namely, that the germ cell, a unicellular organism, is not, and cannot possibly be, so modified by the changes in the other unicellular organisms with which it is associated, the somatic cells, that, when it proliferates, it reproduces a cell-community composed of its descendants, in which those changes are reproduced,—*i. e.* conformably to the *à priori* conclusion that the acquired traits of multicellular organisms are not, and cannot possibly be, transmissible, —we have no alternative but to believe that races which have long been afflicted by a deadly and prevalent disease are more resistant to it than races which have not been so afflicted, or have been less afflicted, solely because in the presence of the disease the survival of the fittest and the elimination of the unfittest has resulted in an evolution, first, of the inborn power of making resistance; and second, of the inborn power of acquiring capabilities for making resistance—powers which to some extent have been developed generally by Natural Selection in all higher multicellular organisms, in consequence of their liability to become the prey of low parasitic unicellular organisms, but which, in the presence of any particular zymotic disease, undergo a further evolution in a particular direction.

Here it is necessary to enter into a digression. Because some zymotic diseases—*e. g.* syphilis and tuberculosis—are under certain circumstances transmissible from the parent to the offspring, it has often and triumphantly been held, that a proof is thereby afforded of the transmissibility of acquired traits. Such arguments, however, afford proof of great confusion of ideas only. The microbes and toxins of any zymotic disease which may afflict an individual are not acquired traits in any true sense of the term. They are, and for ever remain, foreign elements, just as a rifle bullet that finds its way into the tissues is, and for ever remains, a foreign element. The secondary infection of a germ or of an embryo through the parent, therefore, no more implies the transmission of an acquired trait than the passage of a rifle bullet through the body of the parent into the embryo implies it. If, however, the effect produced by the passage of the bullet on the parent's body were afterwards reproduced in the unwounded developing body of the child, we should then have a true example of the transmission of an acquired trait; so also, if the effects produced on the tissues of the parent by the microbes and toxins of a zymotic disease were reproduced in the non-infected developing body of the child, we should have another example of the transmission of an acquired trait. But this certainly never happens: no non-tubercular child of a parent who has phthisical cavities in his lungs ever reproduces those cavities in the process of development; never are the pock-marks in the pitted skin of one who has suffered from small-pox reproduced during development by the non-infected child.

Following a line of argument somewhat different from that commonly used, and writing at a time when the microbic origin of many diseases was unknown, Mr. Herbert Spencer said—

"But the clearest proofs that structural alterations caused by alteration of function are inherited, occurs when the alterations are morbid. 'Certain modes of living engender gout,' and gout is transmissible. It is well known that in persons previously healthy consumption may be produced by unfavourable conditions of life—by bad and insufficient food; by foul, damp, and unventilated habitations; and even by long-continued anxiety. It is still more notorious that the consumptive diathesis is conveyed from parent to child. Unless, then, a distinction be assumed between constitutional consumption and consumption induced by unwholesome conditions—unless it be asserted that consumption of unknown origin is transmissible, while functionally produced consumption is not, it must be admitted that those changes of structure from which the consumptive diathesis results, may be caused in the parent by changes of function, and may be inherited by their children."—*Principles of Biology,* vol. i. p. 250.

Certain modes of life certainly tend to engender gout in those predisposed to it, who in general are the children of parents with similar tendencies. But gout is certainly not transmissible; only the inborn tendency to acquire it anew under fit conditions is transmissible. The offspring of gouty persons are never born with the structural changes which gout has caused in the parent reproduced in them, nor in the absence of the disease do they reproduce them in after life; but being of similar tendencies they may, under similar conditions—*i. e.* when suffering from the disease—undergo similar changes. Here, therefore, we have no true instance of the transmission of an acquired trait. Again, it is not true that consumption may be produced in persons previously healthy by unfavourable conditions, "by bad and insufficient food, by foul, damp, and unventilated habitations, and even by long-continued anxiety"; and it is never, at the present day, a disease of unknown origin. It is always produced by a particular species of pathogenic micro-organism, the bacillus tuberculosis, which, under conditions that lower the vitality, that reduce the personal vigour of the phagocytes, attacks even persons normally resistant to it; in the absence of it no conditions, however unfavourable, can ever produce consumption even in the least resistant.

Unfavourable conditions merely constitute the battle-field, the microbe is the bullet that wounds. "It is not the disease tuberculosis that comes into the world with certain individuals, or with successive children of the same family, but the aptitude to contract it should external conditions favour."—*Prudden.* The consumptive diathesis, if by that is understood comparatively feeble powers of resisting the tubercle bacilli, may be either of the inborn kind or of the acquired kind, or it may be a compound of both. That only the inborn kind, or only that part which is inborn of a diathesis compounded of both kinds, is transmissible, is decisively proved by the fact already dwelt on at length, that races that have long and severely been afflicted by tuberculosis are more resistant to it than races which have not been so afflicted.

More than any other disease syphilis has furnished arguments to the supporters of the Lamarkian theory. The microbe producing it is apparently so minute, and of such little virulence, that alone of all pathogenic organisms it is able to take up its abode in or on the spermatozoon without destroying it, and so to accompany the latter in its long and devious journey to the ovum; for thus only is explainable the well-known fact that a healthy mother may bear a syphilitic child to an infected father. On the other hand, parents, both of whom are infected, may have for offspring a healthy child, as in the case mentioned by Mr. Jonathan Hutchinson (*Syphilis,* p. 412), when of twins born to parents both of whom were suffering from the disease in a virulent stage, one twin was diseased and the other healthy. Here, as regards the healthy child, not only cannot the spermatozoon have received and conveyed infection, but the ovum as well as the developing embryo must have escaped it, a combination of events which must necessarily be rare. The limiting membranes of the ovum and spermatozoon probably prevented the entrance of the pathogenic microbes, which, judging from the observed behaviour of the micro-organisms of many other diseases, probably find their nutriment normally in the fluids of the host, and are taken up by certain cells, the phagocytes, only when an attempt is made to destroy them. The escape of the embryo is more easy to understand. As has long been known, there is no connection between the blood-vessels of the mother and those of the fœtus, and therefore no blood flows from the one set of vessels into the other. They are, however, in intimate apposition, and are so thin-walled that, while no solids can pass from the parent to the embryo, or *vice versâ,* fluids with absorbed gases pass readily by osmosis. Like other solids, the microbes of disease are stayed in the placenta,—Klein mentions that he has examined the placenta of a guinea-pig dead of anthrax, and found the microbes therein strictly limited to the maternal part of it,—and cannot, therefore, under normal circumstances infect a healthy embryo from a diseased mother, nor *vice versâ.* Under abnormal circumstances, which may be induced by the action of the micro-organisms themselves, a breach of continuity in the walls of one or more vessels occurs, and the micro-organisms pass from the diseased to the healthy individual. In cases when this does not happen, when a healthy child is born to a diseased mother, or when a healthy mother bears an infected child to a diseased father, it is highly significant that the healthy individual is henceforward, or for a considerable time, immune to the disease; or at least a healthy mother who has borne an infected child is immune to the disease, as is proved by the fact that she does not contract it when giving suck to her infant, whereas other healthy women become infected if they similarly tend it; and it may safely be prophesied that the healthy child of a diseased mother will exhibit a like immunity. Here the toxins, soluble substances, passing, even when there is no breach of continuity in the vessel walls, from the diseased to the healthy individual, cause a reaction in the phagocytes of the latter by virtue of which they are enabled, if the microbes at any future time find entrance, to destroy them.

So also as regards other zymotic diseases, small-pox, measles, &c., against which immunity may be acquired; while it is certain that a parent who has acquired immunity does not confer it on the offspring which subsequently arise from his or her germs, it may safely be prophesied, though I am not aware that the phenomenon has actually been observed, that a pregnant mother, suffering from any such disease, if she recovers and does not abort, will confer immunity on her fœtus of as enduring a nature as it would acquire did it take the disease itself after birth and recover. Mr. Hutchinson speaks of immunity acquired in this way as an unexpected fact; I think, however, we may claim that the fact is just such as might be expected. The only thing puzzling about the process is the circumstance that an individual, in whom the toxins alone are present, acquires immunity so much more quickly, and with so much less injury, than an individual in whom the microbes are present as well as the toxins; for instance, a non-infected mother, pregnant of a syphilitic embryo, acquires immunity with far greater speed, and with far less harm to herself, than a woman who is actually infected, and in whom the microbes are present as well as the toxins. The explanation of this is probably found when we remember the great length of the incubation period, and of the general duration of the disease. This seems to imply that the toxins of syphilis are of feeble virulence, a conclusion which is further borne out by the circumstance that the general disturbance of the system produced by them is small when compared to the disturbance produced by the toxins of acuter diseases—*e.g.* anthrax or diphtheria. The microbes, therefore, since Natural Selection has so little developed their toxins, and since they are able so long to resist the phagocytes, must, like those of tuberculosis, possess considerable "personal vigour," and must therefore considerably injure and enfeeble the phagocytes in the physical struggle waged between them. As a result, the phagocytes of an infected individual are at a great disadvantage, as compared to the phagocytes of an individual in whom the toxins alone are present, and therefore are much less able to vary in a fit direction, are much less able to respond appropriately to the stimulation of the toxins; so that when, at a given stage of the disease, the phagocytes of the latter, uninjured by a physical struggle, are quite able to destroy the microbes, should they find entrance, the phagocytes of the former are quite unable to do so speedily, and recovery from the disease is delayed to a later period.

Children born of parents recovered from syphilis and immune to it are not themselves immune, though non-infected children born of a *mother* not yet recovered are certainly immune, as we may prophesy with confidence. Here again we light on a fact of the highest importance, the significance being this—that while in the non-infected embryo of a diseased mother there are phagocytes which react to the stimulation of the toxins derived from the microbes in the mother, whereby the immunity of the child is secured, in the ovum (or the spermatozoon) of an infected person there are no phagocytes; in it therefore there can be no fit reaction, and therefore the individual into which it may subsequently proliferate is not immune.

The arguments in support of the Lamarkian doctrine, which are founded on the supposed transmission of acquired immunity, therefore fall to the ground also. Acquired immunity is never transmitted, but when it seems to be so is really acquired anew by the embryo.

CHAPTER IV

ONCE again we may resume the main thread of the argument. We have already noted that since, at the present time, men perish mainly by disease, man's present evolution must be principally against disease. The most cursory examination reveals how considerable this evolution is and of what importance. It is time to attempt some estimate of it, and this, perhaps, will best be done by contrasting the effects produced by some of the more death-dealing diseases when afflicting, on the one hand, races that have had extended experience of them, and have therefore been rendered resistant by the survival of the fittest, with the effects produced by them when afflicting, on the other hand, races which have had no extended experience of them, and therefore have undergone no evolution in relation to them. But before entering into this detailed examination, it will be well if we pause for a space and endeavour by means of a general survey to trace the course of this evolution; a proceeding which will enable us to form some idea of the vast, the vital importance of the subject with which we are dealing. We have seen that the microbes of some diseases, of which malaria may be taken as the type, are perfectly capable of maintaining, and normally do maintain, an entirely saprophytic existence. A human prey is not essential for their persistence; they are able to find their nutriment and to multiply in man; but they are equally able to find their nutriment and to multiply in the environment outside him; they are therefore to be found in totally unpeopled districts, but are limited to certain well-defined areas, where the conditions of heat, moisture, nutriment, &c. are favourable; and since these conditions differ in different districts, the microbes in different districts differ in their pathogenic characteristics. Owing to their enormous numbers and to the generally sparsely populated condition of the countries infested by them, only an infinitesimal proportion of the microbes can take up their abode in human beings, and therefore the various species can have undergone no evolution in relation to man; but because no human being dwelling within their areas of distribution escapes infection unless he is immune, or death unless he is resistant, the races within those areas have undergone great evolution in relation to the microbes—an evolution which differs in degree in different districts, being greatest where the environment is most favourable to the microbes, where they are most abundant and virulent, and least where the environment is least favourable to them, where they are least abundant and virulent. In consequence of this evolution various races of mankind are able to inhabit areas such as the West Coast of Africa, and that strip of forest land in India known as the Terai, within which a continued existence is impossible to races that have not undergone it. It is probable that the races inhabiting the worst of these areas did not develop the whole of their resisting powers within them, for the virulence of the microbes infesting them is such that, in their presence, any race which had not developed some powers of resistance would probably have been destroyed before a sufficient evolution of such powers could have occurred. Probably, then, part of the resisting power of these races was evolved in other districts, where the microbes were less virulent, and from which they subsequently migrated.

But as regards diseases for the persistence of the microbes of which a human prey is essential, it is clear that not only have the races of men which they have afflicted undergone evolution in relation to them, but their microbes must also have undergone evolution in relation to man, since these microbes, some species of which are now wholly parasitic, must have descended from ancestors that were originally wholly saprophytic, and which, at a time when man had not yet appeared on the scene, were able to maintain existence perfectly in his absence. This evolution, this passage of the microbes from the saprophytic to the parasitic type, must have occurred at a date long subsequent to man's appearance. He cannot have inherited any of them from his brute ancestors, for, so far as we know, they never afflict animals in a wild state, though some species of animals in a domesticated state are afflicted by some of them. Again, nomadic tribes in a sparsely populated country are never afflicted by them, except sometimes when they are infected from more populous countries, and even then they are never afflicted by tuberculosis, the most death-dealing of all diseases, except they contract it under circumstances to which nomadic tribes are not usually subjected—*e.g.* close dwellings. An abundant and settled population is essential for their evolution and persistence—abundant as regards all of them, because otherwise the scant supply of nutriment would prevent the evolution and persistence of a parasitic species; settled as regards most of them, because otherwise, since they are earth, air, or water-borne, the removal of the nutritive supply would also prevent their evolution and persistence; and this is especially true as regards tuberculosis, the microbes of which require a specially favourable environment.

In other words, only after the supply of new nutriment had become secure can the descendants of saprophytic micro-organisms have evolved entirely parasitic habits. The evolution, therefore, of such zymotic diseases as are not malarial in type must have proceeded concurrently with the evolution of man's civilization. With the advance of this civilization, as his race, abandoning nomadic habits, gathered itself into larger and larger communities, into villages, towns, and cities, the nutritive supply of various species of micro-organisms, which had become pathogenic, became more and more abundant and secure, and therefore they more and more abandoned their saprophytic traits and became entirely parasitic. But with the evolution of the non-malarial zymotic diseases there must have proceeded, in such races of men as were afflicted by them, a gradual evolution through survival of the fittest of the power of resisting them, concurrently with which there probably occurred an evolution of attacking power in the microbes. At the least, as we see, there certainly occurred, in consequence of the continually increasing aggregation of men into fixed communities, a continual increase of their opportunities for attacking; so that in many communities as regards many of these diseases, just as regards malaria in regions afflicted by it, no man, not only recently, but for ages past, has escaped attack except he were immune, or death except he were resistant. In such communities, which, generally speaking, are those which have long been civilized, *i. e.* which have long led a settled and crowded existence, the evolution of the power of making resistance to the microbes, whether of the inborn or the acquired kind, has proceeded so far that their members are able to exist under conditions so favourable to the microbes, that when individuals from other communities, which have had little or no experience of the diseases produced by them, take up their abode in such long civilized communities, they perish with almost as much certainty as do Englishmen who migrate to the West Coast of Africa or to the Terai. Thus, while most Englishmen can dwell with impunity in London or any other British town, a Pacific Islander who takes up his abode there is in great danger of perishing from tuberculosis.

But while the members of crowded communities are, on the whole, more liable to attack from zymotic diseases of the non-malarial class than the members of less crowded communities, or of nomadic communities, and while, therefore, protective evolution against such diseases has on the whole proceeded farther among the former than among the latter, yet this difference in liability to be attacked, and consequently in evolution, between crowded and sparse communities is not equally great as regards all these diseases, since the microbes of some of them are much better able than the microbes of others to travel from point to point, and therefore to inflict the inhabitants of sparsely peopled lands, and even nomadic tribes. Such diseases are of the air-borne class. Their microbes have never been microscopically observed, and possibly they never will be so observed, for the fact that they are air-borne seems to indicate that they are exceedingly minute. Like the finest dust, they may be carried by the wind to considerable distances, and therefore they are more able to prevail among sparse populations than earth or water-borne diseases, especially when, on the borders of a sparsely inhabited country, there are more crowded spots, among the inhabitants of which they are endemic. Air-borne diseases may also be earth or water-borne, but their chief mode of infection is through the air.

The microbes of earth and water-borne diseases have mostly been observed under the microscope. They are too large and heavy to be carried far by air currents, and presumably also, owing to their greater size, they are less numerous than the microbes of the air-borne diseases. They are, therefore, less able to afflict sparse populations than the latter, and therefore the difference in protective evolution between the inhabitants of thickly and thinly populated lands is greater as regards them than as regards air-borne diseases.

Water-borne diseases are naturally better able to travel from point to point than earth-borne diseases, yet, journeying as they do by the great trade routes, along which the population is usually settled and dense, or down considerable rivers, on the banks of which the population is in a like condition, even they seldom afflict sparse populations. As regards trade routes, in places along them where the population is scanty, they seldom spread. As regards rivers, when carried down in the waters, they usually pass from a dense and settled population to a yet denser and more settled. They therefore seldom afflict sparse populations; and therefore the members of sparse and nomadic communities, on that ground alone, should exhibit much less protective evolution against them than the members of denser and settled communities; but because these water-borne diseases are never or very rarely so prevalent, even in the densest communities, as to cause the elimination from age to age of all individuals who are weak against them, this difference is not so marked as might otherwise be expected. Moreover, since these diseases only prevail when other conditions (of heat, &c.) besides that of density of population are favourable, the contrast here does not lie merely between dense and scanty populations, but also between one dense population and another. This is also true, but to a much less extent, as regards air-borne diseases; and it is also true, but to a much greater extent, as regards earth-borne diseases. The chief of these latter is tuberculosis; it afflicts, to a great extent, only dense populations, *i.e.* populations so dense that the infected and the healthy are brought into frequent contact with one another; and it afflicts to a very great extent only those dense populations which inhabit a particular kind of dwelling, a dwelling, namely, which is more or less air-tight, and the inhabitants of which therefore breathe vitiated air. Tuberculosis is never air-borne for more than a few yards, but since it is a disease of long duration, it may be carried under present conditions of travel round the globe in the bodies of those suffering from it. If its victims, when arrived in lands to which it is strange, take up their abode in more or less air-tight dwellings similar to those in which they contracted it, then the disease will spread to their co-dwellers, and will, under the circumstances, be more fatal to them than to the inhabitants of the land whence it came; but if its victims, when arrived in lands to which it is strange, take up their abode in dwellings which are freely swept by all currents of air, and especially if they take up their abode with nomadic peoples, then their co-dwellers will comparatively seldom be infected, and the disease will not spread to a great extent.

Now we have every reason to suppose that the inhabitants of the Old World, of Europe, Asia, and Africa, and their adjacent islands, have been afflicted for thousands of years by zymotic diseases of the non-malarial type, and therefore that for thousands of years evolution in relation to them has been taking place; and so dense from remote antiquity has been the population of the Old World, so few and of such limited extent, so bordered by densely-peopled countries, and so intersected by trade routes, its sparsely-inhabited tracts, that from time immemorial the whole of its inhabitants have been afflicted by one or more of these diseases: the sparse and nomadic populations least, and, generally speaking, only by air-borne diseases; the more dense and settled populations most, and, generally speaking, by the earth and water-borne diseases as well as by the air-borne diseases: and therefore the whole of its inhabitants from time immemorial have been undergoing evolution in relation to one or more of these diseases, the sparse populations least, and in relation, generally speaking, to the air-borne diseases only, the more dense populations most, and in relation, generally speaking, to the earth and water-borne diseases, as well as in relation to the air-borne diseases.

In the New World, in North and South America and the adjacent islands, in Australia and the islands of the Pacific, as well as in certain oceanic islands of the Old World, such as the Andamans, the case, until recently, was very different. We have every reason to believe, that before the discovery and invasion by Europeans of these vast regions, zymotic diseases of non-malarial type were unknown, or almost unknown, to their inhabitants, and therefore we have every reason to conclude, *à priori*, that their inhabitants before that date had undergone no evolution in relation to such diseases, a conclusion which is amply confirmed by *à posteriori* considerations. Diseases of the malarial type, the micro-organisms of which are perfectly able to maintain a wholly saprophytic existence, and are therefore able to persist in places in which a human population is scanty or entirely absent, were as prevalent in the New World of old as at the present day in territories where the environment was favourable to them, and of old, as at the present day, the native inhabitants of such territories were more resistant to them than strangers from outside the infected areas. Here similar diseases caused a similar evolution in both the Old World and the New. But zymotic diseases of the non-malarial type, the microbes of which are unable to maintain a purely saprophytic existence, and which are therefore diseases of crowded populations, were not evolved in this major half of the world, for here the necessary conditions did not exist. The inhabitants of the great continents, North and South America and Australia, were mostly scattered and nomadic. Such dense and settled communities as did arise, as in Mexico and Peru, were far inferior as regards numbers and antiquity to the settled populations of the Old World, such as those which for thousands of years inhabited Egypt, Asia Minor, India, and China. The conditions, therefore, in the great continents of the New World were unfavourable to the evolution of non-malarial zymotic diseases. In the islands, particularly in the small isolated islands, the conditions were so unfavourable that it is scarcely possible that any such diseases could have been evolved. Such a disease as syphilis, which persists long in the infected individual, and is acquired by direct contact, might possibly have been evolved, as also might such a disease as tuberculosis, which not only persists long in the infected individual, but against which immunity cannot be acquired. But such diseases as small-pox, measles, scarlatina, influenza, &c., which do not persist long in the individual, and against which enduring immunity may be acquired, could not possibly have been evolved, for the simple reason that the supply of nutriment for the pathogenic organisms would not have been sufficient, either as regards quantity or duration, to permit of such an evolution from purely saprophytic to purely parasitic habits. A single epidemic of small-pox or measles, for example, would have so exhausted the nutritive supply of the microbes in a small island, no matter how crowded with human beings, that before a fresh and susceptible generation had arisen the microbes would have perished.

