

## THE PATHOLOGY OF CHRONIC ALCOHOLISM: A REPLY.

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IN January, 1904, I published an article in *The British Journal of Inebriety* in which I sought to prove that agencies from the external world, acting on or through parents, do not cause alterations in the inborn characters of offspring subsequently born. For example, I contended that alcohol or the toxins of disease, circulating in the parental fluids, do not make offspring degenerate. In the following number of the *Journal* appeared a paper in which Dr. Ford Robertson declared me extremely wrong. In October Dr. Harry Campbell replied. Dr. Robertson now (January, 1905) complains that Dr. Campbell has misunderstood him. I am sure he is right. As will be seen, I did not understand him myself, and I have grave doubts whether anyone else did very clearly. The meaning of his words is plain enough, and he stated a great deal that is undeniably true. The difficulty lies in ascertaining the connection between his premises and his conclusions, between his facts and the deductions which, apparently, he draws from them. He was rightly eulogistic of the embryological work of Dr. J. Beard, of whom he and I are admiring students. It is odd that Dr. Beard's discoveries, his ascertained facts, so far as they bear on the subject in hand, should confirm Dr. Campbell and me in our opinions, and Dr. Robertson in his opposition to them. This appeal to Dr. Beard carries us into rather abstruse regions of discussion—into a discussion which I think is totally irrelevant, but which Dr. Robertson forces us to enter if only to prove that it is irrelevant.

Omitting such forms of reproduction as budding, which do not concern us here, every living being arises, directly or indirectly, from a germ-cell (or a conjugated pair of germ-cells). Within

the germ-cell, in the nucleus, is a hypothetical substance which has been termed the "germ-plasm," and which is the "bearer of heredity." Some such substance there must be—some substance which is the bearer of heredity. Resident in the germ-plasm are certain tendencies which we may term the "hereditary tendencies," and which, under fit conditions, cause the germ-cell to proliferate into an individual—the offspring. These tendencies differ in different species of plants and animals; thus, those resident in the germ-plasm of a mouse differ from those resident in the germ-plasm of an elephant, and as a consequence the germ-cell of the one gives origin to a mouse, and that of the other to an elephant: Formerly it was supposed (*e.g.*, by Darwin and Herbert Spencer) that the germ-plasm was derived from the tissues of the individual. It was supposed that every tissue (body or somatic) cell sent out gemmules, or representatives, which migrated into the germ-cells, for which reason, as was thought, parents reproduced their like. This view, however, has long been abandoned by the majority of biologists. They now suppose that, when a germ-cell proliferates and gives rise to an individual, all its germ-plasm is not used up in the formation of his tissues, but that some of it is reserved, undergoes growth, and passes (otherwise little changed) into his germ-cells. His child, therefore, resembles him because they are both derived from portions of the same germ-plasm. In brief terms, the old view was that the child's hand (for example) resembled the parent's hand because it was derived from the latter; the modern view is that the child's hand has nothing to do with the parent's hand, but that the one resembles the other because they have a common origin. As Dr. Robertson says,\* "Offspring derive nothing from their parents but shelter and nutrition; they are not, in a biological sense, the products of the bodies of their parents"—that is, they are not the products of the soma. Beard, Robertson, Campbell, and I are all strenuous supporters of the modern view. The old view was formulated to explain the alleged transmission of "acquired" characters; but since there is no evidence that acquired characters are ever transmitted, and very abundant evidence that they are not, the justification for an obviously far-fetched hypothesis vanishes.

