

not used. In the after-treatment of this case the subcutaneous saline injections proved of great value on the third and fourth days after the operation when the condition was critical. We are indebted for the notes of the case to Mr. Harris, the clinical clerk.

A girl, aged 11 years, was admitted into St. Thomas's Hospital on Oct. 14th, 1898. On Oct. 9th she was said to have had some shivering followed by a feeling of heat with pains in the limbs, but she had been ailing for some days before this with diarrhoea and a little nocturnal delirium. On admission she was noticed to be a well-nourished girl with flushed cheeks. She was brighter and more cheerful than is commonly the case at about the tenth day of typhoid fever. Widal's test gave a negative reaction and the diazo-reaction was doubtful. The tongue (though not dry) was thickly furred, the abdomen was somewhat distended, and the spleen could be felt. No characteristic spots were seen. During the first five days after admission the temperature remained steady between 102° and 103° F. It then began to fall with a large daily variation, but on Oct. 27th the fall ceased and a fresh accession of fever set in. Widal's test still gave a negative result. Nov. 13th found her in good condition. The bowels had tended towards constipation throughout, enemata being necessary from time to time. The temperature, which often reached 104° and sometimes 105°, showed a large daily oscillation. The spleen was considerably enlarged, one doubtful spot was noted, the abdomen was slightly distended, and the tongue was thickly furred but not dry. The pulse varied between 100 and 114. The patient was taking milk well, no stimulant had so far been necessary, and up to this point all appearances were favourable. At 1 A.M. on Nov. 13th, the forty-first day of the illness, she was seized with pains in the abdomen and was sick, while the bowels acted freely. The pulse was 108 and the temperature was 103.4°. Nothing abnormal was found on examination of the abdomen and the patient fell asleep. At 3.30 A.M. another attack of pain occurred, but there was no alteration in the condition of the abdomen beyond slight impairment of the respiratory movement, and there was no diminution in the area of hepatic dulness. Hot fomentations gave great relief, but at 6.45 A.M. the pain recurred, the pulse-rate rose to 120, and the temperature to 105°. At mid-day vomiting set in, a little dulness appeared in the flanks and the face began to alter; the pulse was 140, and the abdomen was more rigid though not distended, while the hepatic dulness remained unaltered. Permission to operate was obtained with great difficulty, so that the abdomen was not explored until 4 P.M., some 15 hours after the occurrence of perforation. The patient being anaesthetised with chloroform a four-inch incision was made in the middle line below the umbilicus. On opening the peritoneal cavity a large quantity of sero-purulent fluid and lymph escaped, which had a faint faecal odour. The caecum was brought to the surface with the idea of using it as a guide to the lower end of the ileum, the most frequent site of perforation, and a perforation was at once found on the anterior caecal wall. The orifice was about one-eighth of an inch in diameter; it was surrounded by a zone of indurated tissue, and through it a little gas was escaping. A thin layer from the margin of the aperture was excised. It was then closed by a row of Lembert sutures, which produced good inversion of the edges, and a few reinforcing sutures were also passed. The abdomen was thoroughly irrigated with sterilised water and was then mopped out with marine sponges, special care being taken to cleanse the pelvis and lumbar hollows. The intestines were slightly distended, but no thinned areas were seen and it was not thought advisable to prolong the operation by a lengthy search for a possible second perforation. A rubber drainage-tube was inserted into the pelvis, a smaller one into the right loin, and a gauze-drain was passed superficially upwards. The wound was closed with silkworm-gut sutures. The duration of the operation was 25 minutes. The pulse failed slightly towards the end. The patient was given morphia hypodermically and she slept throughout the night. Nutrient enemata, each containing a drachm of brandy, were administered every two hours. On Nov. 14th her general condition was fairly good. The abdomen was flat and resonant and respiratory movement seemed unimpaired. A fair quantity of sero-purulent fluid had escaped and a gauze-drain was introduced down the pelvic tube. Small quantities of milk and beef-tea were given alternately by the mouth. On Nov. 15th the discharge was less in amount but more purulent and

offensive. The abdomen remained unchanged but the patient vomited twice after taking milk. On Nov. 16th she had paroxysmal attacks of abdominal pain and the abdomen became somewhat distended above the umbilicus and moved less freely below. The amount of discharge was unaltered. The gauze-drain was removed, the lumbar drainage-tube was replaced by a strip of gauze, and the pelvic tube was changed for a smaller one. On Nov. 17th the abdominal pain became severe. The child was frequently sick, the abdomen became distended and tender, the discharge was serous and increased in amount, the pulse-rate rose to 146, the bowels acted five times, and her general condition was grave. On two occasions 60 cubic centimetres of normal saline solution were injected subcutaneously. On Nov. 18th the general and local conditions showed improvement. The pelvic tube was replaced by a gauze-drain. A few days later all anxiety as regards the peritoneal trouble was at an end. Rectal feeding was entirely discontinued, sufficient food being taken by the mouth. A parotid bubo on the right side now made its appearance and an abscess developed in the buttock at the site of an injection of saline fluid; the abdominal wound gaped widely and showed no signs of repair. The temperature was steady, between 102° and 103°, and the bowels were loose. On Nov. 26th the bubo and the abscess in the buttock were opened and the abdominal wound was re-sutured. On Dec. 2nd the patient complained of pain and tenderness in the right femur, but no swelling could be detected. On Dec. 4th a double otitis media supervened. On Dec. 7th the pain in the thigh had ceased, but there was effusion into the left knee-joint with pain and tenderness, followed on the next day by a similar condition of the right knee. A daily dose of five cubic centimetres of anti-streptococcic serum was injected on the next 11 days and during this period there was great improvement in the general and local conditions. The bubo and the abscess disappeared, the abdominal wound began to heal, the knee-joints became normal, the discharge from the ears gradually ceased, and the temperature fell to normal. The knee trouble was probably of septic origin and was not an instance of the specific typhoid arthritis described by Keen. For the next 24 days the patient slowly improved and there was no ground for anxiety. But the temperature, though always normal at some time of the day, was generally 99° or 100° in the evening and the tongue never became quite clean. On Jan. 12th, 1899, a definite relapse set in which lasted 14 days. Widal's test now for the first time gave a positive reaction and the spleen again became enlarged. The abdomen was somewhat distended, the tongue was furred, and the temperature at the height of the relapse reached to from 103° to 104°. On Jan. 26th the temperature became normal and real convalescence began. The patient is now—a year from the onset of her illness—fat and well, and she presents no sign of abdominal disease.