Under such conditions, therefore, as formerly obtained in the New World, the evolution of zymotic diseases of the non-malarial type could hardly have occurred; the nutritive supply was insufficient, the human population too scanty, to permit of saprophytic micro-organisms acquiring parasitic habits in relation to man. This *à priori* conclusion also is amply confirmed by *à posteriori* considerations, for no evidence is forthcoming to show that zymotic diseases of the non-malarial type were prevalent in the New World before it was infected from beyond its bounds. We may read plenty of accounts which set forth how Spanish, Portuguese, or English adventurers suffered from "calentures," &c., but the diseases mentioned are evidently all malarial in type. Of syphilis there is indeed a tradition that it came to the Old World from the New, but there is no supporting evidence, and the tradition is negatived by the fact that the disease is particularly virulent among New World races, whereas it is particularly mild when attacking some Old World races, such as the Portuguese. The case for yellow fever is stronger; and it may be in the large and, at the time, somewhat densely-populated West India islands this one zymotic disease of a non-malarial type—perhaps it would be more correct to say of a semi-malarial type—did undergo evolution, and that it existed among the native Caribs before the discovery of America; but it is probable that even this disease was imported from the Old World.[1] "The first reliable accounts of yellow fever date from the middle of the seventeenth century; they tell of the importation of the disease from place to place, and from island to island." It is significant that the disease prevails on the West Coast of Africa, and that more than all other

[1] Hirsch's *Handbook of Geographical and Historical Pathology*, vol. i. pp. 317-18.

races negroes are resistant to it. It is possible, however, that negroes owe their superior powers of resistance in this instance to their long experience of malaria, for it seems to be true that persons immune to malaria are, for some reason, immune to yellow fever also, even when they are emigrants from regions where the latter has never prevailed; in other words, it seems to be true that evolution against yellow fever proceeds on identical, or nearly identical, lines as that against malaria, and therefore that extended racial experience of the one disease confers protective powers against the other. However this may be, it is at least certain that zymotic diseases of the non-malarial type, if not quite unknown, were almost unknown, over the whole of the New World, and were quite unknown over by far the greatest part of it. At the very least it is certain that all the non-malarial zymotic diseases of the Old World were quite unknown throughout the whole of the New World, and this without the exception even of yellow fever, which must have been originally either a West Indian or an African disease. It can scarcely have been both, for it is not likely that an identical species of parasitic micro-organism can have been evolved simultaneously and independently in two far-distant parts of the world.

Among the races of the New World, therefore, survival of the fittest can have caused no evolution of resisting power against the non-malarial zymotic diseases of the Old World, since there was no elimination of the unfittest from that cause, and therefore, when these diseases arrived in the train of conquerors, colonists, missionaries, and traders from their ancient seats, there then began the greatest tragedy the world has ever known; a tragedy so tremendous that, beside it, all the combined tragedies caused by all wars in all countries during all time shrink into insignificance. Wherever the races of the Old World—the white from Europe, the black from Africa, the yellow from Asia—penetrated, there the aboriginal inhabitants vanished or are vanishing. Some races, such as the Caribs and the Tasmanians, have become extinct; others are tending towards extinction, such as the Red Indians, the Polynesians, the Australians, and the Maoris; others again, such as the inhabitants of Central and some parts of South America, persist, though in diminished numbers, and possibly may persist long enough for the survival of the fittest to endow them with powers of resistance sufficient to save them from total extinction. It is to be observed, that these yet persistent though long-afflicted races of the New World inhabit tropical or sub-tropical countries, where the heat renders pleasant the admission of much air to the dwellings. They are therefore subjected, generally speaking, only to syphilis and to air-borne diseases,—small-pox, measles, scarlatina, &c.,—against which immunity may be acquired by the individual, and which are death-dealing to a far less degree than that earth-borne disease tuberculosis, to which the inhabitants of colder climates—*e. g.* the Red Indians and the Maoris—are subjected, as well as to the air-borne diseases.

Writing of the Spanish occupation of the West Indies, the late Professor Froude says—

"The Carib races whom the Spaniards found in Cuba and St. Domingo had withered before them as if struck by a blight. Many died under the lash of the Spanish overseers. Many, perhaps the most, from the mysterious causes which have made the presence of civilization so fatal to the Red Indians, the Australians, and the Maoris. It is with man as it is with animals. The races that consent to be domesticated prosper and multiply; those that cannot live without freedom pine like caged eagles, or disappear like the buffaloes of the prairies.

"Anyway, the natives perished out of the islands of the Caribbean Sea with a rapidity which startled the conquerors. The famous Bishop Las Casas pitied and tried to save the remnant that were left. The Spanish settlers required labourers for the plantations. On the continent of Africa were another race, savage in their natural state, which would domesticate like sheep and oxen, and learnt and improved in the white man's company."

These sentences are typical of much that has been written concerning the decay of the New World races. Almost all writers unite in speaking of it as mysterious, and yet the facts are patent, are manifest to every observer on the spot. There is no more mystery connected with their decay than with the extinction of the dodo or the bison. It cannot be doubted that the New World races have suffered or are suffering extinction in consequence of the introduction among them of Old World diseases, and because of one other cause, also an importation, of which more hereafter. So much is quite beyond dispute, and these causes may be seen in operation over half the world at the present day—in North and South America, in Australia, in New Zealand, in the islands of the Pacific, as well as in the Andamans, and several other of the oceanic islands of the Eastern Hemisphere. The sole mystery has lain in the circumstance that the races of the New World are less resistant to diseases of the non-malarial type than those of the Old World, and to that mystery I trust I have furnished a key. It is no question of freedom or of domestication, or even of civilization *per se*. The continental savages of the Old World do not perish when brought into contact with civilization. In India and Ceylon are tribes of an exceedingly wild type that have existed for thousands of years in contact with, and in the midst of, most ancient civilizations and very crowded populations. There is no conceivable reason why the Caribs should have been less capable of enduring domestication or slavery or civilization than the equally barbarous or even more barbarous negroes. But they perished, as other New World races are perishing, because, unlike the negroes, they had not been rendered resistant to the non-malarial zymotic diseases which the Spaniards introduced, and they would have perished had the Spaniards come among them as slaves, not as masters, and adopted their manners and habits of life, instead of forcing on them a change; for their islands lay in the very highway of the commerce that then sprang up, in the very path of infection.

The above considerations give rise to thoughts as exceedingly grave as they are painful. Are not all our efforts, whether prompted by philanthropic or religious zeal, by which we seek to protect and preserve the aboriginal races of the New World, wholly mistaken? Are they not in effect absolutely murderous? We gather them into close school-rooms and churches, where teachers and missionaries speak to them from infected lungs. We endeavour to persuade them to abandon their nomadic habits and form settled communities. We—and thereby we prove our own barbarity, the imperfection of our own civilization—force them in climates where clothes are wholly unnecessary, and therefore a species of dirt, to wear clothes, than which a better vehicle for air and earth-borne disease cannot well be conceived. In fact, we strive to bring them at one bound into that state of society which has become possible to us only at the cost of tens of millions of lives during thousands of years. During all that time the conditions favourable to the prevalence of zymotic diseases of the non-malarial type have been increasing amongst us; during all that time these diseases have been claiming their many victims among the ranks of the unfit, and our races have been undergoing an evolution in relation to them which, notwithstanding the vast lapse of time, has yet been so rapid, owing to the severity of Disease Selection, that we constantly meet individuals, who, lapsing the evolution of their immediate ancestry, have reverted to the condition of greater susceptibility which characterized their remoter ancestors, and who, therefore, perish in the presence of zymotic disease. It is altogether impossible that the New World races can in so short a time accomplish an evolution which it took the Old World races so long to achieve.

It is probable that most if not all of the still persistent races of America, Australia, and Polynesia are doomed irretrievably to extinction. In the presence of zymotic disease, and of conditions that ever grow increasingly favourable to it, they are as unfit as was the dodo when man invaded Mauritius. But at least we need not favour their extinction by means that are ignorantly intended to prevent it. We need not crowd the American Indians, the Australians, and the Polynesians into school-rooms and churches, and so subject them to conditions the most favourable for acquiring zymotic disease. We need not teach such of them as dwell in warm climates that morals are inseparably connected with clothes, and that the wearing of garments is a necessary prelude to eternal bliss. We need not persuade such of them as are nomadic to form death-traps for themselves in the shape of settled communities. In fact, we need not attempt to civilize them, at least in so far as civilization depends on settled and crowded communities, air-tight houses, and unnecessary clothes. Above all, we need not send to them as teachers and guides, men who are infected with disease, especially with tuberculosis. Only by preserving intact, as far as possible, the conditions under which their ancestry lived, and for which their evolution has therefore fitted them, can we hope to save for a time these—in many cases—noble and interesting races.

The decay of the races of the New World affords the most convincing, as it does the most terrible, proof that man's present evolution is mainly against zymotic disease. The extent to which that evolution has proceeded in the races of the Old World, as contrasted with those of the New, affords proof, moreover, of the immense antiquity of man's civilization in the former, for zymotic diseases, as we see, can have arisen only long after men had become very numerous and had begun to gather into settled communities; since before that time saprophytic micro-organisms would obviously have been unable to assume entirely parasitic ways of life in relation to him. The oldest histories extant tell of plagues and pestilences, of water and air-borne diseases, and when they were written earth-borne diseases, such as tuberculosis, were doubtless present also; yet since these, though death-dealing beyond the others—at least as regards tuberculosis—never prevail in a strikingly epidemic form, they are not mentioned.

We have now to consider man's evolution in relation to particular zymotic diseases, but here the proofs are often much less convincing than when we consider the proofs of his evolution in relation to zymotic disease in general. This again is just what might be expected, for since many zymotic diseases are not very prevalent or not very fatal, evolution against them is frequently not very marked; and it is only by comparing the peoples of countries in which they have been most prevalent and fatal with those of countries in which they have been least prevalent and fatal, or have been not at all prevalent, that we are able to perceive that any evolution has taken place. And here it may parenthetically be remarked, that we must not fall into the mistake, very apt to be made, of supposing that any disease which is now very mild in type, though very prevalent, has become so owing to a protective evolution against it. It may originally have been mild in type. It is obviously no part of the specific interest of pathogenic micro-organisms that the host they for the time inhabit should be destroyed, for when the latter perishes the microbes within him of necessity perish also. Their sole specific object is, of course, specific persistence, and this, as in the case of higher plants and animals, is attained in various ways. Some species of pathogenic micro-organisms, to protect themselves from the phagocytes, secrete a very virulent, a very poisonous toxin —*e. g.* the microbes of small-pox; others again exhibit great personal vigour—*e. g.* the microbes of tuberculosis; in each case the persistence of the species, not the death of the host, is the object; yet others exhibit neither very virulent toxins nor great personal vigour, but the persistence of the species is secured by a rapid passage from hosts that have become resistant to hosts that are still susceptible—*e. g.* the microbes of measles. Therefore we must not suppose of chicken-pox, for example, because it is now very mild though very prevalent, that necessarily it was once very fatal. We must only suppose, necessarily, since chicken-pox causes some deaths, that there may have been some evolution against it—an evolution which may be so slight as to be wholly inappreciable, or which is non-existent owing to concurrent retrogression. It is only when we consider diseases which are very deadly as well as prevalent —deadly at least to races that have had little experience of them—that we shall be able to detect evidence of considerable evolution. Of such diseases there are at least two—tuberculosis and malaria. We will consider them first, and afterwards pass to the consideration of other less prevalent or less deadly diseases, when we shall find that the evidences of evolution, though less clear and unequivocal, are often clear and unequivocal enough.

It must be borne in mind, however, that never before now, at least so far as I am aware, have diseases been considered from the standpoint of the evolutionist, and that therefore there is no literature bearing on the subject to which we can appeal. As I have already indicated, the attention of biologists has hitherto been directed solely to changes of form sufficiently gross to be perceptible to the naked eye, or under the microscope, or to mental changes. This other, this vastly important line of evolution, seems to have entirely escaped observation, and therefore I can offer the reader such proofs only as are to be found in books devoted to quite different objects, when, while accepting the facts furnished by various authorities, I shall often be obliged to dispute their inferences, since these have been drawn without reference to man's evolution in relation to zymotic disease. Before commencing our detailed examination, it will, however, be well to seek and note the explanation of two or three interesting phenomena.

Children are usually much less resistant to zymotic disease than adults of the same race. Thus while the adult negroes of the West Coast of Africa are very resistant to malaria, numbers of their children perish of it, though to nothing like the same extent as do the children of people immigrant from non-malarial countries. We might suppose that the superior immunity exhibited by adults is due solely to the fact that, in the presence of any prevalent zymotic disease, the weak against it perish early in life, leaving the more resistant to survive, and no doubt this to some extent explains the facts; but that it does not entirely do so is plainly proved by the circumstance that the young of various animals are less resistant to zymotic disease, artificially induced by means of an attenuated virus, than adult animals of the same species; for instance, young guinea-pigs are less resistant to the attenuated virus of anthrax than are older animals. The popular belief that the immature are more susceptible than the mature appears, therefore, to be well grounded. It is difficult at first sight to perceive the reason for this difference. In the young, as in the old, phagocytes are present, and presumably are as capable of performing their functions. The explanation probably lies in the fact that immature individuals correspond to an earlier stage in the phylogeny than more mature individuals, to a stage, that is, when any particular zymotic disease, and indeed zymotic diseases in general, did not afflict, or had not so long afflicted, the race. We know that characters that appeared late in the phylogeny appear late in the ontogeny also, and therefore, on *à priori* grounds, it is to be expected that the mature are more resistant than the immature to the action of zymotic disease; but, on the other hand, it does not follow, because children in their ontogeny correspond, generally speaking, to a stage in the phylogeny when the race had not encountered zymotic disease, that therefore they should have no powers of resistance at all. Natural Selection, as we know, acts not only at end of the ontogeny, but during the whole course of it, in consequence of which the ontogeny is but a blurred recapitulation of the phylogeny; and therefore evolution is to be observed not only in the adults during any stage of the phylogeny, but also in the immature during all stages of the ontogeny, thus causing them to differ more or less from their prototypes in the phylogeny. Malaria, for instance, by attacking the immature as well as the mature, causes an evolution, which, so far as it can be noted, is observable during all stages of development, but which is most pronounced in the individual when he reaches, and after he passes, that stage in his development which corresponds to a stage in the evolution of his race, when it already had had extended experience of the disease, *i. e.* to the very last stages in the evolution of the race.

If the above chain of reasoning be correct, it leads us to the further deduction that, since both the mature and the immature individuals of races that have undergone evolution in relation to any disease exhibit increased powers of resisting it, but the former the greater powers, therefore there should be a greater difference in this respect between the mature and the immature of such races, than between the mature and the immature of races that have had no experience of the disease; as in the latter case there can have been no evolution against it in particular, though possibly there may have been an evolution against zymotic disease in general. For instance, the susceptibility of negro children to malaria as compared to that of the adults of their race should be greater than the susceptibility of English children as compared to that of the adults of the latter race. In fact, the mature and the immature individuals of a race that has not undergone evolution in relation to any disease, should be very much on a par as regards their powers of resisting it. I am not aware that observations confirming this *à priori* conclusion have been made, but I have little doubt that any observation that may be made will confirm it. It must, however, be borne in mind, that immature individuals are more liable than adults to have their vitality reduced by various disorders which adults, because they correspond to a later stage in the phylogeny, less commonly suffer from, and under these circumstances to fall an easier prey to the prevalent zymotic disease. This source of error obviously obtains as regards such a disease as tuberculosis, which, as we know, frequently attacks persons hitherto immune, whose vitality has been lowered by other causes. Moreover, evolution against particular zymotic diseases may involve some evolution against zymotic disease in general; in other words, it is possible that a race which has undergone evolution against many zymotic diseases is thereby endowed with powers which may enable it to resist the microbes of a disease new to it, better than a race that has had little or no experience of zymotic disease. This involves another possible source of error, for if such be the case, adults of a race that has suffered much from zymotic diseases would necessarily prove more resistant to any new zymotic disease than their immature kin.

A second point of interest presented by zymotic disease is the apparently well-authenticated fact, that continued residence in a country where any such disease is prevalent often appears to confer on the individual acquired powers of resistance against it, even in the absence of illness and recovery; but these powers are of such a kind that they endure only in the presence of the disease; so that if an individual who possesses them leave for a time the country where the disease is prevalent, he is liable on his return to fall a victim to it. Yellow fever is an example, and the natives of Domingo, for instance, who have returned from a sojourn abroad, appear to contract this disease more readily than compatriots who have never left the infected districts. The only conceivable explanation of this curious phenomenon appears to lie in the hypothesis, that individuals who are already in some degree resistant by nature—*i.e.* inborn traits—acquire greater powers of defence as a reaction to continual assaults from the disease; in other words, that those living within the infected areas are continually infected by the pathogenic micro-organisms, which are, however, destroyed by the phagocytes before they are able to multiply and produce the disease. Thus small doses of toxins are introduced into the system, in response to the stimulation of which the phagocytes react to some extent, and attain a position of some advantage, which, however, since the stimulation is so much less, is not so great or so permanent as that which results in an individual who has actually experienced and recovered from the disease. Such powers are therefore quite lapsed during residence in a non-infected area, and therefore an individual who returns from a lengthened sojourn in it is liable to fall a victim to the disease to which he was formerly immune.

A third point of interest is the following: diseases in which the toxins are powerful are usually exclusive each one of all others; that is, such diseases seldom, if ever, co-exist in the same individual. The reason for this must be that the toxins of a disease that has established itself, poisonous as they are to the component cells of the host, are equally or more poisonous to the microbes of other diseases, which therefore are unable to multiply in such an unfavourable environment. Probably these low unicellular organisms are not able to vary in a fit direction like the cells of high multicellular organisms, and for that reason probably it is that we sometimes hear of a long-established disease, —*e. g.* lupus,—the toxins of which are not virulent, being cured or abated after the host has suffered from some other disease, the toxins of which are virulent—*e. g.* erysipelas. In such a case the toxins of the latter must be even more effective against the microbes of the old-established disease than against the phagocytes of the host; must even more enfeeble them than the phagocytes; in consequence of which, when recovery from the new disease occurs, the old disease is also conquered.

CHAPTER V

Malaria.—Man's evolution against malaria is more striking and conspicuous than that occasioned by any other disease, and this for two reasons. Firstly, because in many districts infested by its microbes, it is so prevalent and virulent that no one resident in them escapes infection unless he is immune, or death unless he is resistant; the elimination of the unfit has therefore been very thorough, and presumably it has been very prolonged, since in such districts the inhabitants, however much they may have warred among themselves, have dwelt secure, protected by their deadly climate from the fate that has befallen so many aboriginal tribes —*e.g.* the aborigines of Great Britain,—extermination by immigrant hordes. Evolution against malaria has therefore been very considerable. Secondly, the illness occasioned by the disease is of a very sudden and marked character, and therefore observers are easily able to contrast its effects on individuals of different races, and to perceive how much more resistant are those races which have had prolonged experience of it than those to which it is strange.

So considerable has the evolution against malaria been in various parts of the world that it is scarcely necessary to bring forward evidence in proof of it. Nothing indeed can be plainer than that different races of mankind differ vastly in their powers of resisting the disease, and that those races that have had extended and disastrous experience of it are much more resistant than those that have had little or no experience of it. Even people who strenuously repudiate the idea of evolution in general, must admit that in this case evolution has certainly occurred; for if, as they usually believe, all the races of mankind had a common origin, and were therefore originally alike, then in no other way is explainable the difference that now exists between one race and another; not only as regards disease, but also as regards size, colour, shape, &c.—*e. g.* between the Englishman and the West African negro. The only question possible is as to how this evolution occurred—as to what are the factors of it. Has it been by the accumulation of inborn traits, or by the accumulation of acquired traits, or by the accumulation of both? This question has already been dealt with; there can be no doubt that it has resulted solely from the accumulation of inborn traits.

But while this evolution, when once attention is drawn to it, becomes so manifest, that we need not waste time in marshalling in proof of it facts that are notorious, we shall nevertheless find it interesting to note how exactly the degree of evolution undergone by any race coincides with the virulence of the disease to which it has been subjected. This fact is admirably brought out in the following—

"In Ceylon there died of malarial fevers per 1000 of the population—

Negroes	1·1
Natives of India	4·5
Malays	6·7
Natives of Ceylon	7·0
Europeans (English)	24·6" [1]

[1] Hirsch, *Geographical and Historical Pathology*, vol. i. p. 245.

The above table is even more significant than it appears. Had the habits and customs of the Europeans been the same as those of the Asiatics, without doubt their death-rate would have been higher than it was, as may be judged from the following extract—

"It has been stated over and over again that negroes and natives of the countries where malarial fevers are endemic are themselves immune to the poison of malaria.

"This is too sweeping a statement to make, certainly so far as regards British Guiana, and I believe of other tropical countries as well.

"It is quite true, I think, that the negroes and Creoles of this colony do not suffer to nearly so great an extent as foreigners; a very marked difference in this respect being evidently noticeable in the numbers of the coolies attacked compared with those of the negroes and Creoles. Coolies all suffer to a very great degree, and are probably the class of foreigners most subject to the malarial poison.

"On the other hand, we must take into account in dealing with the question of immunity in the natives of tropical countries the various conditions under which they live as compared with those of foreigners. Now, unfortunately, the natives of this country live under the most insanitary conditions—conditions which would soon be fatal to Europeans unaccustomed to tropical life. Yet their death-rate would probably compare very favourably with that of foreigners.

"Again, the reason why the coolie suffers so terribly from malaria as compared with other foreigners is obviously due to the conditions under which he lives, and doubtless partly also to his occupation. The coolies, as we all know, live in huts built on the ground, in many cases no attempt being made to raise the floor, the latter as often as not being mother-earth; so that they may be said to be literally grovelling in malarial dust. Their occupation being mainly that of agriculturalists, again only serves to keep them saturated with earth poison.

"It would not have been unreasonable to suppose that the coolies, coming from an evidently malarious country, would have acquired a high degree of immunity to the malarial poison; unfortunately such is very far from being the case, and that this is so is entirely due to the causes mentioned above. Were the coolies to live under conditions more sanitary than their present ones, it is probable that they would show a certain amount of immunity. There can, I think, be no doubt, therefore, that, given a better sanitation, as regards the coolies living on estates, a very sensible decrease in their mortality would at once take place, and there would be far fewer cases utterly broken down in health, with sallow earthy complexions, unfit to work for a few hours together."—Dr. Ozzard in the *British Guiana Medical Annual and Hospital Report*, 1893, p. 91.

This difference in habits between Europeans and Asiatics explains why in India the former appear sometimes to suffer less than the latter.

"The following is a table compiled by Waring of the malarial sickness during ten years among the troops in the Madras Presidency.

	Total strength.	Admissions for intermittent.	Admissions for remittent.	Percentage of the troops.	
				Intermittent.	Remittent.
European troops	103,431	13,264	4336	12·8	4·2
Native troops	568,403	95,354	8046	16·8	1·4

"The native troops accordingly suffered from simple malaria fever to a greater extent than even the European, but the number of cases of remittent fever observed was three times less among the former than among the latter."—Hirsch, *Geographical and Historical Pathology*, vol. i. pp. 244-5.