Weismann and other biologists have held that germ-plasm may travel from germ-cells to descendant germ-cells by way of

\* April, 1904, p. 241.

somatic cells. They suppose, in fact, that there is continuity of germ-plasm, but that the road by which the germ-plasm journeys may lie through somatic cells. Thus, since from a fragment of begonia-leaf may arise a plant which produces germ-cells, they suppose that the somatic cells of the leaf contain germ-plasm which has migrated into them. Beard, supported by Robertson,\* goes a step further: he denies that somatic cells ever contain germ-plasm. He alleges that the germ-plasm contained in the germ-cells of the parent always passes to the germ-cells of the child through a special line of non-somatic cells. I have not discussed the question; it seemed to me utterly irrelevant. From an embryological standpoint, as indicated by Campbell,† it may be interesting to know the particular path taken by the germ-plasm—to know that there is continuity of germ-cells as well as of germ-plasm. From the point of view of heredity, it matters not a straw; but Dr. Robertson, for some mysterious reason, thinks it does matter. He credits me with being a supporter of Weismann's view, though really I have no opinion on the matter. Hence, I suppose, the detailed account of Beard's observations, and the implication that I am extremely wrong; otherwise I cannot imagine why Beard's observations are dragged into the discussion. It is exactly as if I had stated that someone had journeyed from London to Edinburgh, and Dr. Robertson had retorted: "Reid is quite mistaken. Beard's observations prove that the man in question went by the Midland Railway."

In fit conditions of time and place and so forth, germ-cells proliferate, under the stimulus of nutrition, into individuals. The characters of the individual which thus arises are technically termed "inborn characters." But inborn characters may be altered subsequently; thus, a muscle which has ceased to grow under the influence of mere nutrition may increase in size under the *added* stimulus of use, or a character may be altered by injury. These alterations, caused by influences *added* to those which produce inborn characters, are technically termed "acquirements." Dr. Robertson declares‡ that "the distinction between inborn and acquired characters has really no justification in modern scientific fact," because "ears, eyes, and nose . . . likewise result from the action of the environment on the soma. The ten or

\* April, 1904, p. 237.

† October, 1904, p. 55.

‡ April, 1904, p. 242.

twelve stones of body-weight are not derived from the imponderable germ-cell; they come from the environment. . . ." So that, if we are to believe Dr. Robertson, there is no distinction "in modern scientific fact" between an eye which has not been injured and one which has been injured, or between a muscle which has not increased in size as a result of use and one which has so increased. It would seem that modern scientific fact is likely to be extremely confusing to old-fashioned men of science.

It is true, of course, that inborn characters are derived from the environment in so far that they are built up of nutritive material which is derived from it. In this sense the germ-cell and the germ-plasm itself are acquirements, and nothing is inborn. But even this novel discovery does not entitle Dr. Robertson to declare that there is no distinction between the two kinds of characters. He has simply committed the flagrant logical error of taking dissimilar things, finding a single point of similarity, and then declaring that there is no dissimilarity. He might just as well have said that, since cats and dogs have ears, the distinction between cats and dogs has no "justification in modern scientific fact." I may add that I did not invent the terms "inborn" and "acquired"; they are old and well-established scientific terms, which have been universally recognised as marking distinctions of vital importance. No one, to my knowledge, has misunderstood them in quite the same amazing way as Dr. Robertson, and really I should not have thought it possible.

Very similar are his objections\* to my use of the words "inheritance," "ancestry," and "offspring." These also are old-established technical terms with perfectly definite meanings—meanings which, when understood, enable us to avoid a vast amount of circumlocution. I protest, when I say, for example, that a child has inherited his parent's nose, I do not mean that the parent's nose has departed bodily from the parent's face to the child's face, leaving the parent derelict. I do not even mean that the child's nose is in any way derived from the parent's nose; I mean merely that the two noses are very similar—in fact, I mean exactly the same that Dr. Robertson and most other people who have given thought to this matter mean. The view of heredity advocated by him, and to which he thinks I am opposed, so far

\* April, 1904, p. 241.

from being new, as he implies, is more than twenty years old, and is one of which I have always been a strenuous adherent ; for, as I say, if the continuity of the germ-plasm be admitted, then, in this connection, whether there be or be not continuity of the germ-cells matters not an iota.