## THE TEMPERANCE FALLACY.

By G. ARCHDALL REID, M.B. EDIN.

PROFESSOR G. SIMS WOODHEAD's kind and complimentary, though somewhat hostile, address, delivered before the Society for the Study and Cure of Inebriety and published in THE LANCET of July 29th, 1899 (pp. 259, 260), furnishes me with a reason or excuse for re-stating my views on the alcohol question. I do so the more readily as it is clear that I have not as yet made myself understood. Professor Woodhead attributes to me opinions the exact opposite of those which I really hold, and since in this he is in company with most of my critics the fault is probably mine. I hope that on the present occasion I shall be more successful in explaining myself. The pity is, however, that I must discuss problems of heredity, for on them hinges the whole question of temperance reform, and heredity is an abstruse subject by no means easy of lucid statement.

All the problems of heredity in the last analysis are problems of physiology, an essentially medical science. It follows that all physiologists—that is, all medical men—should be authorities on heredity. Nevertheless no textbook of physiology discusses the question; the professors of physiology in their lectures and public utterances usually

display grievous ignorance of it; and though most medical men are sufficiently dogmatic on the subject, few seem aware even that the particular problem of heredity of which they speak with so much certitude has been the object of a prolonged investigation which tells overwhelmingly against the opinions which they hold. That particular problem is the question as to whether traits acquired by the parent are transmissible to the child. It is easy to demonstrate the importance of it to our profession. Suppose a man pursues a course of life which makes him strong and healthy, do his children benefit through heredity? Suppose he subsequently falls into ill health, are his later children any the worse? To descend into particulars, suppose a man who is free from disease has children, and thereafter develops gout or phthisis, are his children subsequently born more liable to gout or phthisis than are the others? Again, suppose a man leads an active physical or intellectual life, do his children benefit? Are they damaged by reverse conditions? Yet again—and here we reach the kernel of our inquiry—suppose a man leads a sober and temperate life, are his children more prone to be sober and temperate? If he subsequently becomes drunken are his children born thereafter more liable to drunkenness than they would otherwise have been or than are the former children? In other words, in all these cases does the parental acquirement tend to reappear as an inborn or congenital peculiarity in the child? Nearly all medical men will answer in the affirmative. They voice the popular opinion and as yet they know nothing more on this subject than does the man in the street. It does not form part of the curriculum at the schools. Nearly all real students of the question, however, will answer the question in the negative. They know that a fierce controversy concerning it has raged for many years and that though the whole plant and animal kingdoms have been ransacked no single instance of the transmission of an acquirement has yet been proved.

Lately, an eminent physiologist argued that questions of heredity were questions of biology, not of physiology. Presumably he did not consider his science a branch of biology. I fancy that by the latter term he meant zoology and botany. But if, as he and most physiologists assert, acquirements are transmissible, the *how* of this transmission is a question of physiology. For example, if a blacksmith by labour enlarges his muscles and this peculiarity so affects his child that as a consequence it has muscles larger than it would otherwise have had, the *how*, the way, the means, the method by which this transmission is brought about is strictly a physiological question. I cannot imagine what it has to do with botany or zoology—except, indeed, as a question of animal physiology. If, however, it be maintained that medical men in general and physiologists in particular are not concerned with problems of heredity, is it not high time that we ceased to prate about them as we constantly do? No longer should we talk of heredity in connexion with health and disease, in connexion with modes of life, or rheumatism, or gout, or tuberculosis, or syphilis, or alcoholism. These questions should be left entirely for the consideration of “biologists,” to be dealt with when they have the time.

I cannot endorse this view. It seems to me manifest that questions of heredity, by whomsoever studied, are purely questions of physiology. No plant or animal has been so closely studied as man. In him, therefore, if anywhere, we should seek the solution of a problem which has long agitated the world of science. Medical men have missed a great opportunity. By right of our superior knowledge we should have played a leading part in the most important scientific controversy of modern times—the late, nay, the present controversy concerning the alleged transmission of acquired traits—a controversy of unexampled importance, not only to the medical man, but to all classes of the community as well, to parents, to breeders of plants and animals, to educationalists, to philosophers, to temperance and moral reformers, to statesmen, and to sociologists. We have not followed even at the heels of “biologists.” Our ideas of heredity are not more advanced than those which prevailed in the days of the patriarchs.