In other words, the Europeans, to whose race the disease was strange, suffered three times as much from the disease in its severer forms as the natives, to whose race it was familiar—a fact to which many parallels may be found. Thus Professor Hirsch remarks—

"In the malarious regions of the tropics the natives take the milder forms of the fever, while the foreigners, and particularly those not acclimatized, take the disease in its severer forms; and, in accordance with that fact, the types with the longer intervals occur in the former, and those with the shorter intervals in the latter."—*Ibid.* vol. i. p. 241.

"Just as the history of malarial disease shows it to have been a malady of all times, so the inquiry into its geography leads us to recognize in it a disease of all *races* and *nationalities.* This predisposition to malarious sickness is developed to the highest degree among all the peoples belonging to the Caucasian stock, not only on European soil, but also among the Arab population of the Barbary States, and in the malarious districts of India, where the Mahommedan and Hindu population suffer in the same degree as foreigners. This is not the less true for the Malay and Mongol stocks, and for the native (Indian) population of North and South America. The predisposition is least for the Ethiopian race, which, although it by no means enjoys an absolute immunity from the disease, is still affected by it, *ceteris paribus*, less frequently, less readily, and less severely than other races; and to this many experiences have incontestably testified, not only in Senegambia, the West Coast of Africa, Nubia, and other parts of its native habitat, but also in other malarious regions of the tropics whither they have migrated. This relative immunity from malarial fever on the part of the negro race is an acquired, not a congenital one, as we may learn by the frequent cases of sickness and death from this disease among the children of the negroes in Senegambia. But the same immunity is enjoyed by the natives of *all* malarious regions, so far as concerns their own home, and such other localities as are affected by malaria less severely than it; so that one might almost formulate a general rule that the predisposition to malarial sickness becomes weaker in proportion as the individual has been continuously exposed, from birth to maturity, to more or less severe malarial influences, without suffering from them to any considerable extent."—Hirsch, *Geographical and Historical Pathology*, vol. i. pp. 243-4.

The passage last quoted furnishes an example of facts which may be accepted, but inferences which must be disputed. The sweeping generalization, that the "relative immunity from malarial fever on the part of the negro race is an acquired, not a congenital one," cannot be accepted as correct. Were it so, it would be as easy to rear European children in malarious countries as it is to rear the children of natives, and under equally insanitary conditions; whereas the fact is, that even under the best conditions procurable, it is difficult to rear European families in such countries as India, and practically impossible to do so in such countries as Senegambia. Moreover, characters which appear late in the ontogeny are not necessarily acquired. The superior immunity or much of it exhibited by adult negroes may be, and probably is, as truly congenital as are their permanent teeth or their beards. That some increase of resisting power may be acquired against malaria by continual residence in its presence is rendered certain by the fact, that natives of countries infested by it exhibit greater susceptibility when returned from a residence abroad than do their compatriots, or than they themselves did before they left their homes; but even this acquired character, like all similar characters, depends on an inborn trait developed by natural selection, for negroes exhibit greater *powers of acquiring it* than do Europeans.

Tuberculosis.—Man's evolution against tuberculosis is not less marked than his evolution against malaria, but owing to the insidious nature of the former disease, the gradual character of the attack, the slowness with which symptoms supervene, it is not so striking to the casual observer. In malaria toxins are present in abundance, and are of great virulence, and therefore the person attacked passes in a few hours from apparent health to extreme illness. Within twenty-four hours of entering an infested country he may manifest the symptoms of a virulent seizure. Ships navigated by men of a race that has undergone no evolution against the disease, may have their whole crews stricken down on entering a malarious port, while the natives around retain their health. Invading armies from beyond the borders of malaria have been decimated, and rendered useless as fighting forces, while the inhabitants of the land were able to pursue their ordinary avocations. Moreover, in malarious countries, the pathogenic micro-organisms are everywhere present in enormous numbers, and therefore no susceptible person escapes infection.

But in tuberculosis the toxins are conspicuously feeble. Infection is not marked by sudden and manifest illness. A slow, long-continued "personal" struggle ensues between the phagocytes and the pathogenic micro-organisms, which, however, is shorter in the less resistant than in the more resistant, in, generally speaking, the men of a race to which the disease is strange than in those of a race to which it is familiar.

Even in countries where the pathogenic organisms are most abundant they are not everywhere present, but are more or less limited to crowded and ill-ventilated domiciles to which infected persons have access. Individual powers of resistance, even among peoples to which the disease is quite strange, vary largely. Parties of strangers from beyond the infected areas are therefore never stricken down *en masse*, but one by one, at different intervals; and the symptoms noticeable in the sufferers are such as are referable by unskilled observers to other diseases—colds, coughs, &c. Last, but not least—among the races which are least resistant to malaria is our own; on the other hand, it is among the most resistant to tuberculosis; and therefore our attention is not drawn, in the same marked manner, to racial differences in relation to the latter disease as it is to differences in relation to the former. We have all heard, for instance, of the sufferings from malaria of our compatriots in India and on the West Coast of Africa, and that in the year 1809 a British army was, as a fighting force, destroyed by the same cause in the Island of Walcheren, but few of us know,

"That of 9000 Kaffirs (negroes from the East Coast of Africa) who had been imported at various times by the Dutch Government into Ceylon, and had been drafted into regiments, scarcely a trace of their descendants remains; they would certainly not be recognized at all among the present population of the island. In the years 1803 and 1810 the British Government imported some three or four thousand negroes from Mozambique into Ceylon to form into regiments, and of these in December 1820 there were left just 440, including the male descendants."—Hirsch, vol. iii. p. 226.

All the rest had perished, mainly from tuberculosis, and in a country where the disease is not nearly so prevalent as in England.

We habitually speak of the fatal climate of the West Coast of Africa or of the Terai; but we are usually unaware that our own climate at the present day is nearly, if not quite, as fatal to the inhabitants of much the greater part of the world—of all the New World and of Africa, a considerable portion of Asia, and part of Europe,—and that therefore our race, which is able to persist under such adverse conditions, has undergone an evolution in relation to tuberculosis fully equal to the evolution against malaria undergone by the West Africans.

The micro-organisms of tuberculosis, since they are essentially earth-borne and entirely parasitic, are unable to travel any distance outside the living body, or to persist except under such conditions as enable them to pass almost immediately from one host to another. These conditions are best satisfied in the dwelling-houses of civilized peoples, particularly of those who dwell in cold or temperate climates. Here sputum, swarming with bacilli, falls from infected lungs to the ground, or on clothes and articles of furniture, and is dried there. The bacilli, much lighter for desiccation, but still retaining their vitality, are thereafter wafted into the air by every movement which causes the dust to rise. Where air-currents blow strongly through the houses they are mostly borne away to perish outside, for which cause tuberculosis is less prevalent in hot than in cold countries; but where such currents are practically absent—*i. e.* in the "well-built" houses of the cold and temperate zones, in which draughts are at a minimum—the bacilli either fall to rest again within the room, or are inhaled by its inhabitants; when such of the latter as are not already infected, and are not sufficiently resistant, contract the disease.

It is clear from the foregoing that the conditions favourable to the tubercle bacillus have on the whole been increasing in civilized countries for a very long time; in England for at least twenty centuries, and we know not for how much longer. It is calculated that, at the present day, at least one-seventh of the total number of deaths is due to its agency; but it is death-dealing to a vastly greater extent under circumstances which are even more favourable to it than such as normally obtain. For instance, in prisons and some barracks where the inmates are crowded together, and their vitality reduced from various causes, mental and physical, and where strong draughts of fresh air are usually absent, the death-rate from tuberculosis is enormous.

"*Phthisis in Prisons.*—Consumption prevails in prisons to a truly disastrous extent. I take the following statistical data from an excellent article on the subject by Bàer: In the United States prisons, from 1829 to 1845, the mortality from phthisis was 12·82 per 1000 prisoners at Philadelphia, and at Auburn and Boston 9·89 and 10·78 respectively; in Baltimore prison it was 61 per cent. of the mortality from all causes. In the French prisons, particularly those in which long terms of penal servitude are worked out, the death-rate from phthisis amounts to between 30 and 50 per cent. of the mortality from all causes. In Dutch prisons it reaches the same height; in the Danish convict prisons it amounted in 1863-9 to 39 per cent. of all deaths; over the whole of the prisons of the Austrian Empire in 1877-80 it was 61·3 per cent.; and in the nine large convict prisons in Bavaria from 1868 to 1875 it was 38·2 per cent. In the penal establishments of Wurtemburg, according to Cless, the yearly average of deaths from phthisis from 1850 to 1859 was 24 per 1000; while from 1859 to 1876, in consequence of an improved diet, it fell, as we have seen, to 8 per 1000, although it still remained two or three times greater than among the people at large. During a period of eleven years (1869-79) the mortality in the prisons of Prussia was 42·87 per cent. of the deaths from all causes, and 12·32 per 1000 prisoners.

"For England we have Baly's report on the prevalence of phthisis from 1825 to 1842 among the convicts at Millbank Penitentiary, where 31 out of 205 deaths were due to cholera, and 75 of the remaining 174, or 43 per cent., were due to phthisis; while of 355 prisoners discharged during the same period on account of ill-health, 90 were phthisical, and of these quite three-fifths, according to precedent, would have died of that disease if they had been left to complete their term. In that way we bring the annual mortality from phthisis at Millbank up to 13 per 1000, or more than three times that of the London population at large. Pietra Santa gives the following facts for the prisons of Algiers:—Of 23 natives who died in the public prison of Alger, 17 succumbed to phthisis; in the central prison of l'Harrach there were 57 deaths from phthisis in a total of 153, or 37·2 per cent. The important influence of imprisonment on the occurrence of this disease is very clearly brought out by its prevalence in those regions where phthisis is in general a rare thing, as, for example, in Lower Bengal. Webb quotes the following remarks by Green with reference to the commonness of the disease among the natives in the prison of Midnapore: 'After a careful examination into the early history and origin of the cases of this disease as they have occurred, I have been led to the conclusion that many of the men thus affected were previously hale, and capable of earning their livelihood, and were not subject to cough before imprisonment. I find that after they have been working a few weeks or months on the roads here, and inhabiting the jail, they have become the subjects of attacks of inflammation of the lungs, and from time to time of frequent repetition of these attacks, which have ended in some cases . . . . in death in the acute stage, in others in a prostrate sinking state with a gradual wasting away of the body, and all the symptoms and ultimately all the post-mortem morbid appearances of tubercular disease of the lungs.' Next to the hard labour, Green lays most stress on the bad ventilation of the cells, and on the highly defective construction of the prison in other respects.

"The great frequency of consumption in convict prisons may seem to be due to many of the prisoners bringing the disease with them; but that such is not the case follows from the well-authenticated fact that most of the deaths from phthisis among prisoners do not occur until later years of their term of confinement. At Millbank Penitentiary signs of a pulmonary affection on admission could be made out, as Baly tells us, in only 12 prisoners among 1502 who entered in 1842, and in only 15 among 3249 who were received in 1844. Among the convicts of 1842 there were 510 women sentenced to transportation who remained at Millbank not longer than three months, and of these two fell ill with phthisis or scrofula during that time; whereas of the remaining prisoners admitted, no fewer than forty-seven became consumptive before the completion of their terms of two or two-and-a-half years. It is further to be kept in mind, that most of the convicts sent to Millbank had already served longer or shorter terms in smaller prisons elsewhere, and not a few of them more than one term; so that in a certain proportion of those who were found phthisical on admission to the central prison, the seeds of the disease might have been implanted while they were undergoing sentence previously.

"There is no doubt that prisoners are exposed to a large number of noxious influences capable of affecting their health or of creating more or less of predisposition to take phthisis, or of augmenting a predisposition already there, and among these a bad or insufficient diet, as we have already seen, might play a not unimportant part. But even under those circumstances, it is evident that the real factor is a protracted detention, or a detention with brief remissions, in crowded and ill-ventilated work-rooms and sleeping-places. That is the one detrimental thing that obtains with more or less of uniformity in all penal establishments, whatever difference there may be among them in their other arrangements, such as the very various kinds of discipline and occupation.

"The same hygienic disadvantages arising out of the manner of living among confined bodies of people, which we have just been considering, contribute not a little also to the prevalence of consumption, be it more or less, among the population living at large; and that holds good equally for the well-to-do classes and for the poor. Here, again, there is no mistaking the drawbacks in the mode of life that have come along with the progress of modern civilization. 'The prevalence of consumption among the families of our villagers and farmers,' says an American writer, 'can be shown, we believe, to have kept step with the deviation of these families from their former frugal, active, and industrious manner of life, and their adoption of the absurd practices which characterize the mode of our fashionable classes in the larger cities.' Once more, I think the stress should be laid on spending the time amidst bad ventilation indoors, in living-rooms, but more particularly in bedrooms. In the latter the human being spends nearly half of his existence; and the rooms assigned as bedrooms by the better classes are too often chosen, not on considerations of health, but out of a desire to have the greatest amount of comfort in the public rooms of the house.

"But the dark side of civilization nowhere shows its influence for spreading consumption more decidedly than in those disastrous outbreaks of the disease among peoples who were wont to live perfectly free from all restraint and conventionality, but have now come into contact with Europeans, and have adopted European manners and vices. Of that we have sad examples in the ravages of consumption among certain tribes of North American Indians, among the natives of several groups of islands in the Pacific, among the Maoris of New Zealand, and in Algiers."—Hirsch, vol. iii. pp. 222-5.

"The same circumstances serve to account for the strikingly common occurrence of phthisis in nunneries, seminaries, and such-like institutions, in evidence whereof a number of observations have been brought forward by Fourcault; also in the Oriental harems, not only among the women but among the children also; again, among badly-lodged troops, of which we have evidence from England, France, Turkey, and India; and, above all, in prisons.

"Among many surgeons there is complete agreement that cases of phthisis are least common in soldiers when they are leading an active life in the open air, on the march, or in manœuvres and campaigns; and that the cases mount up as soon as the troops enter on their garrison life, as, for example, in winter, and spend their time in ill-constructed, crowded, filthy, and badly-ventilated barracks. Welch, who treats of this matter with reference to the British army, says that 'nearly half of army consumption is connected with vitiated barrack atmosphere,' a similar opinion having been expressed by earlier writers such as Tulloch and Maclean, the latter including in his statement the British and native troops in India. With respect to its frequency in the French army, we find a similar reading of the facts in the papers by Champpouillon, Tholozan, Viry, Lausies, and others.

"'L'augmentation considérable des décès qui pèse sur l'armée en temps de paix,' says Tholozan, 'est surtout occasionnée par les lésions pulmonaires d'un caractère particulier; les lésions sont l'effet d'un vice spécial, d'une diathèse spécifique de l'économie qui se développe dans des conditions d'encombrement, d'agglomération, de vie en commun, particulières aux casernes.'"—Hirsch, vol. iii. pp. 221-3.

We see, then, that tuberculosis is essentially a disease of crowded and ill-ventilated habitations, and that bad as are the conditions under which we normally live, they may in relation to it be made infinitely worse. That they are yearly growing worse in the world at large, in consequence of the increase of the population, cannot be doubted, and this in spite of the greater attention which is now-a-days paid in some places to ventilation; for the best ventilation that exists in such houses as we inhabit, though it may help to maintain the vitality by ensuring an abundant supply of unvitiated air, is not of a kind to sweep the pathogenic organisms from the dwellings.

The conditions under which we live are such that, normally, every individual amongst us is exposed to infection, that is, we all at one time or another enter rooms inhabited by tuberculous patients; but such has been the evolution of resisting power in our race, that only one-seventh of us perish from consumption, while six-sevenths of us live immune to the disease, or recover from it, and die from other causes. A notable fact in this connection is the often evenly-balanced nature of the struggle between the microbes and the phagocytes. Many of us are resistant under almost any circumstances; many others easily contract the disease, and fall a rapid prey, even under circumstances the most unfavourable to it to be found in our land; but very many others exist in the border space between immunity and susceptibility. Such people, when their vitality is lowered, or under other circumstances favourable to the bacilli, contract the disease, but when their vitality is raised, or when the environment becomes less favourable to the pathogenic micro-organisms, defy it, or, if they are already diseased, recover. Even when they do not recover the disease usually runs a prolonged course in them; there is a lengthened struggle between the microbes and the phagocytes, and we then behold the phenomena of chronic phthisis. Laennec said that a patient does not die of his first attack of tuberculosis; that is, a patient of the highly resistant type, which is the normal in our race. Less resistant individuals, who have lapsed back to the ancestral condition of greater susceptibility, undoubtedly do often perish of their first attack; as, for instance, of tubercular meningitis, or of acute phthisis (galloping consumption); but the very general prevalence of more resistant individuals, who as regards their powers of defence against the bacilli correspond to a far later stage in the phylogeny, justifies Laennec's dictum.

"In a very large number of cases also, Laennec's first proposition is true, and we may extend it by saying, that not only do they not die of the first attack, but not a few recover from it and have no more seizures. Unfortunately this occurrence is not so common among the patients of our consumptive hospitals as it is among the rich or amongst those who can take the necessary precautions against further attacks." [1]

It is clear, then, that the men of our race are very generally so resistant to tuberculosis, that even after infection their phagocytes, under slightly improved conditions, are able to wage successful war against the microbes. It is clear also that no immunity can be acquired against the disease, since those who are recovered, under fit conditions, take it again and again. Their only safety lies in the absence of the pathogenic micro-organisms. On this account I have always thought it unwise, in the interests of the patients, to gather them into special hospitals. However excellent the arrangements may be in such establishments, they can hardly be made so perfect as to exclude the possibility of those who have recovered, in so far that their phagocytes have destroyed the micro-organisms within them, receiving fresh infection from inmates who are yet diseased. Doubtless such establishments discharge many persons cured of their complaint, but these fortunate cases, I apprehend, are usually those

[1] Dr. Arthur Ransom, *British Medical Journal*, July 23, 1892.

of people of the poorer classes with considerable powers of resistance, but who, under the very unfavourable conditions of dirt, ventilation, occupation, &c. which prevail in their own homes, acquire the disease, and who, in a hospital, have their vitality so improved that they are able to defy it in spite of the presence of the micro-organisms. But conditions as good, as regards diet, &c., as those which obtain in special hospitals, are to be had elsewhere in places where the tubercle bacilli are not so abundant, and where therefore re-infection is not so probable. Certain it is that the results obtained in our special hospitals do not compare with those obtained at foreign health-resorts, where the disease is not prevalent among the natives, and where the bacilli are therefore absent. It is probable, however, that the gathering of infected individuals to health resorts tends to destroy their value, and that they may, if fit conditions prevail, if dwellings such as those which the sufferers came from be built, if the old conditions of life be reproduced, become absolute plague spots.

Excellent health resorts are isolated places at high altitudes; better still are new countries where the disease is not prevalent, or has not long been so. To these we may send, with advantage, such of our consumptives as have not had their vital powers hopelessly lowered, *i. e.* who are not in an advanced stage of consumption, or such of them as are not so little resistant as to have taken the disease in its acute form. Under the new conditions, should their phagocytes overcome the bacilli, there will be less probability of re-infection. But sufferers from advanced or from acute tuberculosis are obviously unsuited for expatriation, for, since they are already infected, and since their phagocytes are unable to destroy the bacilli already within them, an environment free from the pathogenic micro-organisms offers no advantage.

Thousands of our race, who are unable to resist the attacks of tuberculosis in their native land, and on that account are obliged to leave it, are able to maintain a healthy existence under conditions that yet prevail in all such parts of the New World as have not long been settled by us—in certain parts of America, in Australia, in New Zealand, in the Pacific Islands, and also in South Africa; but in these very lands, where the less resistant among us even recover from previous infection, tuberculosis is causing the extinction of the natives. This one fact throws the greatness of our evolution into startling relief, for the natives usually live under hygienic conditions that are far better as regards the disease than do the settlers. The latter endeavour to reproduce their home life as nearly as possible; they gather themselves into urban communities, and build much the same kind of houses as those in which they contracted the disease; whereas the natives dwell scattered, or at most in small communities, and in dwellings more wind-swept than the shanties of the Hebridean fisher-folk, among whom consumption is practically unknown. Nevertheless they perish, and their races are becoming extinct, for, so susceptible are they, that they take the disease in circumstances under which the most susceptible Europeans live immune; and they are so little resistant that they take it in its most virulent form. The microbes, unchecked by the phagocytes, multiply within them at a rapid rate; they exhibit all the phenomena of galloping consumption; and even in their draughty wigwams and whares they infect their fellows. To infect a normal European a considerable dose of the virus seems necessary, since many of the bacilli succumb in the conflict with the phagocytes; to infect a Red Indian, or a Maori, the smallest possible dose seems sufficient, since the phagocytes appear to have no power of destroying the bacilli.

"Consumption is prevalent to a most disastrous extent among the races of the *Southern Pacific.* We have more particular accounts for Fiji and Tonga, Samoa, Tahiti, the Marquesas, and Hawaii (Honolulu). In New Caledonia the death-rate from consumption among the Kanakas is estimated at two-fifths of the mortality from all causes. Almost all the authorities are of opinion that the great prevalence of the malady in these islands dates from the time when the natives began to come into more intimate relation with European immigrants, and therewith to make considerable changes in their mode of life; and that opinion is borne out by the fact, that in the Hawaian Islands, where phthisis at the present time commits great ravages among the natives, it was of rare occurrence forty or fifty years ago. On the other hand, it follows from Wilson's account (1806) of the state of health in Tahiti, that phthisis had been widely prevalent in that group as early as the beginning of the century; and there are accounts to the same effect from the Tonga group, New Caledonia, and other of the Archipelagoes of Polynesia."—Hirsch, vol. iii. p. 187.

"In New Zealand phthisis has made frightful ravages among the Maoris, and has been one of the chief causes of the gradual extinction of that race."—*Ibid.* p. 188.

"On *Nossi Be* also the malady is not uncommon among the coloured races, particularly the Kaffirs. In *Madagascar* and *Mayotte* it is as common as in Europe, and rapidly fatal, as it mostly is in the tropics. In *Zanzibar,* Lostalot did not happen to see many cases, but it is said to be especially common among the Arabian women of the higher class."—*Ibid.* p. 189.