Leaving aside these irrelevant discussions, as well as several others, such as that concerning the correct definition of disease, let us turn to the real question at issue—the question whether influences from the environment, acting directly\* on the germ-plasm, commonly so alter its hereditary tendencies that variations are caused thereby in offspring ; for example, the question whether parental alcoholism or disease so affect offspring that they are born different from what they would otherwise have been born. Dr. Robertson insists that variations are caused in this manner, whereas I have insisted that there is very massive proof that they are not so caused.† Dr. Robertson founds his case on certain experiments and statistics ; I found mine on a general review of the facts of Nature. If I state my own case first, I shall be in a position to deal with Dr. Robertson's.

Probably the most striking fact in Nature is the minute adaptation of every species of living being to its environment. Whenever this adaptation fails the species perishes. Hardly less striking is the minute adaptation of every structure in every kind of animal and plant to its other structures. When this adaptation fails the individual perishes. Muscles, bones, ligaments, glands, etc., in the case of animals, and leaves, trunk, root, etc., in the case of plants, are all closely adapted to one another, and work in collaboration. Now, this minute adaptation, on which the persistence of life depends, must have arisen somehow. At the present day most educated people believe that it has resulted from evolution. They suppose that since the beginnings of life, since the first lowly organisms appeared on earth, the environment has undergone a slow alteration, with which the various living species have kept pace, and that to this is due their differentiation and evolution. Other people believe that plants and animals were created just as they are at the present day. A necessary

\* Direct, as opposed to the indirect influence of natural selection.

† This statement requires some qualification, for which I have not space (see "The Principles of Heredity," p. 46).

corollary to this latter hypothesis is a belief that the environment has not altered, for otherwise the various species would not now be in harmony with it. Now, all species are acted on by a number of powerful and persistent influences—heat or cold, drought or moisture, abundance or scarcity, health or disease, and so forth. If these influences cause variations the species cannot remain stable; it must drift in this or that direction. Therefore, if we suppose that species were spontaneously created in the forms they now present, we are logically driven to the belief that the environment cannot so act on the germ-plasm as to cause variations in offspring.

On the other hand, if, in common with the majority of educated men, we believe that the plant and animal worlds have arisen through evolution, we are just as much forced to the belief that the direct action of the environment cannot be a cause of variations. Suppose, for instance, an injurious influence acted on a race—for example, alcohol or malaria, from which on the West Coast of Africa hardly a human being escapes or has escaped for uncounted generations—then, if the germ-plasm were altered in its hereditary tendencies, the race would drift steadily to destruction. Each generation would be more feeble and degenerate than the preceding one. If the agency were a beneficial one, such as good and plentiful food, the race would be just as much thrown out of harmony with the environment, would just as much drift to destruction. What benefit would it be to a race of men, or insects, or plants, that they should be ten times as big and strong and active as they are now? Their food-supply would be ten times as much restricted. In size and vigour, as in other respects, races are adapted to their environments. If the influence altered, not the whole organism, but a particular structure, the result would still be the same; the harmony of the parts would be fatally disturbed. So, also, if a number of warring influences acted on the race, it would drift helplessly in the direction indicated by the sum of these influences.

The theory of evolution most widely—indeed, almost universally—accepted is Darwin's theory of natural selection. He supposed that Nature selects the more fit individuals for survival, while she eliminates the more unfit, and that thus the specific mean is raised and species are adapted to the slowly changing environment. In passing, I may say that Dr. Robertson's conception of this theory

is totally erroneous.\* He endorses Dr. Beard, who wrote: "If the Darwinian theory, with its chance elimination of individuals, be the true explanation of things, it must be proven that the said individuals are eliminated before they breed. Once they have bred, no elimination of them would be of the slightest value." But the Darwinian theory merely demands that inferior individuals should on the *average* be eliminated before they have contributed their *full* quota of offspring.