Before considering the point at issue let us make sure that we know precisely what is meant by the term “transmission,” and that we clearly grasp the distinction between the acquired and the inborn. We shall thus avoid the confusion which vitiates the writings of most medical authors on the subject. An acquired character may be defined as one which results from the action of the environment on the soma—

the body-cells as distinguished from the germs. Thus all characters which result from exercise—the growth of the limbs, for instance—are acquirements, as are all changes induced by disease or other agencies. As regards the mind, all which dwells within the memory—all the separate words of a language, for instance—are acquirements. Technically an acquirement is termed a “modification.” An inborn trait, on the other hand, is one which results in an organism from the constitution of the germ-cell (or pair of cells) whence the organism sprang. Thus a man’s limbs, his heart, his nose, &c., are inborn traits. They arise because his germ is so constituted that, under fit conditions of shelter, nutrition, &c., it tends to proliferate into an organism having those peculiarities. Now, though every organism tends to reproduce its like, this is true within limits only, for offspring invariably differ somewhat from the parents. Such differences, when they depend on differences in the constitution of the germs whence the parent and child sprang, are technically termed “variations.”

New characters are, therefore, either modifications or variations. Variations, like all other inborn characters, are admittedly transmissible; but, as I say, the majority of those who have given scientific attention to the question deny the transmissibility of modifications. It is not as yet known for certain how variations arise. Many theories have been enunciated, but I need discuss none of them except the doctrine that the parent’s acquirements tend so to alter his or her germs as to induce similar variations in the offspring which spring from the germs. This hypothesis is, of course, that which affirms the transmission of acquired characters. It is important to note that an acquirement, if transmitted, would cause in the offspring, not another acquirement—i.e., modification—but an inborn trait—i.e., variation—since the latter would be due to a change in the germ-plasm. The only case in which an acquirement could be transmitted as an acquirement would be when a modification in a mother so affected her *fetus* that a similar modification arose in it. In that case the modification, if again transmitted, would appear as a variation in the third generation. Obviously male parents can transmit modifications only as variations.

The body is compounded of cells. No single cell is the offspring of any other co-existing cell or group of cells; but every cell is derived from a pre-existing cell. *Omnis cellula e cellula*. The body is, in fact, a cell community all the members of which have descended from a common ancestor, the fertilised ovum. For the purpose of our inquiry cells are separable into two distinct classes, the somatic cells and the germ cells. Now suppose a man acquires a character—suppose, for instance, that he enlarges his arm muscles by exercise, or by some injury to his brain caused by disease or drink or accident he acquires epilepsy—then, if his modification is to be transmitted, his arm or brain cells, situated far distant from his germs, must so influence the latter, must so alter the constitution of their plasm, that the children into whom they proliferate long after separation from the parent organism have, inborn, the particular character which the parent acquired. Surely this consideration renders quite unbelievable the hypothesis that acquired characters are inherited? By what machinery is the alteration in the germ-plasm brought about? It must be remembered, though medical men seem generally to forget, that a child is not derived from the whole of his parent’s body. He develops out of a very minute portion of it only—the germ-cell. His eyes are not the offspring of his parent’s eyes; his legs have not origin in his parent’s legs; his brain is not descended from his parent’s brain; but every portion is derived solely from the germ-cell, which, so far as we know, is indebted to the body-cells for shelter and nutrition only. Again, it must be remembered that in the germ there are no tissues similar to those in the parent—no muscle, bone, or nerve-cells, for instance. It is vain to argue that the potentiality of them is present; the fact remains that they are not there. They only arise much later in the very remote cell descendants of the germ. Yet, again, it must be remembered that it is not asserted by any one that acquired modifications do not influence the germs.<sup>1</sup> It is only asserted that there is absolutely no evidence that changes in the soma influence germ-cells in such a particular

<sup>1</sup> On the contrary, we know as a fact that some germ-cells at least are capable of being altered by changes in their environment. The bacteria of disease, as unicellular organisms capable of continuing the race, are all germ-cells. Bacteriologists continually alter them by changing their environments.

and unlikely direction that the modification of the parent is transmuted through heredity into a variation in the child. For instance, it is not asserted that changes in the brain may not influence the germ in any one of a million or a billion possible directions; it is only asserted that they do not, except perhaps as a coincidence so rare that no instance of it has ever been found, influence the germ in the particular direction which is meant when the transmission of an acquired character is maintained. Lastly, it must be remembered that when some external agency finds entrance into the body and acts directly on the germ it is not asserted by anyone that the germ is incapable of being modified by it. Thus, it is not asserted that alcohol or the toxins of disease when present in the parent's blood do not influence the germ. It is very probable that they do. It is only asserted that external agencies do not, except again as a coincidence so rare that no instance is known, so influence the germs that the offspring arising from them have inborn the modifications which the agency caused in the parent. For instance, experience of some diseases gives rise in a man to acquired immunity—i.e., a great increase of resisting power; but it is denied that his germs are so affected by the toxins in his blood that his children as a consequence develop a resisting power greater than they would otherwise have had. It would be very extraordinary if they did. Such a variation in the child would imply a very delicate and peculiar alteration of the germ, and there is no reason known why that particular alteration should arise, rather than any one of a million other possible alterations—say a taste for blue china or for chocolate, or a diminished liver or an enlarged toe. It will be observed that this last question, strictly speaking, is not a problem of heredity at all. Nothing is supposed to be transmitted from the parent to the child. On the contrary, it is an external agency which is supposed to affect both the parent organism and its germs—the latter in a highly particular way. It is obviously necessary, however, to discuss it, for if the germs were so affected, then, since variations are transmissible, the consequent variation would tend to be transmitted to future generations. I should add that though it is not denied that modifications of the parent or external agencies circulating in the blood may so alter the germs that the offspring arising from them are also altered in this or that other way, yet no instance of such an alteration has been traced, at any rate in the higher animals. The complexity of the high animal body, the multitude of its characters, renders such tracing of cause and effect impossible.