"In *Cape Colony* phthisis is oftenest met with among the Hottentots inhabiting the plains nearest the coast; in other classes of the population it is much rarer than in the East African islands, within the tropics just spoken of; while on the interior plateau of Southern Africa it hardly occurs at all. There is a lack of information of a trustworthy kind as to the state of health on the southern part of the *West Coast of Africa*—the coast of Lower Guinea. Around the *Bights of Benin and Biafra* (country of the Cameroons and of the Gaboon), as well as in the adjoining island of *St. Thomas*, it appears from the entirely trustworthy writings of Daniell, that phthisis is widely prevalent and very malignant among the negroes. As regards the French settlements on the Gaboon coast, that statement is fully borne out by the French medical practitioners; and we have an account to the same effect regarding its occurrence on the island of *Fernando Po*."—Hirsch, vol. iii. pp. 189-90.

"In the *Western Hemisphere* the inhabited regions within northern latitudes, and with an Arctic climate, offer a marked contrast to the corresponding territories of Europe, in respect to the great frequency of phthisis in them. In *North Greenland* that disease is one of the commonest causes of death. At a trading station on the northern shore of *Hudson's Bay* phthisis is prevalent among the scanty population to an enormous extent, according to the evidence of a practitioner who had been five years on the station; and there are reports to the same effect from *New Archangel* and the *Aleutian Islands* (Alaska). It is common also in *Newfoundland*, *New Brunswick*, and *Canada*, in the last particularly among the native Indians (Stratton)."—*Ibid.* pp. 192-3.

Just as regards malaria, so as regards tuberculosis, the resisting power of any race is exactly proportionate to its familiarity with the disease. The English, who have long dwelt under conditions most favourable to the bacilli, are more resistant than Hindoos, who have dwelt under conditions less favourable. Hindoos are more resistant than Africans, who, though they have lived in contact with the races of Europe and Asia, have dwelt under conditions very adverse to the disease. Africans are more resistant than the races of the New World, who, while dwelling under conditions equally adverse, had, until recently, no communication for thousands of years with the races of the Old World, among whom tuberculosis arose. Hirsch lays much stress on the susceptibility of Africans, as may be seen from the following :—

"No *race* or *nationality* enjoys a decided immunity from consumption; but in respect to the frequency of its incidence, the negro race takes first place. Proof of this is furnished by the medical reports from all those parts of the world to which the negro has migrated, and in the mixed population of which he forms a considerable ingredient: such as the United States, the West Indies, the Mosquito Coast, Brazil, the Argentine Republic, Peru, and Bolivia, Algiers, Egypt, the East African Islands, Ceylon and the East Indies.

"In the convict prisons of the United States, from 1829 to 1845, the average mortality from phthisis among the prisoners of the white race was 11·16 per 1000; but among the negroes confined in the Eastern Penitentiary of Pennsylvania it was 40·74, and in the Maryland Penitentiary 28·49; while among the coloured population living at large in New York it was 11 per 1000. At Wilmington, N. C., 0·9 of the whites died of phthisis in 1880, and of the blacks 2·6 (Wood). 'It is a remarkable fact,' says Bartolacci in his work on Ceylon, 'that of 9000 Kaffirs (negroes from the East Coast of Africa) who had been imported at various times by the Dutch Government into Ceylon, and had been drafted into regiments, scarcely a trace of their descendants remains; they would certainly not be recognized at all among the present population of the island. In the years 1803 and 1810 the British Government imported three or four thousand negroes from Mozambique into Ceylon to form into regiments, and of these in December 1820 there were left just 440, including the male descendants.'

"Whether this preponderance of phthisis among negroes is an affair of physiological predisposition due to their nationality, or to what extent it may be so due, we are unable to decide. It is certain, however, that the amount of the disease increases considerably among negroes away from their native countries, an increase that depends in part upon the manner of living. The extent to which a change of climate may operate in that direction will appear from the phthisis mortality among negro troops in the British service at certain military stations.

COMPARATIVE TABLE OF PHTHISIS AMONG BRITISH AND NEGRO TROOPS.

Station.	Deaths per 1000 British.	Deaths per 1000 Negro.
Jamaica	6·2	7·5
Lesser Antilles ...	7·1	9·8
Mauritius	3·9	6·4
Bahamas	2·0	7·0
Gibraltar	6·1	33·5

"Here we have confirmation of the well-known fact, that the migration of the negro to a colder climate is accompanied by a rise in the phthisical average; but it is obvious that we should also make allowance for changed habits of living as weighing not less in the scale. Pruner calls attention to a fact that has a bearing on the question, namely, that at Khartoum, in latitude 17° N., with a temperature not lower than that of the mountains around, consumption ensues among negro captives, as well as among the Arabs of the desert, whenever they give up their nomadic life to live under a roof, even if it be in a warmer region.

"Here also an important part among the disease-factors is played without doubt by bad food, insufficient clothing, and confinement in crowded, filthy, and badly-ventilated huts; and there is nothing to surprise us in the experience that consumption has increased to an alarming extent among the negroes of Arkansas of late, or since their emancipation, the result being due, as the authority adds, to the carelessness and shiftlessness of a class of people who have been suddenly thrown upon their resources, and have been withdrawn from the protection and consideration of a race more intelligent than themselves."—Hirsch, vol. iii. pp. 225-8.

But the fact remains, that notwithstanding their high death-rate from tuberculosis, negroes are able to exist and multiply in contact with Europeans, while the races of the New World are not so able. The death-rates of the latter then grow so high that they tend to become extinct. Natives of the crowded city-studded peninsula of India, though less resistant than the inhabitants of less crowded but much colder Europe, are more resistant than the negroes, as the following paragraph proves:—

"I pass now to another disease, phthisis, of which the increase in this colony is undoubted, and so far progressive. This increase has received notice more than once, and very fully, in a paper by Dr. Ferguson in the hospital reports. I wish to direct attention especially to a phase of its local development, which is also noticed by Dr. Ferguson—the different clinical history which the disease presents as it occurs in the two races forming the largest proportion of our very mixed populations. Amongst the blacks the disease, considered from the clinical side, is generally of the rapid, acute form known as 'galloping consumption'; or, looking at it from a pathological point of view, it presents in that race the form of tubercular caseous pneumonia. The phthisical East Indian, on the other hand, presents the clinical characteristics of the slower and more variable forms, or, speaking pathologically, suffers most commonly from tubercular interstitial pneumonia or peri-bronchitis. Now, why this difference? We have as yet no reason to doubt that the bacillus of the disease is in both instances alike, but this is a matter which it would be interesting to have settled by direct observation.

"The conditions under which the individuals live here are in both cases practically the same. The difference, therefore, does not appear to arise from any factor acting immediately and directly on the individuals, but to be connected with something more remote. To me there seems but one solution of the matter; the peculiarity depends upon inherited differences, in fact, on racial characteristics."—Dr. Grieve, *British Guiana Medical Annual*, March 1, 1890.

I have not been able to gather anything very definite concerning the Chinese in their relation to tuberculosis; but considering how many and large are their cities, how ancient their civilization, how filthy their habits, and how crowded their dwellings, especially the sleeping apartments, they should be of all the races of the world the most resistant, if not against tuberculosis, which infests in particular houses of the European type, yet against many other non-malarial zymotic diseases.

Measles.—Since measles, though very prevalent, is not very fatal, at least not so fatal as either malaria or tuberculosis, evolution against it is not nearly so marked as against either of those diseases. Its microbes secure their specific persistence by a rapid passage from host to host, and by an enormously rapid rate of multiplication. Nevertheless, all observers are agreed in declaring that it is more fatal when afflicting races to which it is strange than it is when afflicting races to which it is familiar—*i. e.* it is sufficiently death-dealing to have caused some evolution against itself. But all observers are also in agreement in thinking that the greater malignancy of type it displays when attacking the natives of the New World is to be attributed to improper medical treatment of the sick, or to lack of treatment rather than lack of resisting power. From this latter opinion it is impossible not to dissent. No doubt in any epidemic improper treatment, or lack of treatment, even in the case of so mild a disease, may result in a death-rate high as compared to that which occurs in epidemics during which a proper treatment is pursued; but in view of the very considerable number of deaths caused even among the most resistant races by measles, as well as of the fact that it is so prevalent among such races that no non-resistant person escapes death from it, it is impossible not to believe that this elimination of the unfittest and survival of the fittest has resulted in some evolution in races that have long been familiar with the disease, and that therefore the virulent character of its epidemics among New World races, as compared to the epidemics among the races of the Old World, is due largely to the circumstance that the former races have not been rendered resistant by the survival of the fittest, whereas the latter have. We shall, however, be in a better position to consider this point after a close examination of the following extracts.

"In considering the reason why some epidemics of measles should have had a malignant type, great stress, in my opinion, is to be laid on *mistakes of dieting and therapeutic treatment.* Without doubt it is here that we have the explanation of the fact, that the disease in past centuries had a much more unfavourable type than in recent times. In forming an opinion, however, on this point, we should bear in mind that many epidemics of measles adduced in evidence from the eighteenth century were, in fact, outbreaks of scarlet fever. But there still remain a considerable number of true measles-epidemics of that period, whose malignant character was due in the last resort, as the chroniclers themselves admit, to the way in which the sick were treated. Even for many of the epidemics of the last thirty or forty years, remarkable for their very considerable mortality, it could be shown that reactionary dietetic and therapeutic practices gave the epidemic its malignant character. The importance of that factor in the causation comes out in the clearest way in those epidemics of measles which, springing up among uncivilized peoples, have run a disastrous course in the absence of all rational treatment of the sick.

"Classical examples of this are furnished by the epidemic of 1749 among the natives on the banks of the Amazon, where the number of those that died of the sickness was reckoned at 30,000, whole tribes having been cut off; also in Astoria in 1829, where nearly one-half of the natives fell victims to the disease; among the Indians of Hudson's Bay Territory in 1846; among the Hottentots at the Cape in 1852; among the natives of Tasmania in 1854 and 1861; and in Mauritius and the Fiji Islands in 1874. Concerning the two last-mentioned epidemics, both of them disastrous, it is stated in the Report—'The great mortality has been in large measure due to the fact that the sick were exposed to the most unfavourable conditions. Unprotected from exposure, unattended and untreated, chiefly in consequence of their own unhappy prejudices, every complication of the disease must have been invited and rendered intense; in accordance with this view, we find that those classes of the native population over whom adequate supervision could be exercised have suffered slightly.' Smellie mentions facts of the same kind in the destructive epidemic of 1846 among the natives of Hudson's Bay Territory; of all those who were received into Fort York, and who there received medical treatment, not one died. In the account given by Squire of the frightful epidemic of measles in the Fiji Islands, which was known to have been introduced from Sydney by the retinue of King Kakobau, and which carried off 20,000 of the natives, or one-fourth to one-fifth of the whole population of the Fiji group, we find the following:—

"'The favourable progress of the early native cases negatives the idea of any special proclivity. Dr. Cruikshank, who treated 143 of the native constables, reports nine deaths, most of these resulting from the evasion of needful precautions. Later in the epidemic, when it is said to have been like the plague, and the people, seized with fear, had abandoned their sick, only one death occurred among a number of cases treated in separate rooms with fair attention. . . The people chose swampy sites for their dwellings, and whether they kept close shut up in huts without ventilation, or rushed into the streams and remained in the water during the height of the illness, the consequences were equally fatal. The excessive mortality resulted from terror at the mysterious seizure, and (from) the want of the commonest aids during illness. . . Thousands were carried off by want of nourishment and care, as well as by dysentery and congestion of the lungs. . . We need invoke no special susceptibility of race or peculiarity of constitution to explain the great mortality.'

"But it is not necessary that we should seek in so distant regions and among uncivilized peoples for proofs of the disastrous influence of unfavourable hygienic conditions upon the type of epidemics of measles on a large scale. In the epidemic which prevailed in 1866 among the Confederate troops during the American Civil War, there were 1900 deaths out of 38,000 cases of sickness. In the official report it is stated that 'the disease resembled ordinary measles in adults, except when aggravated by the effects of crowd, poisoning, or other depressing influences'; in two large hospitals the mortality amounted to 20 per cent. of the sick. In Paris during the siege (January 1871), out of 215 of the Garde Mobile who took measles, 86, or 40 per cent., died; and the mortality very nearly reached the same figure among the French troops who returned to Paris after the Italian War, 40 out of 125 cases dying in one hospital (whose sanitary condition was bad), with severe intestinal symptoms. Speaking of the disastrous epidemic of measles in the National Army of Paraguay, Masterman says—'At the beginning of the Brazilio-Paraguayan War, an epidemic of measles swept off nearly a fifth of the National Army in three months, not from the severity of the disease, for I treated about fifty cases in private practice without losing one, but from want of shelter and proper food.'

"I will not say that these considerations enable us to understand completely why some epidemics of measles are of a severe type; there may be other factors acting on the physiological disposition of the people in a given locality, or there may be a concentration of the morbid poison, determining the unfavourable type of the epidemic. But the favourite phrase, *constitutio epidemica*, does not help us at all in the elucidation of the question."—Hirsch, vol. i. pp. 167-9.

"Until a few months ago measles had not entered this group (Samoa). It was conveyed to Tonga, 500 miles south of us, by the New Zealand steamer *Upolo* in June last, and from all accounts we have received it nearly decimated that group. The same steamer brought the contagion to our group nearly three months afterwards. Here, as in Tonga, the epidemic was at first mild. Comparatively few died at Samoa during the period of the fever and rash. The sequelæ and complications have caused the mortality. I have not been able to obtain accurate statistics of the deaths from this recent epidemic throughout Samoa, as the ten inhabited islands of this tropical and volcanic group lie between five parallels of longitude, or, with the intervening straits, cover nearly 270 miles; but judging from the accurate returns obtained here, including a fifth of Samoa, and also from reports obtained from missionaries and others, no fewer than 1000 of the entire population of 31,500 died from measles up to the end of December 1893, and nearly half of these adults. Since then there have probably been a few hundreds more.

"The epidemic was not malignant. Our mortality has arisen principally from gastritis, enteritis, diarrhœa, and dysentery. A few died from suppressed measles. The craving the natives manifest for raw fish, unripe or over-ripe fruit, and especially half-cooked fresh pork, became morbid during the period of convalescence.

Many, lest they should be told to avoid these, abstained from procuring foreign medicine. Nine-tenths of the deaths could have been prevented by care in diet. The worst cases of diarrhœa and dysentery brought to me yielded to treatment. Cases under one's own personal supervision, and where instructions were followed, recovered. With the common strumous diathesis it has excited no surprise to see so many adults as well as children suffering from enlarged suppurating glands in cervical and submaxillary regions, and in the groin, &c.; not a few had parotid abscess with suppuration. Numerous abortions and cases of premature labour occurred, but none died with ordinary treatment. Single and multiple abscesses are an every-day occurrence here, but these have multiplied nearly tenfold since the advent of measles. Before the rash had disappeared a large number of adults passed intestinal worms by the mouth.

"Now that two months have elapsed since the last case of fever and rash, a mild persistent form of remittent fever is prevailing. This with glandular and respiratory affections are the most common ailments at this season.

"In the mission dispensary I am daily seeing cases of sickness the starting-point of which was measles. The two epidemics of influenza at the end of 1891 and January 1893 increased the tendency of Samoans to chest affections. Measles will be found to have still further intensified their susceptibility to respiratory diseases; and the frequent deaths, as well as the many debilitated natives one daily meets with, give evidence that we have not yet reached the end of the measles epidemic—an epidemic which will long be remembered, as not one of the entire population seems to have escaped."—Dr. Davis, *British Medical Journal*, May 19, 1894.

Judging from the above, it is evident that whenever measles has proved especially fatal to the individuals of an Old World race, it was when they were placed under very abnormal conditions—when their vitality was reduced by excessive privations, and their powers of resistance were thereby broken down. Forty per cent. of the Garde Mobile, and of the troops returned from Italy, who took the disease, died, but I am not aware that any European army, in however evil case, ever lost through measles one-fifth of its total strength, as did in South America the "National Army," among the ranks of which there were presumably many individuals of Indian or mixed blood. No records exist showing that Old World communities, however unfavourably circumstanced or however savage, have ever suffered like "the natives on the banks of the Amazon, where the number of those that died of the sickness was reckoned at 30,000, whole tribes having been cut off; also in Astoria in 1829, where nearly half of the natives fell victims to the disease." Nothing of the kind has been known to happen in Africa, for example, in the latitude of the Amazon, in countries where the natives are fully as savage as the South American Indians. In the examples quoted, Hirsch mentions only two instances in which Old World races have suffered very severely, *i.e.* in South Africa, the very extremity of the Old World, and in Mauritius, an outlying island; and the mere fact that measles proved so death-dealing on a sudden in those lands, proves that the races inhabiting them must have been but little afflicted by the disease, since comparatively so few individuals can have acquired immunity through infection and recovery in childhood, as so many of us in Europe do. As regards the epidemic in Fiji, we may be permitted to doubt that in such a latitude such a savage race dwells in "huts without ventilation." Moreover, the Fijians, as well as the other savage races mentioned, were attacked by the disease when in normal health, and in an environment to which Natural Selection had adapted them. The survival of the fittest had inured them to their swamps and streams and other surroundings, but not to measles. That they suffered from sequelæ only proves how much the disease injured them. It certainly appears true that measles is a disease so mild that, in the great majority of instances, infected individuals, even of races that have had no experience of it, may be saved by proper treatment; but it is certainly true also, that individuals of such races are vastly less resistant to it than those of races which have long been afflicted by it—that they take the disease in its severer forms, and under unfavourable conditions succumb more easily.

"We all know the story of the measles in Fiji, how in 1876 it swept away forty thousand out of the population of one hundred and fifty thousand. Measles, when it attacks the Polynesians, is no longer the infantile malady we know of. It becomes a devastating plague. The Tongans, with the experience of Fiji in their memories, took, it is true, some precautions against the after-effects of the disease; but nevertheless one-twentieth of the population was carried off, and the remainder was so demoralized that it was threatened with famine."—Thomson, *The Diversions of a Prime Minister*.

Dysentery, Diarrhœa, Enteric Fever, &c.—Malaria, tuberculosis, and measles afford ample proof that man's present evolution is mainly against disease, and moreover, that in different countries the direction of the evolution is different because different diseases differently determine it. Similar evidence is afforded by a study of many other diseases, for these, like malaria, tuberculosis, and measles, are invariably more fatal to strangers from beyond their areas of distribution than to races that have long dwelt within the districts they infest; but because few diseases are so prevalent within their areas of distribution, or so fatal as the three already discussed, at least as regards malaria and tuberculosis, evolution against few others is so marked as against them. Moreover, most of the other very prevalent and fatal diseases against which considerable evolution has occurred are those affecting the alimentary tract, *i. e.* are those in which, generally speaking, infection depends on the character of the food and water supply, and therefore, as regards them, since strangers who enter the infected districts are usually Europeans, more careful as to their food and water supply than the natives of Africa and Asia, where these diseases chiefly prevail, they usually suffer from them less in proportion to their susceptibility than the natives. For instance, Europeans actually suffer less from cholera in India than do the natives, though their great susceptibility is proved by the history of various pandemics, during which the disease overpassed its normal boundaries, and ravaged Europe. It is therefore difficult to measure with any degree of accuracy the extent of the evolution undergone by races that have had extended and disastrous experience of these diseases, for we cannot with accuracy contrast them with races which have had little or no experience of them, and have therefore undergone no evolution in relation to them. Nevertheless, in all publications in which the subject is alluded to—official reports, medical works, travels, &c.—we continually meet statements, collectively so numerous that volumes might be filled with them, that tend to show that the evolution has been considerable. Thus we read that, owing to malaria and dysentery, Muscat is uninhabitable by Europeans during the summer months;[1] that in the districts infested by yellow fever strangers are vastly more liable to attack than natives;[2] that in Ceylon the mortality from dysentery among the troops reached the

[1] Hirsch, vol. iii. p. 293. [2] *Ibid.* vol. i. p. 340.

ratio of 230 per thousand,[1] and so forth. The following tables, extracted from the official *Report on Sanitary Measures in India in* 1892-93, exhibits the difference between European and native troops in relation to the sum-total of the diseases prevalent in India. In contrasting the two races it should, however, be borne in mind, that while the whites on the one hand, since they are mostly unmarried, suffer more from the venereal diseases than the natives, whereby their sick and death-rates are unduly raised, the natives, on the other hand, are more exposed to infection by the other diseases. It should be noted also, that many of the whites who were invalided home would have died had they remained in

ABSTRACT OF STATISTICS OF EUROPEAN TROOPS IN INDIA.

Year.	Average Annual Strength.	Ratio per 1000 of Strength.				
		Admissions.	Constantly Sick.	Deaths.	Invaliding.	Total Loss.
1870-79 .	57,742	1475	60	19·34	43	62
1881-90 .	61,399	1471	73	14·24	27	42
1882-91 .	62,229	1448	74	14·17	26	40
1891 . .	67,030	1379	79	15·89	27	43
1892 . .	68,137	1517	84	17·07	24	41

ABSTRACT OF STATISTICS OF NATIVE TROOPS IN INDIA.

Year.	Average Annual Strength Present.	Ratio per 1000.				
		Admissions into Hospital.	Constantly Sick.	Deaths from		Mortality including absent Deaths.
				Cholera.	All Causes.	
1877-81 .	118,669	1422	48	1·94	24·90	27·40
1881-90 .	116,712	1054	35	1·28	13·44	16·91
1882-91 .	118,111	1020	34	1·45	13·09	16·61
1891 . .	128,600	972	35	2·64	15·44	19·34
1892 . .	127,355	1092	37	2·14	14·97	18·67

[1] Hirsch, vol. iii. p. 296.

India, and therefore that the total death-rate of the Europeans should be calculated rather on the basis of "total loss" than of "deaths" when comparing it with the "mortality, including absent deaths," of the natives.

The following table shows the extent of child sickness and mortality among the whites in India:—

STRENGTH, SICKNESS, AND MORTALITY OF CHILDREN IN THE ARMY OF INDIA.

Year.	Average Strength.	Admission-rate per 1000.	Constantly Sick-rate per 1000.	Death-rate per 1000.
1881-90 . .	6286	633·5	23·4	50·22
1882-91 . .	6220	609·4	22·2	50·63
1891 . . .	5886	509·0	19·2	49·27
1892 . . .	5762	564·6	22·0	48·59

It is not necessary to pursue this subject any farther. I take it that the facts are indisputable. No one doubts that races which dwell within the area infested by any prevalent and death-dealing disease are more resistant to it than races from beyond the area; the only possible question is as to how this difference in resisting power arose. It is commonly said to be due to "acclimatization," a word which, when applied to races, even by the ignorant, is practically a synonym for evolution, though this is not generally understood; a race that has undergone acclimatization is of course one that has undergone evolution. But this acclimatization, this evolution, is popularly supposed to result from the accumulation of acquired traits. I trust I have sufficiently proved that such is not the case, but that it results solely from the accumulation of inborn variations.