It should be noted clearly that the Darwinian theory attributes evolution to the action of injurious influences. It supposes that the agencies which tend to injure the individual are precisely those which tend to raise the specific mean by the elimination of the unfittest, and so to increase the adaptation of the race to the environment. Now, suppose an agency—malaria, for instance, which acts on the whole race—acted on the germ-plasm, and so caused the species to drift in a particular direction; then, plainly, natural selection could not select, for all variations would be unfavourable. It could not raise the specific mean when every individual tended to be below the mean of his parents. It is evident, therefore, that the theory of natural selection is incompatible with the supposition that the environment acting directly on the germ-plasm tends to alter its hereditary tendencies. It is compatible only with the theory that the offspring tend to vary *spontaneously* all round the specific mean "like bullet-marks round a bull's-eye," and so to present material for the work of natural selection. Indeed, the theory that variations are produced by the direct action of the environment is incompatible with any conceivable theory of evolution—*i.e.*, of adaptation. It is evident also that, if natural selection does prevail in the world, it must always have tended to eliminate those races or families the germ-plasm of which was susceptible to the direct action of the environment. Such families must quickly be thrown out of adaptation with their surroundings and eliminated. Whence it follows, if natural selection does exist, it must tend to bring about in the germ-plasm a high degree of insusceptibility to the direct action of the environment.

To test the truth of the theory of natural selection, and therefore of its corollary, the theory that variations arise spontaneously, we have only to note in what direction races exposed to some

\* January, 1905, p. 109.

powerful injurious influence tend to drift—to note whether they become more resistant to the influence or whether they become degenerate, and so ultimately extinct. The answer returned by Nature when we question her is decisive. Species undergo evolution only when exposed to injurious influences; when exposed to such influences they never degenerate. They degenerate only when exposed to beneficial influences. Thus, when exposed to extreme heat species tend to become resistant to heat; when exposed to cold they tend to become resistant to cold. No one can indicate how exposure to virulent malaria has caused negroes to degenerate; on the contrary, they have plainly become very resistant to the disease. Similarly, prolonged exposure to tuberculosis, measles, and other diseases, has rendered Europeans resistant to them, but it is impossible to indicate how they have degenerated as compared to Esquimaux, Tierra del Fuegians, Polynesians, and Australasians, who until lately were not exposed to these maladies. Tropical races have become resistant to such tropical complaints as dysentery, but we cannot indicate that they have in any way degenerated. The case is exactly similar as regards powerful narcotics. Alcohol and opium have rendered races long exposed to them more resistant—*i.e.*, less prone to excessive indulgence—but in no ascertainable way degenerate.

Turn we now to Dr. Robertson's contention. He admits, of course, the fact of evolution, but he insists that this evolution, and this adaptation, does not result solely from natural selection, and he declares that the environment acting directly on the germ-plasm is the cause of variations.

As Dr. Campbell says, "What has he to offer in the stead" of natural selection? \* It seems "all variations, both somatic and genetic, are evidence of efforts at adaptation." † Imagine it! the germ-cell makes "efforts" to adapt the organism which arises from it to the environment. Apparently this miraculous entity is not only able to modify itself, but it is a conscious agent endowed with a wide and accurate knowledge of the environment, and with prophetic powers. If Dr. Robertson does not mean this, I am sure I am totally in the dark as to what he does mean, and I should be grateful for a clear explanation. Possibly he will appeal to Dr. Beard's theory of germinal election, in which case I hope he will indicate precisely the agency which makes the election.

\* October, 1904, p. 62.

† January, 1905, p. 108.

What reply has he to make to the statement that races grow resistant, not degenerate, when exposed to injurious agencies? Only, so far as I can understand, a flat contradiction, a statement that such influences render races degenerate, not resistant.\* But we have the facts. Does anyone doubt that the inhabitants of cold regions are particularly resistant to cold, and the inhabitants of the tropics to heat? Is it not a fact that negroes flourish on the West Coast of Africa, where Englishmen cannot rear families? Is it not true that Englishmen are able to exist in such hotbeds of tuberculosis as the cities of their native country, in which a native of the Western Hemisphere almost infallibly perishes? What is the explanation of the fact that tuberculosis and other lately-introduced diseases are fast depopulating the New World of its aboriginal inhabitants? Is it not a fact that races that have long dwelt in cities are particularly able to endure the conditions of slum life, to which savages swiftly succumb? How comes it that Jews and Italians are temperate under the frightful conditions found in the East End of London, whereas races which have had no previous experience of alcohol drink to extinction whenever and wherever they have the opportunity? Why do the natives of India, who have long used opium, take it in moderation, whereas Burmans, Australasians, and Polynesians, who have lately acquired a knowledge of the drug, take it to lethal excess? What in all these cases is the evidence of degeneration?