I hope I shall not be thought unduly prolix in the foregoing. I am addressing medical men, and the vagueness and inaccuracy of medical opinions on heredity have compelled me to a more or less detailed statement. I have had a somewhat bitter experience. For instance, the question of heredity was discussed in the Physiological Section of the British Medical Association when it met lately at Portsmouth. My principal critic maintained that acquired traits *must* be transmissible *because all new characters are acquirements* and many new characters are known to be transmissible. Apparently he had never heard of the distinction between modifications and variations. In the Psychological Section the question of heredity was also discussed. Here my chief critic,<sup>2</sup> after prefacing his observations with the remark that "it was difficult to grasp the significance of the term 'hereditary transmission' without a clear and correct conception of the mechanism of heredity," stated that "persons of comparatively sober habits might at rare intervals—e.g., on the occasion of a wedding or other festivity—take more alcohol than was good for them, and an embrace given in a moment of temporary drunkenness might thus prove prejudicial to the offspring begotten under such circumstances." The spermatozoon is a free parasitic organism when residing in the seminal vesicle. My critic apparently thought, for reasons unknown to me and unknown I am sure to most students of heredity, that the moment when this unicellular animal enters the male passage is precisely the instant when it can be most modified in a highly peculiar direction by an external influence operating on its parent and on itself.<sup>3</sup> It would be

interesting to know on what evidence he founded his opinion. He mentioned no observations, experiments, or statistics, and in the absence of these I protest that his ideas of heredity seem to me to be founded, not on sober science, but on a belief in magic.

It is apparent that we should accept the doctrine of the transmission of acquired characters only with the greatest caution. It is so very unlikely that, in common with other unlikely things, only the clearest evidence should satisfy us. Has this evidence been furnished? It has been sought for far and wide. Many of the acutest intellects in the world have given it unremitting attention. But, as I have said before, though the whole plant and animal kingdoms have been ransacked, no single instance of the transmission of an acquired character has yet been proved. The doctrine of such transmission is, in fact, a wildly improbable assumption, founded wholly on a *priori* considerations and unsupported by a solitary fact—a single concrete piece of evidence. Yet to this doctrine most medical men give most unquestioning adherence, notwithstanding the fact that their own sciences furnish proof against it far more massive than any which is accessible to botanists and zoologists. Consider the evidence from physiology. The extra development of the blacksmith's arm is rightly called an acquired trait since it results from exercise and use, not from germinal conditions. But no infant's arm develops into an adult's arm without stimulation similar in kind though less in degree. Therefore all that separates the infantile from the adult arm is acquired. The same is true of most of the other structures of the body, all of which do not develop except under the strain of use. How vast, then, are man's physical acquirements, yet when are any of them transmitted? Every infant has to make afresh under similar stimulations the modifications which its parent so laboriously acquired. If it be argued that exercise and use increase, not only the individual's acquirements, but also his power of making them, and that it is the latter which is transmitted, I have only to reply that here is a manifest error. In the passage from infancy to old age the power of making acquirement constantly declines. In the infant it is at a maximum, hence his development into adult man. In the old man it is at a minimum; it is almost lost; therefore, since nothing in this case is acquired, nothing can be transmitted. The same arguments apply to mind. At birth the infant's mind is a blank. His subsequent mental acquirements are immense. Every single thing contained within the memory of adult man, every single word of a language for instance, is a separate acquirement. But when are the contents of the parent's memory transmitted to the child?

Turn now to the evidence afforded by pathology. When is acquired immunity against disease, a change affecting the whole body, ever transmitted? In syphilis, says my medical reader. But will he prove that so-called "congenital" immunity to syphilis depends on germinal changes, not on a fresh acquirement by the foetus? When are the modifications caused by disease and accident transmitted? Many cases are known, says my surgical reader. If they are one can be proved. Will he prove it? The fact is that medical attempts to prove the transmission of acquirements have generally demonstrated nothing more than an insufficient acquaintance with the conditions of the problem, or in some cases the most distinguished powers for confused thought. Hæmophilia and supernumerary digits have been instanced, but no attempt has been made to prove that these peculiarities are ever acquirements. As a matter of fact they always begin in the race as variations. The beard even has been instanced merely because it arises late in life. Brown-Séquard's epileptic guinea-pigs have been much relied on; but guinea-pigs have been proved to be very liable to epilepsy in the absence of parental injury. Most commonly arguments turn on the supposed transmission of mutilations and maternal impressions, but the element of coincidence is rarely thought of. In a million cases the parent's mutilations and impressions are plainly not transmitted, though the children vary otherwise in a billion ways. In the millionth-and-one case something appears in the child that more or less faintly resembles the parent's peculiarity; thereupon the transmission of an acquirement is triumphantly proclaimed. I daresay I shall be thought dogmatic and even arrogant in what I have written, but even if by such means I can awaken professional interest in this great matter I shall have done a work worth the doing and shall not greatly care.