MENTAL EVOLUTION

CHAPTER I

In a former page, when endeavouring to ascertain the direction of man's present evolution, it was remarked—"If we would discover in what direction the evolution of an evolving species is proceeding, it is a good plan to note which members of it survive and which perish. We shall then, by observing the difference in qualities between the fit and the unfit, be able to discover what traits favour survival, and therefore what traits are undergoing evolution." We then came to the conclusion, that since men perished mainly of disease, Man's Present Evolution must be mainly against disease. But beside disease there are in various countries certain other agencies, which are the cause of a great elimination of the unfit, either directly, or more often indirectly, by producing deadly disease or considerable incapacity, and which are therefore causes of evolution wherever they are present. Such are various narcotics, the use of one or other of which is prevalent over by far the greater part of the globe, and with which the unfit are poisoned, and with more or less rapidity destroyed—alcohol, opium, hashish, &c.

Alcohol.—It cannot be questioned that alcohol is a poison, and the cause of many deaths. Opinions are divided as to whether it is ever beneficial, and therefore a cause of survival, but certainly there can be no doubt that it is very frequently deleterious, and therefore a cause of elimination. The following facts and figures, which I extract, with the author's kind permission, from Dr. Ridge's valuable and suggestive work, *Alcohol and Public Health*,[1] which should be read by every one interested in the subject, places this beyond doubt. It should be stated, however, that Dr. Ridge, a distinguished advocate of total abstinence, is in no way identified with the opinions expressed by me; for them I alone am responsible.

"CHAPTER V.—ALCOHOL, LIFE AND HEALTH. 59. The effects of alcohol on the longevity and health of the nation can be tested by the results of its use, comparing together abstainers and non-abstainers under as nearly as possible the same conditions.

"Perhaps the most telling comparison is that furnished by the United Kingdom Temperance and General Provident Institution, a life office which was founded in 1840 for abstainers only, but afterwards admitted non-abstainers into a separate section. The expected and actual claims in each section for the last twenty-five years have been published, as shown in the Table on the next page.

"60. This gives a mortality in the Temperance Section of 71·49 per cent., and in the General Section 96·66 per cent., a difference in favour of the former of 26·17 per cent. There were 1433 fewer deaths than expected in the former section, and 243 fewer in the latter, both being calculated by the same life tables. If the members of the General (non-abstaining) Section had lived on the average as long as the abstainers, there would have been only 5130 deaths instead of 7034, a saving of 1904 lives. Similarly, if the abstainers had used alcohol in the same way as the others, and had died at the same rate as they, the deaths would have been 4693 instead of 3423, a loss

[1] *Alcohol and Public Health*, by J. J. Ridge, M.D., London. H. K. Lewis.

	Temperance Section.		General Section.	
Year.	Expected Claims.	Actual Claims.	Expected Claims.	Actual Claims.
1866	100	85	180	186
1867	105	71	191	169
1868	109	95	202	179
1869	115	73	212	201
1870	120	87	223	209
5 yrs.	549	411	1008	944
1871	127	72	234	217
1872	137	90	244	282
1873	144	118	253	246
1874	153	110	263	288
1875	162	121	274	297
5 yrs.	723	511	1268	1330
1876	168	102	279	253
1877	179	132	291	280
1878	187	117	299	317
1879	196	164	305	326
1880	203	136	311	304
5 yrs.	933	651	1485	1480
1881	214	131	320	290
1882	225	157	327	295
1883	235	174	333	301
1884	247	196	342	283
1885	258	177	348	361
5 yrs.	1179	835	1670	1530
1886	271	171	354	337
1887	282	219	360	363
1888	298	216	372	335
1889	307	184	378	326
1890	314	225	382	389
5 yrs.	1472	1015	1846	1750
25 yrs.	4856	3423	7277	7034

of 1270 lives. Again, if all had been non-abstainers, the deaths would have been 11,727; if all had been abstainers, they would have been 8553, a difference of 3174 deaths. This represents the true measure of the injury done to a number of picked lives by the use of alcohol.

"61. It has been objected that these statistics do not furnish any criterion of the effect of the strictly moderate use of alcohol, because some of those insured in the General Section die of alcoholism, cirrhosis, &c., and are excessive drinkers. It is true that there are some drunkards, and probably some who considerably exceed the limit of one and a half ounces of pure alcohol laid down by Dr. Parkes. As a physiological experiment it is open to the objection to some extent, but yet it is obvious that excessive drinking is not very common among them, because the death claims in the General Section are below the calculated number, notwithstanding that the lives of abstainers are excluded, which would have reduced the number of percentage of claims.

"62. It is extremely probable that if a number of abstainers could be compared with a number of non-abstainers, who never in their whole lives exceeded one ounce of alcohol a day, the difference would not be as great as in the present instance. But this is altogether a hypothetical and, one may almost say, impossible case. The experiment is a fair test of the effects of the moderate use of alcohol, and its consequences under present social conditions. One of the inevitable results of the moderate use of alcohol, is that a percentage of the drinkers will in the course of time increase the amount, and become more or less excessive drinkers. To leave these out of the comparison would be as unfair as to leave out all persons with 'dropped wrist' when comparing those who drank water with lead in it and those who did not. Excessive drinking, due partly to an increased tolerance of alcohol, partly to a growing craving for it, partly to habit and other causes, is one of the morbid consequences of the so-called moderate use of it, just as much as any other pathological change.

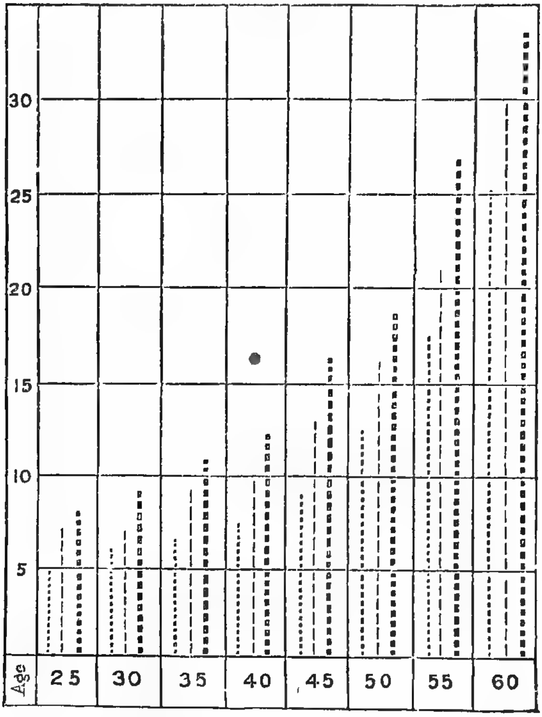
It is one of the risks to which every moderate drinker renders himself liable.

"63. It has been alleged that the difference arises from the fact that when members of the Temperance Section cease to abstain they are transferred to the General Section, and that this alteration would be brought about by the failure of health among the more delicate, and hence that these would die earlier and swell the number of the General claims. The actuary of the office has stated that during the ten years 1881-90 the number so transferred was 470, while 725 were transferred the other way. But of the 470 only 44 have died, equal to about 0·94 per cent. per annum; while of the 725, 84 have died, or about 1·16 per cent. per annum. It is clear, therefore, that the transference of members will not account for the great difference in favour of the abstainers.

"64. The same fact comes out in the experience of the Sceptre Life Office. This is a much younger and smaller office, and draws a large number of its lives from members of one religious denomination. The majority of them are steady, good lives. The abstainers are kept distinct. In the seven years, 1884-90, the expected claims in the General Section were 679, and the actual claims 527 = 77·61 per cent. In the Temperance Section the expected claims were 306, and the actual claims 174 = 56·86 per cent. Here, again, some transfers from the Temperance Section took place, namely, 397 in seven years, but of this number only seven died = 0·25 per cent. per annum. These transfers are made if information is not sent every year of continued abstinence, and as a fact many are transferred who still remain abstainers. Others cease to be abstainers for social reasons, and quite apart from the question of health.

"65. It may be taken as proved, that if a number of people are insured as abstainers, and an equal number are insured as non-abstainers, the total years of life of the former will considerably exceed those of the latter. The expectation of life, or mean after-lifetime, will be shortened by the use of alcohol. The materials which would enable us to construct a Temperance Life Table have not yet been published. We do not know the ages at death of those who have died in the two sections, nor the number or ages of those at risk. The average age at death in the Sceptre Office has been

RATE OF MORTALITY PER 1000.



1. Dotted Line = Rechabites.
2. Thin Broken Line = Twenty Life Offices.
3. Thick Broken Line = Foresters.

made public. It was 51·3 in the General Section, and 44·2 in the Temperance Section; but these cannot be compared, as we do not know the average age of the living. It is probable that the average age of the abstainers is lower than that of the non-abstainers, as the temperance movement is modern, and has spread most among the young.

"66. It has been alleged that those insured in the Temperance Section have a better constitution than the non-abstainers, and live longer as a result of this, and not on account of their total abstinence. This is purely imaginary, begging the question altogether. There is not a particle of evidence in support of it. The lives in both sections are drawn from the same classes scattered all over the kingdom, so that exceptional circumstances neutralize one another. If there be any difference it is in favour of the strictly temperate persons, as the abstainers include some who have at some former time drunk to excess, but have reformed.

"67. A similar difference in favour of abstainers is shown by the statistics of the Order of Rechabites compared with the Foresters. The Scottish Temperance Insurance Company has published the diagram on the opposite page, in which the experience of twenty leading life assurance companies is also given.

"68. The eminent actuary, Mr. F. G. P. Neison, has published the results of a careful actuarial examination of the reports of the Ancient Order of Foresters, the Manchester Unity of Oddfellows, and the Independent Order of Rechabites. The Foresters and Oddfellows each supplied more than one million and a quarter years of lives for examination, the Rechabites supplied 127,269, extending over ten years. Mr. Neison gives the following table, showing the rate of mortality in each—

RATE OF MORTALITY PER CENT. PER ANNUM.

Age.	Oddfellows. 1866-70.	Foresters. 1871-75.	Rechabites. 1878-87.
Under 25	·632	·753	·603
25	·788	·807	·509
35	1·094	1·174	·619
45	1·647	1·802	1·119
55	2·877	3·286	2·325
65	5·911	6·510	5·815

"69. The next table shows the number who survive to various ages, and proves that the abstainers have a much better prospect of long life—

The number that would survive to Age.	Of 1000 Persons all aged 18. Oddfellows. 1866-70.	Foresters. 1871-75.	Rechabites. 1878-87.
25	957	950	962
30	922	915	938
35	884	873	914
40	840	828	888
45	791	776	856
50	764	714	815
55	636	641	760
60	584	552	686
65	483	453	590
70	370	335	461
75	245	218	306
80	134	118	165

"70. He also gives a table showing the mean after-lifetime of the members of the same societies, as follows—

THE AFTER-LIFETIME.

At age.	Oddfellows. 1866-70. Years.	Foresters. 1871-75. Years.	Rechabites. 1878-87. Years.
20	41·3	40·2	45·1
30	34·0	32·9	37·3
40	26·7	25·8	29·1
50	19·9	19·1	21·2
60	13·6	13·2	14·2
70	8·5	8·3	8·5
80	5·0	4·9	4·9

"71. Mr. Neison states that, 'at the age of twenty the expectation of life among the Rechabites is more by about four years than the experience of the Oddfellows and Foresters.' But he goes on to say, that the 'probable lifetime' is better for comparison, and this shows, as between Foresters and Rechabites, a gain to the latter of 5·7 years at age twenty. This is an enormous advantage. It would mean on the 400,000 young men (about) who every year reach the age of twenty, about 2,280,000 additional years of life; allowing 39·4 years of the mean after-time of each adult at twenty, it would be equal to the addition to the population of 57,360 men, with all the wealth which they could create!

"72. The London Grand Division of the Sons of Temperance has had an average death-rate during the eleven years 1880-90 of 5·29 per cent., the average age at the end of 1885 being $32\frac{1}{3}$ years. The same low mortality is experienced in other parts of the world. An actuarial report on the Friendly societies of Auckland during five years showed the actual as compared with the expected mortality to have been, among 8315 abstainers (Rechabites and Sons of Temperance) 71·56 per cent., and among 49,698 non-abstainers, 93·34 per cent.

"73. It must not be forgotten that there are a considerable number of abstainers among the Foresters and Oddfellows; if these were withdrawn the mortality of the rest would be seen to be greater. There are also, unfortunately, many who drink to excess, and their mortality is highest of all. These two classes tend to counterbalance each other, and hence the comparison is, to a large extent, between abstainers and moderate or medium drinkers.

"74. The Collective Investigation Committee of the British Medical Association in 1888 published a series of returns furnished by several medical men on the connection between the age at death and habits with regard to alcohol. The results obtained were that the average age at death of males over twenty-five years of age was as follows:—

122	total abstainers	51·22 years.
1529	habitually temperate ...	62·13 „
977	careless drinkers	59·67 „
547	free drinkers	57·59 „
603	habitually intemperate ...	52·03 „

The figures referring to the abstainers are evidently fallacious, partly because the number (122) is too small to furnish an average result, and considerably below (28 per 1000) the average number of abstainers among the adult population, and partly because the average age of the living population (abstainers) is not given, but is certainly a good deal lower than the average age of non-abstainers.

"75. But the other returns indicate very clearly that the effect of alcohol in shortening life is very considerable, and in proportion to the quantity taken. Drunkards lose on the average about ten years of life as compared with strictly temperate men. These returns also show that about 15 per cent. of these adult males were habitual drunkards, and 15 per cent. more were decidedly intemperate, making 30 per cent. altogether. This would indicate the number of intemperate males in the United Kingdom as about 2,000,000, or a little over ten to each public-house. This estimate may seem high, and cannot pretend to be exact. But if the physiological limit of alcohol per diem be placed at 1½ oz. (Parkes), or even 2 oz. (Anstie), there can be no doubt whatever that excess is far more common than moderation, and the inevitable consequences follow.

"76. If any further evidence is needful, it can be found in the increased mortality of licensed grocers after the passing of the Act of Parliament in 1860, by which they were allowed to sell wines and spirits, thus getting increased facility for obtaining drink. In the Appendix to the Registrar-General's 39th Annual Report, the late Dr. Farr gave the following table—

MORTALITY PER CENT. OF GROCERS.

Ages, Years,	15	25	35	45	55	65	75 and upwards
1860-61	·531	·840	·923	1·280	2·053	4·334	12·488
1871	·592	1·115	1·021	1·466	2·567	5·461	13·442
Excess in 1871	·061	·275	·098	·186	·514	1·127	·954

There is no other possible cause for such an increase, and the chronic poisonous action of alcohol is established beyond a doubt.

"CHAPTER VI.—MORTALITY AND SICKNESS FROM ALCOHOL 77. Several attempts have been made to estimate the mortality due to alcohol. It is, of course, impossible to make an exact computation. There are so many ways in which alcohol affects the health and life of those that take it, and, indirectly, the life and health of others, both abstainers and non-abstainers. It is useless to look to the returns of the Registrar-General for this purpose. Comparatively seldom does the certificate attribute the death to its primary cause, even when there is no complication. But in a large number of instances the death which is certified as due to pneumonia or nephritis is as truly the result of alcohol as any inflammation arising from arsenic. Nevertheless, the deaths attributed to intemperance show a serious increase, being, in 1889, 54 per million persons living; in 1886, 49; in 1881, 47; in 1876, 46; in 1871, 32; and in 1866, 44. The deaths from cirrhosis of the liver increased from 2570 in 1876 to 3300 in 1889, the proportion of females slightly increasing as compared with males, being as 101 in 1889 to 98 in 1876.

"78. Dr. Norman Kerr, from the result of his own practice and that of twelve other medical men, estimated the direct and indirect mortality from intemperance at 128,000 per annum. He has since estimated the direct mortality at 40,000, and the indirect at 80,000. Dr. Morton, in conjunction with nineteen medical friends, arrived at the conclusion that the deaths in England and Wales, wholly or partly due to alcohol, were 39,287, equal to 52,640 for the United Kingdom. As a result of his inquiry, the Harveian Society of London instituted an investigation, and found that in London, of 10,000 persons dying over twenty-four years of age, the result was as follows:—

A.—Deaths in nowise due to alcohol . . .	8598
B.—Deaths accelerated or partly caused by its abuse	1005
C.—Deaths wholly due to it . . .	397

Of these, 7505 were certified by private medical men; 1183 occurred in workhouses, infirmaries, and lunatic asylums; 646 in hospitals; and 666 were certified by a coroner. These 1402 deaths constituted almost exactly 14 per cent. of the total deaths. If this proportion still continues (and, as we have seen, there is reason to believe it has increased), the total deaths in the United Kingdom for 1889, altogether or partly caused by alcohol, were 94,416, of which 26,736 would be directly due to alcohol, and 67,680 accelerated or partly caused by it.

"79. Even this appalling estimate, made after the closest scrutiny by a critical committee of medical men, does not tell all the tale. It does not include many cases in which the constitution, having been damaged by alcohol in years gone by, succumbs earlier than it otherwise would to the inroad of disease. It does not include all cases in which the body has been starved or injured by the neglect or cruelty of drunken parents, and has been handicapped in the race of life. In these and many other roundabout methods alcohol destroys life, so that it appears extremely probable that 120,000 is the lowest number which can be estimated as the annual loss of life due to the presence of alcohol in our midst, and its use as a beverage. This is between one-fifth and one-sixth of the total deaths.

"80. I have before referred to the report of the Collective Investigation Committee of the British Medical Association. In that inquiry, conducted over totally different ground, the deaths of intemperate males over twenty-five were 30 per cent. of the whole, while 25 per cent. more were careless drinkers, sometimes taking excess. This being the proportion among adult males, we find no difficulty in believing that about one death in seven is partly or wholly caused by alcohol, omitting for the moment those caused indirectly. All these results, arrived at in different ways, strongly confirm one another, and point to the irresistible conclusion that alcohol causes more deaths in the United Kingdom than any single disease, and justify all who are interested in the public health in devoting the most strenuous efforts to getting rid of it.

"81. It must not be forgotten that these statistics of mortality from alcohol represent a great deal more than so many deaths. The deaths from intemperance mean a more or less prolonged course of vicious drinking and all its attendant horrors affecting both the individual and the friends and neighbours. Intemperance is not simply a self-regarding vice; it is a danger and a loss to the State, and hence its causes and means demand recognition and, if possible, removal. It is necessary to realize, at least to some degree, the extent of the evil if the nation, or rather the individuals who compose it, are to be aroused to take steps to prevent it. No effectual measures are ever likely to be adopted until the intelligent portion of the public is thoroughly convinced that something must be done, and that speedily.

"82. I shall briefly consider the effects of alcohol on the nation under several heads—

"(A) *Sickness.*—The various forms of diseases, directly and solely due to alcohol, only need enumeration, such as delirium tremens, mania à potu, chronic alcoholism, alcoholic paralysis, and cases of true dipsomania. The extent and nature of other diseases caused or aggravated by alcohol requires further elucidation. If there be twenty cases of sickness for every death, then it would be easy to reckon up the excess of alcohol-caused disease. But this would be an exceedingly rough and fallacious method. The effect of alcohol can only be strictly determined when all other circumstances are either the same or are neutralized. The comparison of one Friendly society with another is not entirely satisfactory, as they differ to some extent.

"83. Some facts of this nature have already been alluded to. The London Grand Division of the Sons of Temperance was valued in 1881, on the preceding five years, by professional actuaries, Messrs. Gomme and Hatton. (See Table, page 320.)

"84. The experience of the Rechabites is rather different, and is evidently dependent on some cause or causes peculiar to that Order. One of these causes is that members have been admitted for years past at fifteen years of age instead of eighteen, as in the

Foresters (the last two or three years the Foresters' age has been reduced to sixteen), and those at fifteen who have been members of the juvenile branch have been admitted without medical examination. From these causes the rate of sickness at age eighteen was 1·005 week in the Rechabites, and ·875 in the Foresters, a difference of ·22 week, about 1½ day. The difference decreases year by year until age thirty-four, when the Rechabites have 1·117 week, and the Foresters 1·062

AMOUNT OF SICKNESS PER ANNUM FOR EACH MEMBER AT RISK.

Ages.		Sons of Temperance.	Oddfellows, M. U., Rural Towns and City Districts.	Oddfellows, M. U., Rural Districts.	Foresters.
		1875-80 Weeks.	1866-70 Weeks.	1866-70 Weeks.	1871-75 Weeks.
18—20	...	·41	·66	·63	·91
21—25	...	·54	·76	·77	·81
26—30	...	·52	·82	·84	·87
31—35	...	·66	·97	·97	1·01
36—40	...	1·06	1·08	1·06	1·18
41—45	...	·82	1·32	1·32	1·44
46—50	...	1·02	1·75	1·83	1·77
51—55	...	·97	2·35	2·45	2·48
56—60	...	·75	3·30	3·23	3·39
61—65	...	·73	5·13	4·68	5·12
66—70	...	Nil	8·06	6·90	8·68
		7·48	26·20	24·68	27·66

week only, ·055 week less. Then the difference increases again to age forty-one, when the sickness is 1·451 and 1·326 week respectively, the difference being ·125 week. It then rapidly falls, till at the age of forty-nine the difference in favour of the Foresters is only ·056, and at age fifty the tide has turned and the numbers are 1·933 (Rechabites) and 1·953 (Foresters), difference ·020. It then rises considerably to age sixty-seven, when it is at its highest, 6·055 (Rechabites) and 8·102 (Foresters), difference 2·047 weeks. It then falls till, after age seventy, the comparison ceases, as the numbers at risk do not warrant it. Taking the whole of life, the total difference is largely in favour of the abstainers, being 14·119 weeks less sickness.

"85. I quote these figures as they represent a singular exception to the general rule. I am aware that some time ago statistics were published of all the Friendly societies and sick clubs in East Grinstead, in which there were 57 abstainers and 1042 non-abstainers, the former having in one year 21 members ill, and the latter 188, and the results arrived at were stated by the compiler to be that drinkers had less sickness, but more severe and of greater duration when they did become ill, and more liable to a fatal result. This would be very satisfactory to the abstainers if it could be relied on, but the small number of abstainers and the short period of observation (one year only) render the results unreliable.