Moreover, we have the undoubted fact that whenever an injurious agency ceases to act—that is, whenever selection ceases and the species is exposed only to beneficial influences—degeneration sets in. Thus, unless the unfit are eliminated with care, all breeds of domesticated animals and plants, which are those which we can best observe, tend to degenerate. Thus, also, internal parasites tend to become mere bags of eggs, when selection ceases except as regards their reproductive functions.

So far as I am able to understand Dr. Robertson—and I confess I am vastly puzzled at times—he appears to maintain that though individuals manifestly vary greatly from one another in

\* Dr. Robertson is exceedingly vague on this point (see April, 1904, pp. 251, 252). But if he does not mean to controvert the thesis that alcohol, disease, and other injurious agencies, are causes of protective evolution, it is impossible to understand the purport of his paper.

all other ways, yet in this one particular of resisting power against disease and narcotics they vary little, if at all. To him death from disease is a mere accident in which the individual's innate lack of resisting power plays no part. He quotes Behring's opinion that we get infected with tuberculosis by drinking tuberculous milk during infancy, and that the bacilli "remain latent until they find their opportunity for development, and that individual predisposition is of little account."\* But it so happens that the very people who suffer most from tuberculosis when exposed to it are precisely those who have had no opportunities of being infected during infancy, who have never drunk any milk but their mothers' milk—who, indeed, in many instances have never even seen a cow, let alone a tuberculous cow: Esquimaux, Tierra del Fuegians, Polynesians, and the like. Here are a couple of examples: "The tribe of Hapaa is said to have numbered some 400 when the small-pox came and reduced them by one-fourth. Six months later a woman developed tubercular consumption; the disease spread like fire about the valley, and in less than a year two survivors—a man and a woman—fled from the newly-created solitude." . . . "Early in the year of my visit, for example, or late the year before, a first case of phthisis appeared in a house of seventeen persons, and by the month of August, when the tale was told me, one soul survived, and that was a boy who had been absent at his schooling."† Now, had those unfortunate Polynesians, living in an almost ideal climate under conditions almost ideally hygienic as regards tuberculosis, a marked predisposition to the disease, or had they not? This is the kind of thing that so puzzles one in Dr. Robertson. One brings forward massive and conclusive evidence: it is quietly ignored; a flat contradiction on mystical grounds of "modern biological fact" is given, and Dr. Robertson assumes that he has settled the matter. We are left to hunt for rhyme or reason.

Here is a second example of his confusing method. Two germ-cells, a sperm and an ovum, conjugate. This fertilized ovum divides and redivides many times, and so gives rise, directly or indirectly—*i.e.*, by an "antithetic alternation of generations"—to an individual who has within him a number of germ-cells. According to Dr. Robertson, these germs are cells *in*, but not cells

\* April, 1904, p. 253.

† "In the South Seas," by R. L. Stevenson, p. 27.

of, the individual.\* To me this seems a question not so much of science as of dialectics, or at least of metaphysics; but to Dr. Robertson it is a scientific truth of vast importance; indeed, so far as I am able to judge, it is *the* "modern scientific fact" *par excellence*. But Dr. Robertson does not explain why it is of importance. It may, to be sure, be of import as a detail of embryology; but we are discussing heredity, and it has long been known that there is continuity of cells, and the continuity of the germ-plasm has long been assumed. So far as I am able—very dimly—to gather, he believes that the circumstance that the germs are *in* but not *of* the parent causes or proves them to be particularly easily influenced by the condition of the parent. But, in the name of clear thought and reason, how and why?