<sup>2</sup> THE LANCET, August 12th, 1899, p. 452.

<sup>3</sup> I do not know that it greatly matters, but it is perhaps worth mentioning that the union of the sperm with the ovum does not take place till a later date. During the embrace the sperm merely passes from one environment, the male body, to another very similar one, the female body.

There is practical agreement that all the races of mankind sprang from a common stock. If that be admitted it matters not what the original stock was—Adam and Eve or a species of lower animal. In the course of ages the various races have diverged vastly and in a multitude of ways. There are big races and small, white races and black, races which are highly resistant to this or that disease and races which are less resistant, races which are prone to excessive indulgence in alcohol or in opium and races which are much more temperate. The differences are endless and can have arisen only through the transmission and accentuation through generations of inborn variations, or of acquired modifications, or of both. Even if we suppose that the Deity predestined and directed the evolution the course of it must yet have been on these lines. It is admitted on all hands that variations are transmissible and therefore everyone must admit that human, as distinguished from pre-human, evolution must be due in part at least to their accentuation. It is affirmed by most medical men, but denied by the mass of those acquainted with the facts, that acquired modifications are transmissible. Human evolution being admitted, it follows that controversy can only arise as to what part in it acquired modifications have played. As medical men we are not directly concerned with racial alterations in shape or colour and so forth. To us belongs especially the consideration of racial alterations in relation to disease or to potent and lethal narcotics like alcohol and opium. In the case of disease one fact of high importance at once rises into view. All races which have long been afflicted by any prevalent and deadly zymotic disease are proportionately more resistant to it than races which have been less afflicted or not at all afflicted. Thus Englishmen are more resistant to tuberculosis than the natives of India, who in turn are more resistant to it than West African negroes. The last, on the other hand, are more resistant to malaria than Hindustanis and infinitely more so than Englishmen. It is invariably argued by medical men that the increased resisting power to disease which racial experience confers is due to the transmission of acquired immunity. The one generation, it is supposed, acquires immunity and transmits some part of it to the next, which, starting from a position of comparative advantage, again acquires immunity and transmits still more resisting power.

On the other hand, diseases are plainly selective in their action; that is, individuals vary as regards their inborn power of resistance to this or that disease. For example, some people take scarlet fever and perish, their inborn resisting power being low. Others of higher resisting power take the disease but recover—i.e., they acquire immunity. Yet others are congenitally so resistant, that they escape infection altogether. It is a known fact that offspring tend to inherit the inborn peculiarities of their parents and in particular that the children of parents weak or strong against any given disease tend to exhibit a like peculiarity. Now, there are some diseases against which immunity cannot be acquired, but against which there has been considerable evolution. Tuberculosis is an example. The English, who have long been weeded out by that disease, are able to persist in the face of it even when the conditions are most unfavourable—e.g., in large cities—whereas Polynesians and Australasians are exterminated by it under conditions much less onerous. It follows that evolution against disease is possible through the transmission and accumulation of inborn variations alone. In other words, in such cases the evolution is due solely to natural selection. Contrasting with tuberculosis, there are some diseases which, though prevalent, are not deadly. Nearly everyone in the community is afflicted by them and nearly everyone recovers and acquires lasting immunity. Chicken-pox is an instance. In this case, there being no elimination of the unfit, there can be no natural selection of the fit; evolution, therefore, cannot proceed on lines of inborn variations, but, if it occurs, must result solely from the transmission of the acquired immunity. It is highly significant, nay it is conclusive, that no evolution of resisting power occurs in such cases. Races which have for untold ages suffered from chicken-pox are as susceptible to it and suffer as much from it as do races to which it has been newly introduced. It appears, therefore, that evolution against a disease occurs only when the latter is the cause of a considerable death-rate—a positive proof that acquired resisting powers are not transmissible. But if acquired immunity, a profound physiological change which affects the whole body, is not transmissible, what likelihood is there that those lesser and merely

local effects of disease which the profession is continually instancing are so.<sup>4</sup>