"86. It is more satisfactory when abstainers and drinkers in the same society can be compared for a number of years. Thus, the experience of the Foresters' Court at Streatham, London, for seven years was as follows—

Year.	No. of Abstainers.	Amount of Sick Pay. £	s.	d.	No. of Non-Abstainers.	Amount of Sick Pay. £	s.	d.
1869	22	1	5	0	98	95	15	0
1870	25		14	0	111	90	6	0
1871	45		Nil		105	68	0	0
1872	37	20	17	0	139	104	15	4
1873	44	23	8	0	131	123	17	2
1874	45	1	8	0	113	116	10	8
1875	46	8	19	0	99	110	12	4
Total 7 years.		56	11	0		709	16	6

This gives an average of 4*s.* 3¼*d.* per abstainer, and 17*s.* 10*d.* per non-abstainer.

"87. In the Loyal Prince Alfred Lodge of Oddfellows, Kelvedon, Essex, from 1876 to 1880 the results were as follows—

	Abstainers.	Non-Abstainers.
Proportional number...	1	3
Sick Pay	£17 3*s.* 10*d.*	£172 17*s.* 4*d.*
Days of Sickness ...	205	2287
Funeral Pay	Nil	£20

"This return shows that the drinkers had more than three times as much sickness as the abstainers in proportion to their number. But in these instances the numbers are small, and must, therefore, be received with caution; they only have weight as they correspond with other and larger returns.

"88. The army and navy present the most hopeful field for the settlement of this point, because in these services the men are under strict observation, and under practically the same conditions in other respects. The number of abstainers in the Indian Army is now over 17,000, and there are considerable numbers in the army at home. There are also a great many abstainers in the navy, almost every ship having representatives. It is therefore very desirable that an inquiry should be instituted, certain diseases being omitted which would unfairly handicap the non-abstainers, for there can be no doubt that total abstinence is more practised by the steadiest men in other respects, though it is also true that the fact of abstaining assists in promoting self-respect and continence, other things being equal.

"89. I have before referred to the returns from the Madras European soldiers in 1849, which showed the admissions to hospital to be—

450 Abstainers,	130·888	per cent. of strength.
4318 Temperate,	141·598	,, ,,
942 Intemperate,	214·861	,, ,,

"90. I have also mentioned the returns from 3878 abstainers and 8829 non-abstainers in the Indian Army in 1885-6, showing more than twice as high a percentage of admissions to hospital among the latter. To these may be added the following from Indian Army hospital returns—

		Abstainers.	Admissions.	Non-Abstainers.	Admissions.
1878*	60th Rifles, 2nd Batt.	273	75	294	130
1874-75†	65th Regiment	252	59	621	357
1874†	5th Fusiliers, 1st Batt.	229	38	487	365
1877-78‡	54th Regiment ,, ,, 25th ,, 1st Batt. ,, ,, 92nd Highlanders	791	567	1621	1663
	Total	1545	739	3023	2515

* 5 months. † 6 months. ‡ 12 months.

This gives the following percentage of admissions—

Abstainers ... ... 47·8 per cent.
Non-Abstainers ... ... 83·2 ,, ,,

"91. The table previously given, compiled from Dr. Ogle's report, also proves conclusively that alcohol not only produces its own special diseases, but renders its consumers more liable to die from other complaints, such as diseases of the nervous and circulatory systems, gout, and phthisis, and even from suicide.

"92. In 1872 Dr. Dickinson gave the details of diseases, verified by post-mortem examination at St. George's Hospital, of 149 traders in liquor and of 149 men who belonged to all kinds of other trades. Comparing them as to exposure to the weather, it was found that of the alcoholic traders four-sevenths worked indoors; of the others, three-sevenths. The average age of the liquor-traders at death was 36·8 years; of the others, 40·6 years.

ALCOHOLIC.	NON-ALCOHOLIC.
More frequent.	*More frequent.*
Empyema.	Pneumonia, rather more.
Tubercular diseases, much more.	
(*Multiple tubercle*, 61 *cases.*)	(*Multiple tubercle*, 44 *cases.*)
Tubercles of brain, liver, kidneys, bowels, mesenteric glands, and peritoneum, *twice* as common.	
Atheroma and fatty degeneration of heart and arteries.	

ALCOHOLIC.	NON-ALCOHOLIC.
More frequent.	*More frequent.*
Pericarditis less frequent, but more often suppurative.	Endocarditis and pericarditis.
Hypertrophy of heart.	
All kinds of cerebral inflammation and hæmorrhages.	
Repair slow and unsafe, by tendency to suppuration and cellulitis.	Fibrinous exudation and adhesive inflammation.
Renal changes, 82 *cases,* congested and granular kidneys.	*Renal changes,* 83 *cases,* lardaceous disease.

"93. Dr. Dickinson summed up his paper by saying—'Alcohol causes fatty degeneration and fibroid encroachment; it engenders tubercle, encourages suppuration, and retards healing; it produces untimely atheroma, invites hæmorrhage, and anticipates age. . . . Alcohol gives an asthenic type to disease . . . it retards adhesive and plastic processes.'

"94. In confirmation of these observations the experience of the last Russo-Turkish war may be cited. The Turkish soldiers, though much more poorly clad and fed, recovered from wounds which proved rapidly fatal to the vodka-drinking Russian. This was so marked as to be notorious, and was noted at the time by several newspaper correspondents.

"We cannot but conclude, therefore, that the disappearance of alcohol from popular use would be attended by a considerable improvement in the health of the nation."

CHAPTER II

Dr. Ridge's statistics place beyond question the fact that alcohol is a considerable cause of mortality. But his figures, however significant, are by no means significant enough. Alcohol not only causes disease and death among the bread-winners of the community, to whom his statistics chiefly refer; it is also a cause of destitution to their families, and therefore a factor in the elimination of those who, inheriting the inborn traits of their progenitors, would in the next generation indulge in it to excess. Every shilling spent on drink is a shilling less for food, clothing, shelter, and the provision of better sanitation. Moreover, apart from the question of disease and death, individuals who indulge in alcohol to excess, *i. e.* to such an extent as to leave them appreciably poorer, or to such an extent as to damage their health, must on the whole have fewer offspring than those who do not so indulge, for the reason that they are less able to support wives and families, and because men and women are generally unwilling to marry the intemperate.

In generation after generation alcohol is therefore the cause of a considerable elimination of the unfit in relation to it, and therefore, like a very prevalent and deadly disease, in generation after generation it must be the cause of a considerable evolution against itself. This evolution may be in one or both of two directions; it may be in the direction of an increasing power of tolerating the poison, or in the direction of an increasing power of avoiding it, that is, of abstaining from it, or both, *i. e.* it may result in an increased power of imbibing alcohol without ill effects, or it may result in a diminution of the craving for it, or both. In the presence of an abundant supply of alcohol and of a craving for it, it can hardly result to any great extent in an increased power of toleration, for, under such circumstances, the drunkard would simply drink more, and thereby poison himself as effectually as a less resistant person would with a smaller quantity. The evolution against alcohol must therefore be in the direction of an increased power of avoiding it—in a diminution of the craving for it. *À priori*, therefore, we should expect that races that have long been familiar with alcohol, like races that have long been familiar with a very prevalent and deadly disease, are less harmfully affected by it than races that have had little or no experience of it, and this because they crave less for it, and therefore drink less of it. *À posteriori*, this is exactly what we do find. The peoples inhabiting the northern coast of the Mediterranean, the Greeks, the Italians, the South Frenchmen, and the Spaniards, who have lived for thousands of years in the presence of an abundant supply of alcohol, are pre-eminently temperate, whereas savages of all races, who have had no racial experience of it, or a slight experience only, the natives of North and South America, Australia, Polynesia, Africa, Greenland, &c., whether inhabiting the Arctic, the Temperate, or the Torrid zone, crave for it to such a degree that, unless we protect them by prohibitory laws, they perish in its presence. Moreover, races that, in their experience of alcohol, are intermediate between the Italian and the North American Indians, crave for alcohol, and are inclined to excessive indulgence in it, more than the former and less than the latter, *e.g.* the inhabitants of North Europe, the English for example.

The question as to how the craving for alcohol and other narcotics, the love for those frames of mind which they severally induce, arose, can be answered in one way only. It can have arisen only as a bye-product of mental evolution, a bye-product which, in the absence of narcotics, was harmless, but which in the presence of them is harmful, and against which, in races long afflicted by this or that narcotic, a secondary evolution has occurred; just such a bye-product as the paresis which accompanies the life-saving faculty of fear (*e. g.* in birds or frogs, "fascinated" or frightened by snakes), against which also a secondary evolution is also doubtless occurring. In intellectually the highest animals only is the love for that state of mind which narcotics induce present, and only in the highest of all animals, *i. e.* only in man, is the craving for that state of mind present in the greatest degree. Elephants and monkeys, but not fish or reptiles, for instance, can be brought to enjoy indulgence in alcohol; in man alone is the love for such indulgence easily awakened.

I conceive it is impossible that any thinking person will deny the reality of this evolution. Even though it should be denied that the whole organic world has arisen by evolution, it must be admitted by all that the races of mankind, originally alike, because descended from a common ancestry, have diverged each one from all the others in this or that particular, in shape, in colour, in size, in the power of resisting disease, in the craving for alcohol, &c., through a process of evolution, which can only have been through the accumulation of inborn or of acquired variations, or both. We have already considered at length the question as to whether acquired variations are transmissible, and have decided that they are not, and therefore we may conclude, *à priori*, that traits acquired under the influence of alcohol are also not transmissible. But as regards the evolution that has been produced by alcohol, just as regards the evolution that has resulted from disease, we have abundant reason to conclude on *à posteriori* grounds also, that no part of it has resulted from the accumulation of acquired traits; for were the acquired effects of alcoholism transmissible, *i. e.* were that part of the craving for alcohol, that increase of it which results from the indulgence in the poison, transmissible, then races that have had the most extended experience of alcohol should, of course, crave most for it, and be the most drunken. The contrary, however, is the case, for races that have long been familiar with it are conspicuously temperate, whereas races which have had little or no familiarity with it are conspicuously intemperate when opportunity is afforded for indulgence.

The evolution against alcohol has therefore resulted solely from the accumulation of inborn variations through the survival of the fittest. To prevent any misapprehension it should be noted, however, that alcohol, like disease, has not directly, but indirectly only, been the cause of evolution. It has been a factor of elimination, not of survival. Those individuals whom it has greatly affected have tended, other things equal, to perish and leave no offspring, whereas those whom it has affected little or not at all, have tended to survive and continue the race. In this it differs from a beneficial agency such as food, which, since it is a factor of survival, not of elimination, is a more immediate cause of evolution.

Even more than disease alcohol has been supposed to furnish a case of the transmission of acquired traits. Among the vulgar, who commonly repudiate the theory of evolution, the belief is universal that an indulgence in alcohol by the parent tends to result in a craving for it in the offspring greater than would otherwise arise, and this notwithstanding the fact that the logical outcome of such a belief should be a belief in evolution. Among biologists, even those who support the theory that acquired traits are not transmissible, do it in this instance haltingly, as though they were of the opinion that their position is here at its weakest. I have, with amazement, seen it argued that the apparent transmission of an acquired craving for alcohol is due to the circumstance that the drunkard's germ is bathed in alcohol, whereby it is habituated to it, *i. e.* whereby it, practically speaking, acquires a craving for alcohol, which is afterwards manifested in the individual into which it proliferates. But the craving for alcohol depends of course on consciousness, which in turn depends on the presence of nervous structures—at least we have no evidence that in the absence of nervous structures consciousness anywhere exists; for instance, in the protozoa and in plants. In the germ there are no nervous structures, and therefore, *à priori*, it can have no consciousness; *à posteriori*, we have no shadow of reason for supposing it has any, for supposing it thinks or feels more than a plant. The mere fact that it represents in the ontogeny a stage in the phylogeny when plant life had not diverged from animal life negatives the idea; as does also the fact, that consciousness would be utterly useless to it, and therefore, even had consciousness been present in its prototype of the phylogeny, that faculty would have undergone such retrogression in successive germs as long ago to become non-existent. It is not to be denied that the alcohol drunk by an intemperate person may affect his or her germs, possibly or probably in an injurious manner, but it is most strongly to be denied that the germs are so affected by alcohol as to cause them, after conjugation, to proliferate into organisms in which the craving for drink is greater than would otherwise have been the case.

Were that so a race that was introduced to alcohol would become, other things equal, increasingly drunken in successive generations, exactly as if by the transmission of acquired traits, for in such a case the son would inherit the father's craving plus an increment to it, caused by the presence of alcohol in the fluids that bathed the father's germs, and would therefore drink more; the grandson would inherit the son's increased craving, plus a further increment caused by the more drunken habits of the latter, and would in consequence drink still more; so also as regards the great-grandson and subsequent descendants, till, in the presence of alcohol, the race would become more and more unfit, and would ultimately suffer extinction. But, as we have seen, races longest familiar with alcohol manifest the least craving for it, and therefore, *à posteriori*, we have every reason for believing that alcohol does not so affect the germs as to cause, in the individuals which spring from them, a craving for alcohol greater than would otherwise arise. When, therefore, a drunken son succeeds a drunken father, we must conclude that his tendency to excess is due solely to the inheritance of an inborn trait, and not to the inheritance of an acquired variation, nor yet to any effect produced by the poison on the germ whence he sprang. But since a fœtus has nervous structures, drunken habits in a pregnant woman may affect her unborn child, in such a manner as to render the individual into which it subsequently grows more drunken than he would otherwise be. But even in such a case, it is probable that the resulting variation is not of such importance as is commonly supposed; since between the birth of the affected individual and his arrival at an age when, under the ordinary conditions of society, indulgence is possible, so long an interval occurs that the variation is probably entirely lapsed.

The evolution against alcohol appears to have proceeded on lines and under conditions very similar to those under which the evolution against zymotic diseases, particularly against tuberculosis, proceeded. We have seen that in early times the environment must have been very unfavourable to the tubercle bacillus, but that with the advance of civilization it must have grown increasingly favourable, till in the most civilized communities it is, at the present day, extremely favourable; and that concurrently with the change of environment favourable to the bacillus, but unfavourable to the races attacked, these races underwent a protective evolution, which has proceeded so far that they are now able to persist under conditions so favourable to the bacillus and unfavourable to themselves, that races which have not undergone the same evolution perish under like conditions. Similarly in early times the conditions must have been very "unfavourable" to alcohol. It must have been very scanty as to quantity and dilute as to quality.

"The ancient wines were of various kinds, some being the pure and unfermented juice of the grape, preserved either by sulphuring or inspissation; others sour, in which some or all of the alcohol had undergone acetous fermentation. But even the most alcoholic wine did not contain more than 15 per cent. of alcohol, and many contained much less. There was no such practice as 'fortification,' at all events not with alcohol, though there is evidence that some other drugs were sometimes added to increase the intoxicating quality. All, or almost all, the intoxication of which we read in classical and Biblical history was produced by what we now call 'natural wine,' or by a kind of beer made from malted grain, or by a few other alcoholic drinks (such as mead from honey, &c.), which are of minor importance."—J. J. Ridge, *Alcohol and Public Health*, p. 3.

The ancient conditions as regards alcohol, like those regarding tuberculosis, are still persistent in various savage countries, in consequence of which we are enabled to institute comparisons between savage and civilized races, and therefore, in effect, between ancient and modern communities. In Guiana, for instance, the natives manufacture an intoxicant from cassava, of which a debauch of from thirty-six to forty-eight hours is necessary before the wished-for state of drunkenness is attained. The art of manufacturing alcohol must have been as imperfect among our early ancestors as among the Indians of Guiana; their first intoxicant must have been as dilute, indulgence to excess must have been at least as difficult, and the elimination of the unfit equally small. Under such conditions the least resistant individuals only of those that had the best opportunities for excessive indulgence can have perished. As the art improved, as intoxicants of greater and greater strength were manufactured, as indulgence to excess became more and more easy, Alcoholic Selection must have been exercised with greater and greater severity, and as a consequence, the evolution of the races affected must have proceeded further and further, with the result, in modern days, that some races have evolved so far that they are able to persist in the presence of what is practically an unlimited supply of alcohol. In Italy, for instance, until lately the peasantry, for a very trifling sum, drank at their taverns by the hour, not by the quantity, a custom that afforded great facilities for intemperance.

Various explanations other than that given above have been offered of racial differences in respect to over-indulgence in alcohol, all of them extremely unsatisfactory.

It is said that some races are by nature abstemious. This is certainly true, but no attempt has been made to explain how this difference in "nature" arose. It must have arisen by some process of evolution, and I submit that it can have arisen only through the operation of Alcoholic Selection; through the continued survival, in the presence of alcohol, of those that craved least for it, and the elimination of those that craved most for it. The Greeks and Italians are notoriously more passionate and less self-restrained than the English, *i. e.* the mental traits they inherit or acquire from their progenitors tend less than with us to create a power of self-restraint; but they are notoriously more abstemious as regards alcohol than the English. Whence this difference, this superiority of self-control in one respect, which belies their characters in all other respects, if not from the survival of the fittest? If ancient literature can be trusted, the classic races were formerly much more drunken than at the present day. In Sparta, for instance, the question of temperance was a burning one, and unhappy Helots were made to furnish "fearful examples" to the aristocratic youth. In England at the present time, of all the community, the upper classes, though possessing the best opportunities for indulgence, are, on the whole, the least addicted to drunkenness; that is, those individuals who, generally speaking, have descended from ancestors to whom wealth afforded opportunities for excessive indulgence (*i. e.* for poisoning themselves), are on the whole more temperate than those who have descended from an ancestry to which poverty denied similar opportunities; and this notwithstanding the fact that to the poor man excessive indulgence means ruin for himself and his family, since food and shelter compete with alcohol for his resources; whereas the rich man often need suffer nothing more than ill-health as a consequence of overindulgence.

It is said that racial differences in the craving for alcohol, or at any rate in the indulgence of it, are due to differences in education and example, but it is forgotten that the craving for alcohol, albeit a bye-product of mental evolution, is an instinct, not an acquired trait. It is comparable to hunger or thirst, or to sexual or parental love, not to a love of books, or of paintings, or of scenery, or of country, or of a particular religious system. It is conceivable that a man might be reared in entire ignorance of women, but in such a case, though he knew not what he desired, he would yet crave for them, and his passive desires would instantly be stimulated into activity by their presence. So a savage, of a race not rendered resistant by Alcoholic Selection, craves unknowingly for alcohol, for that state of mind which alcohol induces, and his passive desires are quickly stimulated into activity by indulgence in it.

It is true, as regards man, in whose mental nature acquired traits play so large a part, that example and precept may do much to counteract instinct. Thus, for example, religious enthusiasm, a purely acquired trait, may so counteract the dictates of sexual instinct as to cause men and women to become monks and nuns, or so counteract the instinctive love of life as to cause them to throw themselves beneath the wheels of Juggernath or mount the funeral pyre; thus also love of country, obviously another acquired trait, often impels men to difficult and dangerous enterprises, from which all their instincts call them to abstain; thus also religious enthusiasm has, more or less, banished alcohol from Mahomedan countries. When, however, an instinct is not opposed by counteracting acquired traits, the part played by it is of course generally proportionate to its strength, and we know of nothing, and it is inconceivable that there is anything, in the mental traits acquired by the Greeks and Italians, for example, that would counteract the craving for alcohol, were it as strong in them as it is in savages, or even in some North

Europeans. Indeed it is probable in England, for example, where so much misery is caused by alcohol, where public opinion is so much against excessive indulgence in it, and where the cost of indulgence is ruinously high, that the traits acquired by the inhabitants are such as tend much more to counteract alcoholic craving than the traits acquired by the inhabitants of Italy and Greece, where little misery is caused, where public opinion is much less powerful, and where indulgence is much less costly. We must conclude, therefore, that the inhabitants of Southern Europe are more abstemious than those of Northern Europe, *à fortiori*, than savages, not because the craving for alcohol is in them more counteracted by the influence of precept and example, or because it is more stimulated by precept and example among the North Europeans and savages, but solely because among them the craving is weaker, or practically non-existent.

It is said that racial differences with respect to the craving for alcohol result from differences in the strength, &c. of the alcoholic beverages in common use in various countries; for instance, it is said that the English are more drunken than the Italians, because the spirits and fortified wines they drink are stronger than the light "natural" wines drunk in Italy; but this hypothesis is evidently founded on a confusion of cause and effect. It is evidently *not* true that the English are more inclined to drunkenness than the Italians, because their alcohol is less dilute; but, on the other hand, it *is* evidently true that because the English are more inclined to drunkenness than the Italians they prefer their alcohol of greater strength. The British Army of invasion during the Peninsular War was exceedingly drunken on the wines of the country, in the presence of which the Spanish Army remained abstemious; on the other hand, various savage races, such as the natives of Guiana, with their cassava intoxicant, and the Africans, with their pombé beer, who are only able to manufacture alcohol so dilute that it is difficult to drink an injurious quantity of it, crave ardently for a more concentrated beverage wherewith to gratify their desire for deep intoxication. If the above topsy-turvy theory were correct such races should be more abstemious than the Italians, while savages, such as the North American Indians, who have never been able to manufacture alcohol, should be more abstemious still, whereas the contrary is the case. It cannot be true, therefore, that the degree of concentration in which any race uses alcohol in any way determines the strength of its craving for drunkenness, except in so far as results from the survival of the fittest; though, on the other hand, it is certainly true that the strength of the craving for drunkenness determines in great measure, when there is a choice, the degree of concentration of the alcoholic beverages used. It is quite possible, indeed quite easy, for the most resistant individual, the most seasoned toper, to drink "natural," *i.e.* unfortified wine, like that used by the South Europeans, to such an extent as to produce the deepest intoxication; that is, to such an extent as to place him, from loss of health, &c., at a great disadvantage in the struggle for existence; and therefore natural wines are quite strong enough to be a source of considerable elimination, and therefore of evolution. In fact, though these natural wines do not possess the immediately poisonous properties of much stronger solutions of alcohol, yet, since alcohol is rarely, very rarely, drunk at any place of such strength as to be immediately poisonous, and since the deepest intoxication may be produced by imbibing them, they are practically speaking as efficient causes of elimination and therefore of evolution as the strongest spirits.