Here is a third example. On p. 253† he says of me: "We are solemnly told that our modern efforts to combat this disease are preparing a day of retribution for future generations." As a fact, I "solemnly" and categorically stated the exact opposite.‡

Here is a fourth example. "His fundamental idea that the craving for alcohol is an instinct, a special inborn character, can, I think, be shown to be untenable on physiological grounds."§ I do not know how it can be shown to be untenable on physiological grounds, and I should be glad to learn, but I am sure it can be shown to be untenable on grounds of common-sense and common experience. I certainly do not hold an opinion so egregious. I have long maintained (*e.g.*, "Alcoholism," p. 82) that the specific craving for alcohol is never instinctive, never inborn, but always acquired, and therefore that no man ever craves for alcohol who has not had previous experience of it. My contention has been merely that some men quickly and easily acquire a craving for the poison. But that is a very different thing, and also an obvious truth. The fact that there is no specific craving for alcohol, however, does not, of course, imply that there is no such general craving for stimulation as that indicated by Dr. Campbell.||

Here is a fifth example. "My study of this question forces me to the conclusion that the effects of alcoholic intemperance on the people of this country are much more grave and far-reaching than

\* April, 1904, p. 241.

† April, 1904.

‡ "Alcoholism," p. 172.

§ April, 1904, p. 254.

|| October, 1904, p. 62.

has been generally suspected. Most people have seen with any degree of clearness only its more immediate effects. The influence it has on the race has only been dimly suspected by a few, and they have been derided as ignorant and unscientific.”\* In this passage Dr. Robertson maintains, presumably, that alcohol is a cause of racial degeneration. But on p. 250, when seeking to meet the argument that, if parental alcoholism causes filial degeneration, it must of necessity cause racial degeneration, he says: “This does not in the least follow, for the contribution from other ancestral lines of germ-cells may counteract the tendencies to genetic variation produced by chronic alcoholic poisoning.” Now, which is true? Does alcohol produce racial deterioration or does it not? If racial deterioration *is* produced, what is the use of saying that it *may* not be produced? Moreover, in the case of malaria, and, indeed, of alcohol in some countries, practically all lines of germ-cells are affected.

Here is a sixth example: “On grounds of modern biological fact (!) I maintain that, as things are circumstanced amongst us, the people of this country could never be evolved into a universally sober race by allowing natural selection to have full play in the manner advocated by Dr. Reid.”† In reply, Dr. Campbell denied that I advocated free drunkenness as a cure for drunkenness. The following is Dr. Robertson’s remarkable rejoinder: “It was, however, Dr. Reid’s views upon this point that I criticised in my paper, and not Dr. Campbell’s, and here they are: ‘Every scheme for the promotion of temperance which depends for success on the abolition or diminution of the alcoholic supply . . . is in effect a scheme for the promotion of drunkenness.’ There is no limitation here.”‡ There is certainly no limitation in the passage quoted; but there is a great deal in the volume from which it is taken.§ Here is another passage from it: “I suppose nothing I can say will prevent some critics from declaring that I propose free drunkenness as a remedy for intemperance. It must be admitted that the statement is rather easy to make, and, when made, may be very effective with people who read the review, but not the work reviewed. . . . I traced the course of a pestilence, demonstrated the futility of the ordinary methods of

\* April, 1904, p. 255; see also p. 236.

† January, 1905, p. 110.

‡ January, 1905, p. 110.

§ See, for example, “Alcoholism,” pp. 167, 168, 193.

sanitation, and sought to provide a remedy. I was promptly accused of advocating the spread of the disease.”\* As a fact, Dr. Campbell is right. I have merely insisted that ordinary methods of temperance reform were, by themselves, worse than useless; that we should keep the drunkard sober if possible, but *in addition* deter him in every way from reproducing his type.

I hope Dr. Robertson will forgive me, but at times I am tempted to believe that he has not read a word I have written, but that he has derived his information from someone who knew someone who knew a man who had read something of mine. Otherwise it is difficult to explain his excessive inaccuracy.