Almost without exception medical men believe that the acquired effects of alcoholism are transmissible. It is easy, however, to demonstrate that this is just one of those hoary superstitions which have currency only because they have never been questioned. The physical effects of intemperance, the diseases produced by it, are important, but the principal effect is a mental one without which the physical effects could not arise, for no one would drink. Indulgence in alcohol awakens in the drinker the craving for it. It is said that this craving for drink is transmissible. The term, however, is used in an exceedingly loose way and leads to endless confusion. It is necessary, therefore, to define our meaning precisely. Before a man can crave for drink he must first of all have a mind so constituted that drinking is, or may become through indulgence, delightful to him. This capacity for enjoying alcohol is clearly inborn and transmissible. Secondly, no man can crave for drink except he has had personal experience of it and has thus learnt the sensations which it awakens. This knowledge is an acquirement and is as clearly not transmissible, since no man can remember the sensations which drink aroused in his father before he was born. Strictly speaking, therefore, the craving for drink is not transmissible. But within limits indulgence in drink increases the capacity for enjoying alcohol and therefore the craving for it. In other words, the more a man drinks the more within limits does he crave for drink. The limits vary immensely with different individuals. Prolonged experience of alcohol increases but little, if at all, the capacity of some men for enjoying it. Such men never crave for deep indulgence and are sober all their lives. In other men continued experience begets a considerable increase of the capacity for enjoying alcohol. They crave more and more strongly for deeper indulgence and eventually, unless saved by lack of opportunity or the exercise of strenuous will-power, they become drunkards. We have, therefore, as factors of the drink crave, first, the drunkard's inborn capacity for indulging in alcohol; secondly, his acquired increase in this capacity; and, thirdly, his recollection of the sensations drunkenness induces. Now, when transmission of the drink crave is spoken of I take it that transmission of the second factor is generally meant. It is supposed that the father's drinking increases his capacity for enjoying drink and that this so affects his subsequent children that they have inborn a capacity for enjoying alcohol (or a faculty for acquiring that capacity) greater than would otherwise have been developed. In other words, it is supposed that the parent's drinking increases the child's proneness to drunkenness. If this be true it is a fact of momentous importance. In that case not only is it the single instance of the transmission of an acquired trait known to science, but more tremendous issues hang on it. On it depends the whole question of temperance reform. If it be true, all the methods commonly advocated by reformers are certainly right; but if it be not true—if the parent's drinking does not increase the child's proneness to drunkenness—then, not only does this fact fall into line with all the rest known to us of heredity, but it can be shown that every scheme of temperance reform hitherto propounded—prohibition, local veto, the Gothenburg system, &c.—is in effect a *scheme for the ultimate promotion of drunkenness*.

Temperance reformers, including many industrious members of our profession, have compiled voluminous statistics proving that drunken parents tend to have drunken children, and have argued thence that the parent's drinking tends to increase the child's liability to drunkenness. The industry has been wasted. *Post hoc* has been confused with *propter hoc*. If I like peaches, is it because my father ate peaches? No sane man doubts that children tend to inherit the inborn peculiarities of their parents. A big man tends to have big children, a fair man tends to have fair children. The voluminous statistics prove only that a parent prone to

<sup>4</sup> Evolution against disease is a matter of enormous importance both from the political and the scientific points of view, but it cannot be dealt with adequately here. Consider, however, that we cannot colonise, but can only conquer countries where virulent malaria is present. The natives who have long suffered elimination persist, but our death-rate in such places is balanced, not by birth-rate, but by fresh immigration. To the New World, in places undefended by virulent malaria, we go as colonists. Our diseases, which have been hitherto unknown to the native inhabitants, exterminate them and we enter into their inheritance. To South Africa, where virulent malaria is unknown, we go as colonists, but the natives are not exterminated, for they also have suffered for ages from our diseases. We must therefore share the land with them. (Vide, *The Present Evolution of Man*, pp. 199 to 306.)



drunkenness tends to have children with a similar peculiarity, they do not prove that the parent's *drinking* increases the child's capacity for delighting in drink, and therefore his proneness to drunkenness. This could only be done and all sources of error eliminated by proving that nations hitherto without knowledge of alcohol are comparatively temperate when first introduced to abundant supplies of it, but become increasingly drunken as their experience is prolonged, the son inheriting the parental capacity for enjoying alcohol *plus* the increment due to parental indulgence, the grandson inheriting this *plus* a further increment caused by the drinking of the son, and so on. It is evident, since alcohol is a lethal agent, that such a process would at length exterminate the race.

But quite apart from the circumstance that it is wildly improbable that a change in the parent's mind—i.e., an increased delight in drink—can so affect his germ (situated far distant from the seat of his mind, the brain) that the child arising from it has inborn, long after the particular change which the parent acquired, or on the other hand quite apart from the circumstance that it is equally improbable that alcohol circulating in the parent's blood can change his germ in this particular manner rather than in any one of a million or billion others possible,—quite apart, I say, from all this, what are the concrete facts? All races which have had little or no experience of alcohol are excessively drunken when introduced to concentrated modern forms of it—so drunken that, given the opportunity, they drink to extinction—for example, the whole of the native inhabitants of America from Terra del Fuego to the Arctic Circle, the inhabitants of the rest of the Arctic Circle, the inhabitants of Polynesia, Micronesia, and Australasia, the natives of New Guinea and of the Andamans, and certain primitive tribes of Africa and Asia. All races on earth which are now temperate in spite of an abundant supply of alcohol were anciently drunken, but have for centuries—nay, in the case of the most temperate, for thousands of years—been afflicted by an abundant supply of that poison—e.g., Jews, Greeks, South Germans, Italians, South Frenchmen, Spaniards, and Portuguese. Indeed, throughout the world the tendency to excessive indulgence is in inverse proportion to the ancestral experience. The facts concerning opium are similar. That drug has been in use for hundreds of years in India; it has been used for two centuries in China; it was introduced within the memory of living man to Burmah. Witness after witness—in fact, every witness whose official standing or scientific attainments entitled him to attention, Sir John Strachey, Sir George Birdwood, Sir Joseph Fayrer, and many others—declared before the late Royal Commission on Opium that the natives of India never or very rarely take opium in excess. There was general agreement that, though many Chinese were temperate, many perished from over-indulgence. It was agreed on all hands that the Burmans were excessively prone to intemperance in the use of opium—so prone, and as a consequence in such thousands do they perish, that we, the British rulers of the country, are obliged to prohibit the use of opium in Burmah to the Burmans alone, while permitting it to all immigrant races (Chinese, Indian, Malay, &c.), just as in Canada and Australia we are obliged to prohibit the use of alcohol to the natives alone while permitting it to all other races. Two things, then, are positively clear: (1) that the capacity for delighting in alcohol—the alcohol diathesis in which drunkenness has its origin—arose in man quite apart from the use of alcohol, since all races which have had no previous experience of alcohol—e.g., Red Indians, Esquimaux, and Australian blacks—have the diathesis strongly developed; (2) that prolonged use—or rather abuse—of alcohol tends to eliminate the diathesis and render a race more and more innately sober.