The degree of intoxication desired by the average individuals of any race, when once that desire has been awakened in them by the use of alcohol, is exactly in inverse proportion to the past familiarity of the race with the poison; in other words, it is exactly in inverse proportion to the amount of elimination alcohol has caused in the ancestry. But however extended the racial experience of alcohol, the desire for intoxication is never entirely eradicated, for, since the smallest amount of alcohol must produce some mental change, however small, and since smaller or larger quantities of it are imbibed by all races with pleasure, it is clear that all races, from the South Europeans to the Red Indians, drink it with more or less conscious desire of producing some degree of intoxication. The difference between races lies in the degree of intoxication desired. The South Europeans, who have had a long and disastrous experience of the poison, on the average desire intoxication to such a degree only as practically speaking leaves them unaffected for the struggle for existence in its presence. The North Europeans, who have had a less extended and disastrous experience of it, on the average desire intoxication to such a degree that in the presence of it many of them are injuriously affected for the struggle. But savages, who have had little or no experience of it, on the average desire intoxication to such a degree that in the presence of an abundant supply of alcohol the whole of them, practically speaking, are rendered unfit. In all this alcohol very closely resembles such a disease as malaria, which is so prevalent in its areas of distribution, that, practically speaking, every individual within them is liable to infection, and which least affects individuals of races that have had long and disastrous experience of it, more individuals of races that have had less experience of it, and most individuals of races that have had little or no experience of it. In the presence of very virulent malaria few—comparatively few—West Africans perish, but more natives of India, whereas comparatively few Englishmen survive, even when placed under the most favourable conditions. In the presence of abundant alcohol few—very few—South Europeans perish, more North Europeans, but few savages, of such races as have had no experience of the poison, survive. Such savage races, when introduced to concentrated modern forms of alcohol, are in as evil case as when introduced to tuberculosis under modern conditions—they cannot then accomplish in a few years, and a generation or two, that amount of evolution which other races, under stringent Alcoholic Selection, accomplished only after the lapse of scores of generations and thousands of years; and therefore they suffer extinction.

To sum up and complete the foregoing, it is plain (1) that races differ from one another in respect to the craving for alcohol as a result of a process of evolution due to Alcoholic Selection; (2) that the direction of this process has been towards a lesser craving from a greater craving; and (3) that in some cases—*e. g.* among the English—the process has been very rapid, since, side by side with individuals who have but a little craving for alcohol, are found others with a very great craving, and since it frequently happens that parents who crave but little for the poison have offspring that crave very greatly for it, *i. e.* offspring who have reverted to the ancestral type, in which the craving was very great, the greatness of reversion being due to the swiftness of the evolution, owing to which reversion to a not very remote ancestor results in a considerable change of type.

CHAPTER III

VERY important considerations now demand our attention. The whole organic world has arisen through the action of Natural Selection, under which general heading should properly be included the various special forms of selection—sexual, artificial, disease, alcoholic, &c. But whenever the stringency of selection is abated, every type, whether plant or animal, throughout the whole organic world, constantly displays a tendency towards retrogression, the strength of which is strictly proportionate to the rapidity of the previous evolution. Now we have no reason to suppose that races that have undergone evolution through the operation of Alcoholic Selection are less liable to undergo retrogression when the stringency of that selection is abated, than are races or types which have undergone evolution under any other form of Natural Selection. On the contrary, we have every reason to suppose, since inborn traits alone are transmissible, that when the stringency of Alcoholic Selection is abated, when the unfittest survive and have offspring as well as the fittest, when the innately intemperate have as much influence on posterity as the innately temperate, alcoholic retrogression will ensue, and therefore that a race that has undergone Alcoholic Evolution will, under such circumstances, undergo Alcoholic Retrogression, and revert to the ancestral type in which the craving was greater. It follows, then, if by any means we cause relaxation of the stringency of Alcoholic Selection, *e. g.* by diminishing or abolishing the supply of alcohol, whereby the innately drunken are permitted to survive and have offspring equally with the innately sober, that the craving for alcohol will grow in generation after generation, till the race reverts to that ancestral condition in which the craving was as strong as it is at the present day among the North American Indians or the native Australians, who have undergone no evolution in relation to alcohol; in other words, the success of every scheme for the promotion of temperance, which depends on the diminution or extinction of the alcoholic supply, or on voluntary or involuntary abstinence from alcohol, Total Abstinence, Local Veto, the Gothenburg System, &c., will—must—result in an aggravation of the craving for that state of mind which indulgence in alcohol induces; a craving which, in each stage, will be proportionate to the degree of retrogression undergone by the race, both in respect to the strength of the craving, and in respect to the depth of the intoxication desired; and therefore it follows, that all such schemes for the promotion of temperance are in effect nothing other than schemes for the promotion of drunkenness, or at any rate for the promotion of the craving for it.

It cannot be too strongly insisted on, or too often reiterated, that the craving for alcohol, like sexual love, is an instinct, not an acquired trait. It is called into activity, but is not created by appropriate stimulation, by experience of that state of mind which indulgence in alcohol induces. In this it differs radically from such an acquired passion as that, for instance, for a particular religious system, which is entirely created, not merely called into activity by stimulation, and for which may be substituted aversion or a passion for some other religious system; that is, which may be destroyed by opposing acquired traits. The passion for alcohol cannot be destroyed by opposing acquired traits, though it may be counteracted by them, especially when it is weak, as among the Italians, or when the counteracting traits are very strong, as among ardent temperance reformers. When it is strong, and the counteracting traits are weak, as among the North American Indians, and when, moreover, an abundant supply of alcohol is accessible, the race is doomed to disaster, perhaps to extinction. Thus, also, the sexual instinct is not destroyed, but is merely counteracted by the mental traits which monks and nuns acquire. The craving for alcohol, age after age, is born anew with each generation, undiminished, except by Alcoholic Selection, and in a race which has undergone Alcoholic Evolution, is increased age after age, and generation after generation, in the absence of such selection. But acquired passions change from generation to generation and from age to age, especially when such passions involve a limitation, not an extension, of instinct. Who then can doubt that when a changing acquired passion is opposed to an unchanging instinct, in the end the instinct will be triumphant? Who can doubt that when an inborn craving for alcohol is opposed to an acquired detestation of it, that in the long run the inborn craving will prevail, and therefore that if here in England we succeed at first in an attempt to counteract the inborn craving by moral influences, by acquired mental traits, that in the end the inborn craving will prevail, especially in such a race as ours, which has undergone some alcoholic evolution, and is therefore prone to undergo alcoholic retrogression when the stringency of selection is abated?

It will certainly be argued in reply, that since moral influences have caused Mahomedan races to abstain from alcohol for twelve hundred years, and for all we know will continue to do so for an indefinite time longer, there is no reason why moral influences should not cause Anglo-Saxons and other races equally to abstain. But the particular moral influence which has caused total abstinence among Mahomedans, religious enthusiasm, the strongest of all such influences, since it appeals to self-interest by promising rewards and punishments of enormous magnitude, cannot bring about the same result among us, for the simple reason that the Christian religion does not enjoin abstinence from alcohol, and there is no prospect of our substituting for it another religion which does. Again, Mahomedans are total abstainers only at the cost of being barbarians also; the same influence which has banished alcohol from among them has banished much else besides, and has in great measure cut them off from intercourse with other peoples. But Christians use alcohol in many other ways besides drinking it; without serious injury to their arts and manufactures they could not banish it from their midst; and their increasing civilization causes intercourse among themselves, and with other peoples, to become more free and extensive every year —that is, brings into more intimate relations peoples such as the Italians, whom the drug now injures very little, and who therefore have little reason to adopt a policy of total abstinence, and peoples such as the English, whom the drug injures very much, and who therefore might on the score of self-interest be persuaded to adopt such a policy. With our higher civilization it is then impossible to banish alcohol from our midst as it has been banished from among the Mahomedans, and without such banishment it is tolerably certain that abstinence from the poison could not be secured; for it must be clearly understood that Mahomedans abstain from alcohol, not solely because religious enthusiasm forbids, but mainly because among them opportunity for indulgence is not afforded to individuals who crave for it. The sexual craving is not stronger than the craving for alcohol is in races that have not undergone Alcoholic Evolution, nor stronger presumably than the craving for alcohol would be among races that had undergone Alcoholic Retrogression; but never yet has even the fiercest fanaticism, in the presence of opportunities for indulgence, been able to do more than counteract the sexual craving to a very limited extent; to do more, that is, than forbid its indulgence except under certain regulations (*i. e.* among the married only), and even in this it has been but partially successful. The early Christians, for example, considered sexual indulgence abominable, and believed that abstinence from it was conducive to eternal bliss; yet their strong enthusiasm withheld but a limited section of the community, those with the strongest enthusiasm, the members of the male and female religious orders, from indulgence in it, and even among the latter immorality was not unknown.

Seeing then that it is practically impossible to banish alcohol from our midst, if only for the reason that the world grows more cosmopolitan every day, and races that have undergone great Alcoholic Evolution, and are therefore now little injured by the poison, would not consent to abandon its use; seeing also that, without absolute banishment, alcoholic indulgence would certainly ensue in spite of moral influences, we cannot rationally hope that moral influences will ever bring about total abstinence; and, *à fortiori*, since the craving for alcohol increases with indulgence, since the traits acquired through the use of alcohol form an extension, not a limitation, of the inborn craving for it, we cannot rationally hope that moral influence will ever result in temperance—*i. e.* in a *moderate* use of alcohol.

The above conclusion will, I fear, be very unpalatable to a large and earnest section of the community, and doubtless it will be said, especially by those who have not taken the trouble to read this work, that I am an advocate of intemperance, and probably—by a transition easy to a certain order of intellect—that I am intemperate myself. The problem must be faced, however; it is a great matter, and big with fate; a step in the wrong direction may result in untold evil. It *is* true that races differ vastly in various respects, among others in the intensity with which they crave for drunkenness; and this latter difference, like all other differences, *must* be due to a process of evolution in one direction or another; either from a condition in which the craving was great towards a condition in which it is less, or else from a condition in which the craving was little, or practically non-existent, towards a condition in which it is great. The popular belief is that the racial use of alcohol results in ever-increasing racial intemperance; but the fact certainly is that races which have had the most extended experience of the poison are the most temperate; whereas all primitive peoples who have had no experience of alcohol or any other narcotic, crave for it intensely, and if they have the opportunity, poison themselves with it. Clearly the direction of the evolution has been from a greater craving towards a lesser, and no probable cause for it can be imagined except Alcoholic Selection, except the survival of the fittest in relation to alcohol, *i. e.* the survival, in generation after generation, during which alcohol was manufactured in stronger and stronger, in more and more poisonous solutions, of those individuals who craved least for it, and the elimination of those who craved most for it.

It *is* true also as regards evolution in general, that, whenever the stringency of selection is relaxed, any race which has undergone evolution immediately begins to undergo continued retrogression; and therefore it *must* be true that any race which has undergone Alcoholic Evolution will, in the absence of Alcoholic Selection, undergo Alcoholic Retrogression; whence it follows, since it is impossible to banish alcohol, that any scheme for the furtherance of temperance, which is founded on abstinence from alcohol, whether enforced or voluntary, is doomed to failure and worse than failure. It is doomed to failure if founded on enforced abstinence, because even if a race, which has so far undergone Alcoholic Evolution that the majority of its individuals crave little for indulgence in the poison, consent to the passing of a law forbidding its use, yet, if such a law be passed, a time will surely come, in some future generation, after the race has undergone retrogression to such an extent that the majority becomes the minority, when the law will be repealed or fall into abeyance. It is doomed to failure if founded on voluntary abstinence, because in the presence of temptation and the lapse of generations, the opposing acquired traits are prone to undergo change, while the instinctive craving remains unchanged except as regards strength; and because the craving would eventually grow so ardent as a result of retrogression that, even if the opposing acquired traits underwent no change, no opposing traits would be sufficiently strong to counteract it.

Quite apart from the theories of evolution and retrogression, it is surely evident, since children inherit the traits of their parents, and since alcohol causes a large number of deaths, which occur almost exclusively among those individuals who crave greatly for indulgence in it, and among their offspring, that a race in which such deaths occur, must differ from what it would be if such deaths did not occur, and that this difference, since the naturally drunken do not to the same extent influence posterity, must be in the direction of a lesser craving.

If, then, racial abstinence from alcohol must have for ultimate result only excessive racial indulgence in the poison, how may we save posterity from the curse of intemperance? How cause our race ultimately to crave as little for it as the South Europeans? Most assuredly only by imitating the process of Alcoholic Selection, by eliminating those individuals among us who crave for alcohol to an excessive degree, at least in so far as to prevent them influencing posterity by leaving offspring, who, by marrying the children of the naturally sober, would contaminate the whole race. In other words, we must either permit Alcoholic Selection to run its cruel course, or we must assist it by Artificial Selection, and so prevent much of the misery, by weeding out the obviously unfit, either by forbidding marriage to drunken individuals, or in the married by preventing the procreation of children by separating the parents, or such other means as science may devise.

The above proposals, as well as the conclusions on which they are founded, will certainly be received on "moral grounds" with horror by a certain section of the community, and in a country, in some respects the foremost as regards sanitation in the world, where that monstrous crime, the repeal of the Contagious Diseases Act, was possible, whereby, notwithstanding our endeavours to banish all other zymotic diseases, notwithstanding the great care with which we isolate individuals suffering from small-pox, diphtheria, scarlatina, &c., the venereal zymotic diseases were let loose on the community, and England made the plague-spot of Europe—of the world, as many unhappy and vanishing aboriginal races attest—it is certain that they will not at present be accepted. In the future, when the reign of prejudice has grown weaker, and our legislation more rational, I hope and expect it will be different; I hope and expect that individuals, weak against alcohol, suffering from what in the unavoidable presence of alcohol is to all intents and purposes a mental disease, will not be permitted to contaminate the race by bearing offspring, any more than individuals suffering from other grave forms of mental disease are permitted at the present time to do so; but that they only will be permitted to continue the race who are innately sober, and crave little for excessive indulgence in alcohol. If it is morally right to prevent imbeciles and lunatics reproducing themselves, it is surely right, from a moral point of view, to adopt similar measures against inebriates. We have ceased to walk in processions when threatened with pestilence, and resort only to cleanliness and sanitation; presently in our endeavours to procure sobriety we shall cease to resort to enforced or voluntary abstinence—*i. e.* we shall cease to oppose changing moral influences to an unchanging instinct, and we shall recognize that the will of man, even in the most limited sense, is not entirely free, but, as Mr. Herbert Spencer has truly said, is trammelled by his desires, and that on the average his actions are regulated by the strength of those desires. When we have recognized this, we shall recognize also that it is vain, especially in a retrogressing species, to expect that permanent racial sobriety will result notwithstanding the survival of the unfittest—*i. e.* notwithstanding that the unfittest are permitted to have as much influence on posterity as the fittest.

It may be argued, Why, if we seek to banish the microbes which cause zymotic disease, should we not seek to banish alcohol, which causes what is practically the death-dealing disease of alcoholism? If it is right to banish the one, it is surely right to banish the other. On the other hand, if the only way to secure permanent exemption from the evils of alcoholism is by the survival of the fittest, and the elimination of the unfittest—at least, in so far as to prevent the latter influencing posterity—it is surely only logical to suppose that the way to secure exemption from zymotic disease, is by the survival of the fittest, and the elimination of the unfittest against the latter?

But there is this essential difference between disease and alcoholism: no man craves for disease, whereas many men crave, knowingly or unknowingly, for excessive indulgence in alcohol. We may reasonably hope to banish the microbes of disease; and, if we achieve their banishment, it will not matter how far the race retrogresses in relation to them, for no man will endeavour to bring them back. But in the face of the craving for alcohol, we cannot reasonably hope to banish that poison permanently; and if we succeed in banishing it for a time, the craving for it (or other narcotics), ever growing in consequence of retrogression, will assuredly result in some future generation in its reintroduction, when the last state of the race will be worse than the first; and, owing to unchecked atavism, the more complete and lengthened its banishment, the worse will be the state of affairs consequent on its reintroduction.

Moreover, there is no reason why we should not employ against disease, especially against such a disease as tuberculosis, which we can scarcely hope to banish, the same measures it is here proposed to employ against alcohol; there is no valid reason, for instance, why we should not forbid the marriage of persons weak against tuberculosis, or, at least, why we should not forbid the procreation of children by them, but, having regard to the happiness of posterity, every reason why we should. Such a proposal, also, will doubtless be objected to on "moral grounds"; but of this portion of the popular moral code, founded as it is on ignorance and prejudice, productive as it is of much avoidable pain and misery, emblematical as it is of the imperfection of our civilization, the depth of our barbarism, it is difficult to find words sufficiently expressive of contempt and abhorrence.

CHAPTER IV

Opium and other narcotics.—After having devoted so much space to alcohol, it is not necessary to dwell at length on the other narcotics, the use of which has resulted in mental evolution; that is, which have been sufficiently poisonous and prevalent to have caused a considerable elimination of the unfit in relation to them, in this resembling prevalent and deadly diseases—*e. g.* malaria and tuberculosis,—and differing from diseases which though deadly are not prevalent—*e. g.* rabies,—and diseases which though prevalent are not deadly—*e. g.* chicken-pox; which last diseases, in the absence of any appreciable evolution caused by them, are resembled by such prevalent but comparatively innocuous narcotics as tobacco. The evolution against the other deadly and prevalent narcotics must have proceeded on much the same lines as that against alcohol, and against deadly and prevalent forms of zymotic disease. The gradual improvements in the manufacture of such narcotics—*i. e.* the gradual increase in the poisonous nature of the preparations of them manufactured by races that had discovered their use—must have been accompanied, as in the case of alcohol, by a protective evolution whereby, through survival of the fittest, the craving grew less. Doubtless, however, since the manufacture of strong—*i. e.* poisonous—preparations of some of them is much easier, and less complicated than the manufacture of strong preparations of alcohol, when once their use became prevalent, evolution against them was much more rapid than was the evolution against alcohol, that is, in the case of such easily manufactured poisons, Narcotic Selection was doubtless more severe in the beginning than in the case of alcohol, and therefore evolution against them must have been more rapid.

When subjected to the assaults of some zymotic diseases—*e. g.* measles and chicken-pox—the average individual, even of a race that has had no experience of them, is able to vary in such a manner that with comparative ease he becomes highly resistant to them, *i. e.* he acquires immunity against them. But when subjected to the assaults of certain other diseases—*e. g.* tuberculosis, leprosy, and malaria—the average individual, even of a race that has long been familiar with them, is not able appreciably to vary in such a manner as to render him more resistant, no matter how often or how long he be subjected to their assaults. Racial differences in relation to such diseases as these latter as a consequence depend solely on inborn powers of making resistance, not on inborn powers of acquiring capabilities of making resistance. Between diseases against which powers of resistance may be easily acquired by the individual, and diseases against which powers of resistance cannot in the least be acquired, lie all the other zymotic diseases, scarlatina, typhoid, diphtheria, small-pox, typhus, &c.; some of which resemble chicken-pox in the ease in which the individual is able to undergo protective reaction against them, and others tuberculosis in the difficulty or impossibility of any such protective reaction. The worst, the most death-dealing of diseases are those against which little or no protective reaction can be undergone by the individual—*e. g.* tuberculosis; and against which, therefore, the race is protected only by the inborn power of making immediate resistance.

In all this there is a close analogy between narcotics and diseases. Against some narcotics—*e. g.* tobacco—the individual, even of a race that is strange to them, easily acquires immunity; that is, the cells concerned (nerve-cells) not only acquire the power of tolerating the poison (toxin), but the craving for it is satisfied in the individual by a limited and generally harmless degree of indulgence; and therefore since such narcotics are the cause of little or no elimination, they are the cause of little or no evolution; and races that have longest experienced them, crave for indulgence in them as greatly as races that have had little or no experience of them. On the other hand, there are other narcotics —*e.g.* alcohol—against which powers of making resistance cannot be acquired to nearly so great an extent. For instance, no man can tolerate alcohol to nearly such an increased extent as he can nicotine, as a result of individual experience of it; that is, no amount of indulgence can reproduce in the experienced smoker those immediately poisonous effects of nicotine which his first indulgence in tobacco produced, whereas a slight increase of indulgence will reproduce in the drinker those immediately poisonous effects of alcohol which he experienced when first he indulged in the poison. The power of making resistance to alcohol, unlike the power of making resistance to nicotine, is therefore mostly of the inborn kind. The individual craving for it in races not rendered resistant by evolution in the ancestry is not satisfied by a degree of indulgence so limited that it is generally harmless, but only by a degree of indulgence which is exceedingly harmful; and therefore, since such narcotics are the cause of much elimination, they are the cause of much evolution; whence it follows, that races that have longest experienced them crave very much less for them than races that have had little or no experience of them. Between narcotics in relation to which the individual is able with ease to vary greatly—*e. g.* tobacco—and narcotics in relation to which the individual is able to vary comparatively little —*e. g.* alcohol—lie the other narcotics, some of which resemble tobacco in the ease with which the individual is able to undergo protective reaction against them, and others alcohol in the absence of the power of making, to any great extent, such protective reaction. As in the case of diseases, the worst, the most death-dealing, of narcotics are those against which the race is protected almost solely by the inborn power of making immediate resistance; a power which arises in the race only through a very tedious process of evolution, the result of a prolonged and stringent process of selection.

Except as regards alcohol and opium, we have little or no evidence to assist us in forming an estimate of the extent of the evolution caused in various races by the use of deadly narcotics. We can estimate its extent in different cases only by comparing races which have been long and disastrously familiar with this or that narcotic, with races which have had little or no experience of them; and save in the case of the two narcotics named, I do not know that anything has been published in which such comparisons are instituted—possibly for the reason that no races previously unfamiliar with them have recently acquired the habit of using them. I confess, however, that I have not very diligently sought evidence bearing on the matter, being convinced that it is a foregone conclusion, that wherever any narcotic has for a lengthened period been the cause of a considerable elimination of the unfit in relation to it, it has also, like virulent zymotic disease, like alcohol, like opium (as we shall see), been the cause of a considerable evolution protective against itself.

In the effects produced by it, both on the individual and on the race, opium appears to lie midway between tobacco and alcohol. As in the case of nicotine, experience of it by the individual induces a great protective reaction—enables the individual to tolerate immensely larger doses of the poison—but this protective reaction, this power of acquiring immunity, is not so complete in the case of opium as it is in the case of nicotine, since increased indulgence in the former may reproduce even in the most experienced individual those symptoms of immediate poisoning which indulgence in it first produced. At the least this is true; that while habitual indulgence in tobacco tends to cause, even in individuals of a race that has had no experience of it, a protective reaction, in consequence of which the individual no longer craves for indulgence in it to such an extent as to reproduce the symptoms of immediate poisoning which he felt when first he indulged in the poison, habitual indulgence in opium by individuals of a race that has had no experience of it, very rarely results in a protective reaction of such a nature that the individual no longer craves for indulgence in it to such an extent as to reproduce those symptoms of immediate poisoning which he felt when first he made the acquaintance of the narcotic. On the contrary, such an individual, notwithstanding the protective reaction he undergoes, whereby he is enabled to withstand increased doses, yet generally craves for indulgence to such an extent as to reproduce the symptoms of immediate poisoning; and therefore—as in the case of alcohol—indulgence in opium to the full extent of his desire tends to bring about his elimination; whence it follows that opium, like alcohol and unlike tobacco, is a cause of evolution—an evolution which is mainly from a greater craving towards a lesser craving, not mainly from a lesser power of toleration towards a greater power of toleration; for in the latter case the individuals of one generation would as effectually poison themselves with larger doses, as did the individuals of preceding generations with smaller doses.