In proof of his contention that variations are caused by the direct action of the environment, Dr. Robertson mentions Ewart's and Weismann's experiments†; but as he merely mentions without describing them, I will content myself with saying that they are fully dealt with in my “Principles of Heredity.”‡ He states, by way of proving that germ-cells are as capable of being injured as other cells, that various gentlemen have manufactured substances which are poisonous to germ-cells. The proof is unnecessary. I have never said that germ-cells could not be injured. I imagine, if anyone placed such a cell between the hammer and the anvil, the contrary could very easily be demonstrated. All I have maintained is that germ-cells, while they have life, do not readily change their hereditary tendencies; for natural selection has so dealt with them that it has fixed those tendencies in them almost as firmly as it has fixed life. It is just the same with other cells. If a gland be diseased for twenty years, and then recovers, it consists, not of a new variety of cells, but of cells of quite the old type. As we have already seen, considering the numerous and powerful influences to which every species on earth is subjected, life could not persist unless the hereditary tendencies were extremely stable.

“Selvatico-Estense mentions the case of a healthy woman married to a drunkard. She had five weakly children, all of whom died in infancy. By a second husband of sober habits she had two perfectly healthy children.” Dr. Robertson quotes also very similar cases from Sabrazes, Brengues, and Bournville.§

\* “Alcoholism,” p. 168.

† April, 1904, pp. 245-248.

‡ See pp. 40, 41, 69, 98, 160.

§ April, 1904, pp. 247, 248.

This sort of "evidence" is extremely common in alienist literature. I have asked its authors many times, and always without success, to explain how they establish the connection between the parental drunkenness and the filial degeneration. I am prepared to bring forward any number of instances of sober parents who have had degenerate children, and of drunken parents who have had normal children. Am I, then, entitled to argue that parental sobriety is a cause of filial degeneration, and parental drunkenness a cause of filial perfection? I venture to recommend the following passage from the historian Buckle to Dr. Robertson's serious attention: "We often hear of hereditary talents, hereditary vices, and hereditary virtues; but whoever will critically examine the evidence will find that we have no proof of their existence. The way in which they are commonly proved is in the highest degree illogical, the usual course being for writers to collect instances of some mental peculiarity found in a parent and in his child, and then to infer that the peculiarity was bequeathed. By this mode of reasoning we might demonstrate any proposition, since in all large fields of inquiry there are a sufficient number of empirical coincidences to make a plausible case in favour of whatever view a man chooses to advocate. But this is not the way in which the truth is discovered, and we ought to inquire, not only how many instances there are of hereditary talents, etc., but how many instances there are of such qualities not being hereditary."\*

Dr. Robertson mentions† also the experiments of De Vries, who transferred wild plants to a garden soil, and, finding that they soon began to vary greatly, concluded that the changes were due to the direct action of the environment. Here again is the old confusion between *post hoc* and *propter hoc*. There is no evidence that the variations were other than "spontaneous." The wild plants were adapted to the wild environments. Natural selection eliminated all that varied out of harmony with it. Not so in the Botanical Gardens, where all sorts of variations survived; hence the mutations. Moreover, De Vries did not complete the experiment. Nägeli did: he returned the altered plants to the wild environment, and found that they promptly reverted to the wild ancestral type.‡

\* "History of Civilization," vol. i., p. 177.

† April, 1904, p. 247.

‡ Weismann's "Essays," vol. i., p. 276.

Reversion, the reality of which Dr. Robertson denies,\* is a subject too big and complex to be dealt with here. It is, however, treated very fully in "The Principles of Heredity." If Dr. Robertson is able to controvert a line of that, I shall be infinitely surprised. This confidence is not due to overwhelming vanity; it is due only to a profound conviction that certain evidence is overwhelmingly strong, and that certain deductions follow of logical necessity. It is enough to say here that, since the child follows in the developmental footsteps of the parent, it is logically *inconceivable* that the development of the individual is anything other than a recapitulation of the life-history of the race. And if development is recapitulation, it is similarly *inconceivable* that regressive variations are anything other than acts of reversion. No phenomenon in Nature, therefore, is more common than reversion. Every living being presents numerous examples of it.

\* April, 1904, p. 249.