How did the diathesis arise? The subject, belonging as it does to zoology rather than to medicine, cannot be discussed at length here.<sup>5</sup> How is the diathesis eliminated? Here we reach the crux of the whole matter. Alcohol, like zymotic disease, is a selective agency which continually eliminates the innately drunken—that is, those who greatly delight in it. Most sober men, and in particular temperance reformers, have a theory very flattering to their self-esteem. They assert that men are temperate or drunken according as they do, or do not, exercise self-control. But let me appeal to my readers, and ask each one whether he is sober because he exercises self-control. Does he say

"Yes." Then from the bottom of my heart I pity him. Continually tormented by his unsatisfied craving for drunkenness, he must be a miserable being—a being only one degree less miserable than an actual drunkard. I, most certainly, am not constituted like him. Never in my life have I had to resist the craving for alcohol. I am temperate, not because I resist temptation, but through lack of it. I have not the alcohol diathesis. Unlike him I come of a selected ancestry—an ancestry selected for survival by that potent agent of death, Alcohol. I am sure most sober men are constituted as I am, not as he says he is. They can, as workmen say, "take a glass or leave it." Most people with whom I am brought into social contact are temperate manifestly without effort. A little alcohol satisfies them; more would awaken sensations which, on the whole, are unpleasant. A certain class doubtless would enjoy deeper indulgence; but the craving is not so strong as to balance their dislike to the consequences—the social degradation, the physical and mental evil. A large remainder so delight in alcohol, are so driven to it as by the force of a tempest, that, ignoring the consequences, they seek immediate satisfaction and are drunken. A man with a strong will may resist temptation, as doubtless many abstainers do; but, generally speaking, the will of man, as of all animate creatures, is exercised to gratify, not to resist, his desires. The man with a powerful will and a craving for alcohol is therefore generally drunken. Men differ in size, in strength, in colour, in craving for sexual indulgence, or for tobacco, or for sugar, or for salt. There is no single trait in which they do not vary in degree. So also they vary as regards their craving for alcohol, and that lethal agent weeds out those who have the greatest craving. Their families suffer from neglect and poverty, and their children, if they survive, inherit the diathesis and in turn are poisoned. Statistics compiled by insurance and friendly societies prove conclusively that, from early manhood forwards, the death-rate of drinkers is much higher than that of abstainers, the higher rate arising almost solely from the excesses of deep drinkers who are smuggled into the societies or become drunken after entrance. Now surely it is evident that a race from which the alcohol diathesis is weeded out must change, must differ from what it would be in the absence of that process of elimination. Surely it is significant that all the sober races of the present time were anciently drunken—Greeks, Italians, South Germans, &c.; that all races recently introduced to alcohol are now excessively drunken; that every race is now temperate in proportion to its past experience of alcohol; that while the lethal narcotics alcohol and opium have been causes of evolution the non-lethal narcotic nicotine—like chicken-pox—has been the cause of no evolution; for races that have longest used tobacco crave as much for it as those to which it has been newly introduced. In his address Professor Sims Woodhead assumed that Red Indians were drunken because alcohol took from them their self-control. The author of another hostile address, writing to me privately, is of the opinion that they were drunken because alcohol raised them to the seventh heaven. I do not quite see what bearing these arguments have on my contention. The amount of alcohol that is pleasant to me does not cause me to lose my self-control or raise me to the seventh heaven and for that reason I am not weeded out by the practically unlimited supplies I can command. Under similar conditions the Red Indians, losing their self-control because they are lifted to the seventh heaven by alcohol, are weeded out. My contention is that the temperate nations are composed mainly of individuals like myself, whereas drunken nations are composed mainly of individuals like the Red Indians. A favourite notion is that the environment—particularly education—influences drunkenness. No doubt it does. Under happy conditions and proper teaching men are doubtless less drunken than under worse conditions and teachings. But I take it that such conditions do not on the whole weigh much in the balance. It must be remembered that no amount of education can change a pleasant sensation—say a taste for salt, or for tobacco, or for alcohol—into an unpleasant one. In England the better classes, with greater opportunities for drunkenness, are more temperate than the lower who have lesser opportunities. But I am not aware that the better classes seek pleasure less eagerly than the lower. It is, however, a fact that the better classes, speaking generally, have descended from the better classes of former times, who, having more purchasing power, have

<sup>5</sup> Vide *The Present Evolution of Man*, p. 327.

been more thoroughly weeded out by drink than the lower.<sup>6</sup>

Many theories other than the above have been advanced to account for racial differences with respect to drunkenness. Climate—but all savages from the North to the South Pole are drunken, whereas Jews in all climates are sober. The worries of civilisation—but South Europeans are more civilised and more temperate than savages. Absence of civilisation—but North Europeans are more civilised and more drunken than South Europeans. Strength of beverages—but savages, who are unable to manufacture alcohol or can manufacture it in very dilute solutions only, when they have the opportunity, are conspicuously more drunken than North Europeans. Parental or ancestral drunkenness—but savages, whose parents and ancestors never had any drink, are the most drunken of all. Education—what is there in the education of South Europeans and Jews which renders them so superior in this particular to North Europeans? Temperament—but how did differences in temperament arise if not through evolution? Advance what other hypothesis one will and there are numerous exceptions. This theory of alcoholic evolution, which is in accordance with all we know of evolution under natural and artificial conditions, alone covers all the instances. Surely, then, there is no escape from the conclusion that all races which are temperate have become so through evolution—through the elimination of the unfit, the innately drunken.