In fact, evolution against alcohol and opium tends to place the race in the same position as regards them as it already holds in relation to nicotine; that is, it tends to bring about such a diminution of the depth of the intoxication desired that the individual is no longer placed at a disadvantage in the struggle for existence by reason of his craving; but in the case of opium, the narcotic in relation to which the individual is himself able to undergo a great protective reaction, this evolution appears much easier of achievement than in the case of alcohol, the narcotic against which the individual is able to undergo comparatively little protective reaction; for while the evolution against alcohol, as we have seen, is only perfected after the lapse of many centuries of stringent Alcoholic Selection, the evolution against opium appears to be perfected, as we shall see, in a much shorter space of time—in quite a few centuries. It appears, therefore, that in the case of opium, the narcotic which resembles nicotine, in that it is a toxin, and in that the individual reaction to it is great, the race with comparative ease achieves a position in relation to it similar to that which it holds in relation to nicotine; but in the case of alcohol, the narcotic which is dissimilar from nicotine in that it is a waste product, and in that the individual reaction to it is comparatively little, the race achieves a like position in relation to it only with great difficulty and after many centuries of selection. In fact, efficient protective evolution against opium appears to result from a much smaller divergence from the susceptible ancestral type than does efficient protective evolution against alcohol.

It has already been remarked that, after having considered alcohol at length, it is not necessary to devote much space to the other narcotics, opium among the rest. The two processes of evolution—*i. e.* against alcohol and opium—must have proceeded on very similar lines, except in so far that immunity against opium was probably much more easily and quickly achieved than immunity against alcohol, for the reasons that a much smaller divergence from the ancestral type apparently suffices to protect against the latter, and because poisonous preparations of opium are much more easily manufactured than like preparations of alcohol—as is natural since opium is a "toxin," a substance that protects the organism producing it from other organisms to which it is liable to become a prey. It must therefore have been from the time it was first used as an intoxicant, the cause of a much severer process of selection than was alcohol when first used.

The Greeks of the time of Hippocrates were acquainted with the medicinal use of opium,[1] and early imparted their knowledge to the Arabs,[2] who in turn introduced the poppy with the knowledge of its medicinal properties to India and China,[3] to the latter country later than to the former, but yet as early as the eighth century. But not till very long afterwards, not till five hundred years or more had elapsed, was the unfortunate discovery made that it might be used like alcohol to produce pleasurable sensations. For some hundreds of years its use as a narcotic has been prevalent in certain parts of India, whence at the beginning of the last century the Chinese acquired the knowledge and the habit of using it, as a consequence of which the famous import trade with India sprang up. The Burmese have had far less experience of the drug, indeed they have used it extensively only within the memory of living

[1] *First Report, Royal Commission on Opium*, p. 147.
[2] *Ibid.* p. 147. [3] *Ibid.* p. 148.

man. If, then, opium has been a cause of evolution, certain peoples of India—*e. g.* the Sikhs and Rajputs—who have longest used it should be the most resistant to it—that is, should crave least for excessive indulgence in it; the Chinese should be less resistant, should crave more for it: whereas the Burmese should be least resistant, should crave most for it.

This is exactly what we find to be the actual case. Numerous witnesses, men scientifically trained, who had had the best possible opportunities for observation, declared before the late "Royal Commission on Opium" that they had never or rarely known opium productive of harm among the peoples of India. On the other hand, numerous witnesses, chiefly missionaries or others connected with different religious bodies, asserted that everywhere in India it was productive of great harm. But as regards this conflict of evidence, I do not think that I overstate the case when I say, that in a question of this sort the evidence of one expert should outweigh that of a dozen enthusiasts, especially when to the cause for which the latter are contending they apply the word "sacred," and I am encouraged in this view when I remember how strangely discrepant may be two versions of the same event given by different and opposed bodies of enthusiasts; for instance, the narrative of this or that event in Central Africa as severally related by Protestant and Catholic missionaries when acting in opposition. Moreover, even by the missionaries, opium is said to be injurious chiefly from a "moral" point of view. It is said by them to mentally affect the natives of India and China much as alcohol is said by people of the same type to affect the natives of England.

"The moral effect on the heathen seems to be to rob them of all that little moral sense they seem naturally to have; and it turns them into thieves, liars, fornicators, and it seems to turn them into everything that is bad. I speak now especially of the heathen."—Rev. F. Brown, *First Report, Royal Commission on Opium*, p. 50.

But even if it be proved true that opium has the alleged disastrous effect on the morals of the heathen, yet, since the possession of a high moral tone does not appreciably affect the survival rate, this aspect of the matter does not concern us here. It need only be remarked, that it is highly unlikely that opium, any more than alcohol, does directly produce such mental effects. Indirectly, through loss of independence, self-respect, &c., it certainly may do so.

On the other hand, some of the scientific witnesses seemed to have erred in the opposite extreme by attributing to opium a rôle altogether too innocent in India. It cannot be that it is entirely harmless there; for however resistant evolution may have rendered the mass of the people, there must occur among them some cases of retrogression in relation to opium, just as some cases of retrogression in relation to alcohol occur among the South Europeans—cases, that is, of arrested development, in which the individual in his ontogeny does not recapitulate the whole of the phylogeny, but halts at the stage a more or less remote ancestor reached. But the mere fact that so many highly-skilled observers, favoured with splendid opportunities, failed to meet with or observe cases of excessive indulgence, proves how great must be the evolution in relation to opium of the natives of India. The following extracts are, for convenience of reference, taken solely from the "First Report" of the Commission, but the succeeding Reports fully confirm the evidence given in it. Sir George Birdwood said—

"I wish here to speak only of my personal observation of the habitual use of opium during my fifteen years' latter residence in Western India. I paid the closest attention to the subject during the whole of the years I was there, and had every kind of experience in relation to it, having at different periods been in medical charge of the Southern Mahratta Irregular Horse, the 8th Madras Cavalry, the 3rd Bombay Native Infantry, a battery of Artillery, the jail and civil station of Sholapore, and the steam frigate *Ajdaha*. . . . Subsequently, and for the remainder of my service, I was attached to the Jamsetjee Jejeebhoy Hospital, Bombay, and was in succession Professor of Anatomy and Physiology, and of Botany and *Materia Medica*, at Grant Medical College. I was also a J.P., and a visitor of the jails in Bombay, and the year I was sheriff I regularly visited them. Besides this, I was probably more intimately familiar with all classes of the native population than any other European of my generation; while, as an ever-active journalist (I was a journalist from the first day to the last of my service in India), I was mixed up in almost every discussion of this sort during my time in Bombay. Well, in all the experience—as here precisely detailed, and capable therefore of being checked at every point—I thus had of the indigenous life of Western India, I never once met with a single native suffering, or who had ever suffered, from what is called the excessive use, or from the habitual use of opium; and, except cases of accidental or wilful poisoning by opium, I never knew of a single instance of death from its use; and I have never met with any one who, in his own personal experience, has known a case of death, or of injury to health, from the habitual use of opium as practised by the people of any part of India proper. I exclude Burmah; I know nothing of it. . . . So far as I can remember, in the printed tables used in Indian civil and military hospitals for the entry of diseases, there is no column for the 'opium habit,' nor for 'deaths from opium.' On the strength of my personal experience, I should be prepared to defy any one to bring forward, from their personal experience, a single authentic record of death, or shortened life, from habitual opium eating or drinking in India. If any one can, let him, and the means of verifying his or her statement are always, within the current generation, accessible in India."—*Ibid.* pp. 77-8.

Sir Joseph Fayrer wrote—

"There cannot be a doubt that, in the great cities of India, in China, and probably elsewhere in the East, the abuse of opium is carried by a certain but a limited number to a great extent, but to nothing like the extent to which the abuse of alcohol"—*i.e.* by the English—"is carried. It is well known that over large areas of country in India, by tens of thousands of people, opium in moderation is habitually used by the natives. . . . It is said, I believe, by its opponents, that the tendency of opium-eating is ever to increase—to induce, it may be, slow but sure degradation and destruction. I do not believe this. In the course of many years' experience in India I have known so many who have been habitual consumers of a small quantity of opium without in any way suffering from it, or without any tendency to increase the habit, that I am unable to agree with those who state otherwise."—*Ibid.* p. 111.

In answer to the question—

"The general effect of your evidence has been that, from your wide opportunities of observation in India, you have not seen that what is called the opium habit has produced widespread and grave moral evils among the population of India?"—*Ibid.* p. 108.

Sir Lepel Griffin replied—

"No, I do not think there is a single resident in India who knows anything on the subject who would possibly say so. I do not think I am singular in my opinion."—*Ibid.* p. 108.

Sir William Moore said—

"Well, I came to the conclusion that opium-smoking was practically harmless, and that drinking umal pawnee, or opium-water, was practically harmless. . . . I should wish to draw attention to the fact that insurance societies do not impose a higher rate on opium-eaters. With respect to that they are guided by the medical officers of the societies. They were addressed on the subject some little time ago in Calcutta and Bombay, and they all gave the same answer."—*Ibid.* pp. 71-2.

To the question—

"Then you would compare the use of opium amongst these people to the use—the moderate use—of alcoholic liquors amongst ourselves?" Dr. Mouat replied—"Undoubtedly," and in answer to the further question, "And quite as harmless?" "Yes, quite, in fact more so, because a man shows a flushed face and many other indications of familiarity with alcohol, but you could detect nothing of the kind in the case of those who used opium. They were all temperate; I never saw, in the whole thirteen years I was living amongst them (and I saw them daily)—they came to me at the out-door dispensary, or at the hospital, and as a friend, and I never saw, in all that time, an opium drunkard."—*Ibid.* p. 75.

In answer to the question—

"Will you explain to the Commission what you yourself have observed as to the evils of the opium habit?" he said, "I never saw any one who exhibited such an amount of misuse of opium, not one in the whole course of that time, so I cannot speak to it from personal knowledge." And again to the question, "You did not see any sufferers from opium?" he replied, "No, not one."—*Ibid.* p. 75.

It is clear then that those races of India which use opium are very highly resistant to it. As regards China, while competent witnesses frequently declared that the accounts given by missionaries of its evil effects are exaggerated, it is significant that none of them appear to have declared, as so many did of India, that opium-smoking is totally unattended by harm.

Sir Thomas Wade said—

"No man who has lived the time I have in China, and who has been in contact with Chinese of all kinds, can deny that the excessive use of opium in that country is an exceeding misfortune to that country, and I myself have stated that proposition, perhaps more positively years ago than I should be prepared to do at this moment. That is to say, that without at all pretending to abate the statement that many people—many thousands of people—do suffer from the excessive use of opium, it is to a great number of people precisely what the use of alcoholic stimulants to the people in our country, taken moderately, is; that is to say, that it will cheer the workman just as our workman is cheered by his glass of beer."—*Ibid.* p. 87.

In an article quoted before the Commission Dr. Ayres wrote—

"My opinion is, that it (opium-smoking) may become a habit, but that the habit is not necessarily an increasing one. Nine out of twelve men smoke a certain number of pipes a day, just as a tobacco-smoker would, or as a wine or beer-drinker might drink his two or three glasses a day, without desiring more. I think the excessive opium-smoker is in a greater minority than the excessive spirit-drinker or tobacco-smoker. In my experience, the habit does no physical harm in moderation. . . . I do not wish to defend the practice of opium-smoking, but in the face of the rash opinions and exaggerated statements in respect of this vice, it is only right to record that no China resident believes in the terrible frequency of the dull, sodden-witted, debilitated opium-smoker met with in print, nor have I found many Europeans who believe that they ever get the better of their opium-smoking compradores in matters of business."—*Ibid.* p. 139.

It is clear then that while most of the Chinese—at any rate the Chinese of the coast, who have been familiar with the poison for some two centuries—do not as a rule indulge in opium to excess, yet an appreciable number of them do take it in such amounts as to place themselves at a disadvantage in the struggle for existence. Probably in inland districts, to which opium has more recently penetrated, excessive indulgence is much more common.

Lastly, there is a consensus of opinion that indulgence in opium is extremely harmful to the Burmese, who have only recently acquired a knowledge of its use.

In reply to the question—

"Can you give us the reasons which, in your judgment, actuated the Burmese authorities and led to the decision to prohibit the use of opium?"

the Rev. J. S. Adams, a missionary, said—

"From conversations that I had repeatedly with Burmese elders, with the Governor of Bhamo, and with the Buddhist Archbishop, I learned that the principal reason was that the people themselves were so weak in the face of such temptations as those offered by opium and liquor, and also that the Buddhist law prohibited the use of intoxicants to the people of the Buddhist faith; and there were also ancient laws of the kingdom of Ava which forbade the same thing."—*Ibid.* p. 24.

And to the question—

"Can you give us your impressions as to the effect of the use of opium upon the people with whom you were brought into contact in Burmah, whether Chinese or the Burmese?"

he said—

"Of the four races I found in Upper Burmah, Kachyins, Shans, Burmans, and Chinese, the Burmese were the more decidedly susceptible to the influence of the drug undoubtedly. The Chinese seemed to resist the influence of it more than even the Hill-men, but in all cases where the habit was once formed the emaciation of the consumer and the deterioration of his moral character alike ensued."—*Ibid.* p. 24.

To the question—

"You have been speaking thus far of the regulations which were established by the native Government of Burmah with reference to the suppression of the opium habit. When the country passed under the rule of the Government of India, what was the policy then adopted with reference to opium?"

he replied—

"The Government of India made a very important proclamation in English, Burmese, and Chinese, to the effect that her Majesty the Queen-Empress would not receive a revenue from the sale of opium in Upper Burmah, and a law was put upon the Statute Book, making it penal to sell opium or any of its preparations to men of the Burmese race. At the same time, the possession of opium or liquor by Burmans was not an offence, but it has been made so during the last year, I believe."—*Ibid.* p. 25.

He also quoted the following from a letter written in 1892 by Dr. A. T. Rose, an American missionary—

"You must not write our mission indifferent to the opium question; it has been connected with it from the days of Judson and Wade. Thirty years ago I was appointed to write a report on the introduction, increase, and effects of opium in Burmah by the 'British Burmah Missionary Convention.' The elder Hough, Wade, Bennett, and Kincaird were then living on the field. They all affirmed that there was no opium in Burmah before the English came. We laboured with Sir Arthur Phayre, who professed to believe that the Government must introduce opium in order to control and regulate it, otherwise the country would be flooded with it. As a revenue measure, the introduction of opium is an enormous blunder, for it blasts the vital sources of the revenue, it converts honest labourers into idle thieves and vagabonds. If all the cultivators in Burmah were to take to growing opium, in five years there would not be a basket of rice. I have never known a Burman or Karen to use it who did not go to the bad sharp."—*Ibid.* p. 26.

Sir John Strachy said—

"The only country—I cannot say of India, because it is not India, it is as unlike India as Algeria is unlike France—but the only country under Indian administration in regard to which it appears to me that any evidence has been produced that deserves serious consideration, to show that any considerable section of the people has suffered from the consumption of opium, is Burmah. Now it is indisputable that there has been a great body of opinion as to the injurious effect of opium on the Burmese. Two chief Commissioners, Sir Charles Aitchison and Sir Alexander Mackenzie, both of them men who are entitled to speak on the subject with the highest authority, have concurred in that opinion, and there is no doubt that the same opinion has been very generally held by the majority of the British officers employed in Burmah. Also it seems to have been an admitted fact, that those views are in accordance with those of the more intelligent classes of the Burmese themselves. 'Native opinion,' Sir Charles Aitchison wrote, 'is unanimous in favour of stopping the supply of opium altogether, and no measure we could adopt would be so popular with all the respectable and law-abiding class of the population. In a matter so intimately affecting the well-being of the community,' he added, 'these expressions of opinion are entitled to the greatest respect. When practical questions of this kind arise, it may become a duty to yield to the strong and general desire of the people, even when their opinions may appear unreasonable.' Now although I have myself, I must say, failed to discover the facts upon which this belief in the injurious effects of opium on the Burmese population rests, I cannot deny that it was right to yield to this general consensus of opinion on the part of both the Burmese themselves and of the English officers most competent to form an accurate judgment, and to take measures for preventing the sale of opium to Burmese, and their possession of the drug, and this has been actually done throughout the whole of Burmah. In regard to this question of the consumption of opium by the Burmese, it is, as Mr. Batten says, remarkable that the authorities in Burmah seem to have arrived at the conclusion that opium is a benefit to every one in the country except the Burmese themselves. I should like to add, that while there has been this unanimity of opinion in regard to the mischievous results of opium on the Burmese, there has been an equal unanimity in regard to the harmlessness of the practice among the large foreign population, Chinese and Indian, of Burmah. Sir Charles Aitchison writes—'There are large numbers of the non-Burmese community, constituting perhaps the most thriving and industrious section of the population, to whom the drug is a necessary of life, and by whom it is rarely abused. It is impossible to say precisely what the numbers of the Chinese and the natives of India are, but they are probably not less than 200,000. The legitimate requirements of these people must necessarily be considered and provided for.' Sir Alexander Mackenzie's views on the point were the same. He objected to any interference with the supply of opium to the non-Burmese population. 'There is,' he said, 'a considerable non-Burmese population of Shans, Chinese, and others who are accustomed to the moderate use of opium, and who consume it without ill effects or with beneficial results.' The Chief Commissioner 'is not prepared to advise the absolute prohibition of the possession or sale of opium in Burmah by persons of non-Burman descent. Such a step would be an unjustifiable interference with the habits of a large section of the population, and would be quite impossible to enforce. It may be considered as established beyond question, that there is a legitimate demand for opium among the foreign residents of Burmah, which would exist whether the Government countenanced the use of opium or not; that if Government decided to declare the sale or possession of opium generally illegal, the demand would be supplied by illicit means, and that the result of any attempt to enforce absolute prohibition of the use of opium would be the loss of a large amount of revenue without any commensurate benefit.' Although, as I said before, I cannot say that I am satisfied that while opium is harmless or beneficial to the Chinese and others, it is poisonous to the Burmese, still I cannot dispute the authority by which that opinion is supported, and if it be correct, I know of only one suggestion by which it can be explained. I believe there is no race of men among whom the demand for one form of stimulant or another does not exist, and it has been held by some—perhaps correctly—that while particular stimulants are harmless or beneficial to some races, they are injurious to others. It is possible that opium, taken even in moderation, may be injurious or a dangerous temptation to a Burmese, although it may be innocent or beneficial to Chinamen or Sikhs, and, as many have maintained is the case, alcohol, taken even in moderation, may be bad for the people of Southern Asia, whilst, similarly taken, it may be good for Europeans. However this may be, Burmah is not India, and it is not reasonable to apply to India conclusions based upon observations made in a totally different country. It appears to me as regards India, properly so called, there is no evidence whatever to show that in any part of the country the consumption of opium is anywhere a common and crying evil. Of course I admit that the use of opium may be abused, but I entirely disbelieve that this occurs to any general or dangerous extent."—*Ibid.* pp. 66-7.

À posteriori considerations therefore abundantly confirm the conclusion we came to on *à priori* grounds; namely, that wherever the death-dealing narcotic opium is in common use as an intoxicant, there it must be a cause of evolution also.

Evolution against any one zymotic disease is not protective to any extent against other zymotic diseases. It would be interesting to know whether the same law obtains as regards evolution against the various deadly narcotics: for instance, it would be interesting to know whether evolution against alcohol is, or is not, protective against opium. Are the weak against alcohol, they who crave greatly for it, the weak against opium also? Or is the one craving a thing distinct and separate from the other? It is very rare for both cravings to be manifested in the same person; indeed I do not remember to have heard of a case where this occurred to any extent; but, on the other hand, the cravings for alcohol and tobacco very often co-exist. It seems probable, therefore, that the cravings for those states of mind that alcohol and opium (and other narcotics) severally induce are distinct and separate, just as the weaknesses against separate zymotic diseases are distinct and separate, and therefore it seems probable that a man may be strong against alcohol, but weak against opium, or *vice versâ;* just as he may be strong against tuberculosis, but weak against malaria, or *vice versâ.* In which case the reason why men do not commonly indulge in both alcohol and opium must be, that when the craving for one is awakened it is so strong as to exclude, to put out of sight, so to speak, the craving for the other, especially when the latter has not been awakened, as the sexual instinct is awakened, by a particular known object. I take it that the mental effects, the mental paresis, produced by one strong narcotic is in some measure much the same as that induced by any other, just as the systemic effects, the systemic prostration, produced by the toxins of one virulent zymotic disease is, to some extent, the same as that induced by any other; and, therefore, that the craving for one powerful narcotic is in some measure satisfied by indulgence in any other.

But just as a man may be strong or weak against one zymotic disease at the same time that he is weak or strong against another, so he may be strong or weak against one narcotic at the same time that he is weak or strong against another. In other words, while one narcotic against which his race has undergone evolution may awaken in him only a feeble desire for intoxication,—*i. e.* a desire for a slight degree of intoxication only,—another narcotic of different chemical composition, against which his race has not undergone evolution, may awaken in him a much stronger desire—*i. e.* a desire for much deeper intoxication. It follows that a race which has undergone evolution against one powerful narcotic—*i. e.* a narcotic powerful enough to produce mental paresis even in those habituated to it—may find the act of indulgence in that narcotic a protection or preventative against indulgence in other powerful narcotics, against which it has not undergone evolution; from which it further follows, that if a race which has undergone evolution against any particular narcotic be debarred from the use of it, it may, by seeking to satisfy its craving by indulgence in a narcotic of which it has had little or no experience, be drawn into much greater excesses, and therefore be much more injured than it was by the narcotic of which it already had had a long and disastrous experience, and therefore against which it had undergone protective evolution. For instance, if the natives of India are debarred from the use of opium, or if Englishmen are debarred from the use of alcohol, it is possible, by substituting alcohol for opium in the one case, and opium for alcohol in the other, that each race will suffer much more than it otherwise would.

In any case the question is by no means so simple as it is thought to be by temperance reformers, with whom as to ends I am in the heartiest sympathy, but with whom as to means I am at issue.

In conclusion, it is surely clear that if the world is to become more temperate it must be by the elimination not of drink, but of the excessive drinker. If Artificial Selection be found impracticable in the future, as, owing to the state of public opinion, it undoubtedly is at present, then the only alternative is Natural Selection, in which case the world will never be thoroughly sober until it has first been thoroughly drunk.

THE END

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