It seems, then, that there are two schemes for the promotion of sobriety in operation. Firstly, there is what we may call Nature's method, the elimination of the excessive drinker. Secondly, there is the temperance reformer's method, the elimination of drink. The two methods are plainly antagonistic, the operation of the one putting the other in abeyance. The question, then, is, which method ought we to adopt? which is the most practical? which offers the most certain and easy success? I am afraid that few will as yet agree with me when I say that Nature's plan is the better, the more practical, the least productive of misery. But when medical men "have a clear and correct conception of the mechanism of heredity" more of my brethren will agree with me. All races which alcohol has afflicted have plainly undergone protective evolution. They began their experience with a great proneness to drunkenness, but have ended with a lesser proneness. But a race which has undergone evolution does not stand still when the eliminating agent which caused the evolution is withdrawn. It reverts with a rapidity, which is proportionate to the rapidity of the previous evolution, to the ancestral type. Thus, if we cease to breed racehorses carefully—i.e., if we cease to eliminate the unfit—they revert rapidly to the original type, the ordinary horse. The same is true of all other prize breeds of animals—e.g., dogs, cattle, pigs, pigeons, poultry, &c. All garden plants, also, which have undergone rapid evolution under artificial selection—e.g., the rose, the apple, the pear, the peach, the cherry, the pansy, &c.—in the absence of selection swiftly revert to the wild variety.<sup>7</sup> If, therefore, total prohibition, the reformer's ideal, were enforced and drinkers no longer eliminated, a race which had undergone alcoholic evolution would degenerate towards the ancestral type and become increasingly prone to drunkenness.<sup>8</sup> If the prohibition were continued long enough that primitive condition would

be reproduced when the proneness to drunkenness was as great as it is among some modern savages—e.g., Red Indians and Australian blacks—who have never commanded an appreciable supply of alcohol. Now alcoholic evolution is possible to primitive peoples only when the supply of alcohol is scanty and dilute, as judging by the analogy of modern savages it must have been in the ancient world. With the strong and plentiful alcohol of modern civilisation the death-rate of primitive peoples becomes so large that they undergo, not evolution, but extinction, as has happened times and again in the Western Hemisphere. To be beneficial, therefore, prohibition must be eternal. It must endure as long as the human race endures. Temporary prohibition can result ultimately in great disaster, in greatly increased drunkenness, only. In the face of an increasing capacity for enjoying drink how could we secure this immortal permanence for a mortal law? What guarantee is there that a future generation of alcoholic degenerates would not repeal it? It is possible that in England, where the majority do not now crave greatly for alcohol, but where much misery is inflicted by it on the minority, a self-denying law enforcing prohibition might be passed. But in the grape countries, where the evolution has been greater, little misery is now inflicted; wine is there used almost solely as a beverage, not as an intoxicant. It is highly improbable that such races will ever pass a self-denying ordinance merely to benefit foreigners. Under such conditions how shall we prevent our thousands of travellers and sailors from acquiring the craving, and seeking means of gratifying it at home. If nothing else can be got men will drink methylated spirits. Alcohol is essential to many of our industries, arts, and sciences; must we abandon all of them? Even if prohibition involved the whole world we could not eliminate sugar or the organisms which produce alcohol from solutions of sugar. In other words, the means of production would remain. Anyone who had fruit could manufacture alcohol of intoxicating strength. Total prohibition is therefore impossible as a permanent policy, and, except it be eternally permanent, is worse, much worse, than useless. Partial prohibition, enforcing moderation rather than abstinence, is yet more impossible, for abstinence is much easier to the innate drunkard than moderation. It may be argued that total prohibition has as yet been successful with Mahomedans and Buddhists. As yet! We have still to learn the future of those races. I doubt, moreover, whether the most rabid reformer would willingly pay the price for prohibition that Mahomedans and Buddhists have paid. The same influence, religious fanaticism, which has rendered them sober has rendered them barbarous also. By limiting intercourse with more free and enlightened, if more drunken, peoples, and enforcing it by such means as the pouring of molten lead down the throats of the drunken, they have rendered prohibition possible. But who would pay that price? Archbishop Magee's saying occurs to me, "I would rather see England free than sober." We should have to manufacture a new religion which, unlike the Christian, forbids alcohol.

Turn next to the other alternative, the elimination, not of drink, but of the excessive drinker. The difficulties which we have now to face are not imposed by Nature but by man, by what we are pleased to call our "moral" system. We cannot alter human nature by laws or moral codes, but we can alter it by careful selection. How shall we eliminate drunkards? By what method? By poisoning them or permitting them to poison themselves with alcohol? Of course not. By confining drunkards as the insane and lepers are confined? Drunkards are so many that the State could not bear the cost. By forbidding drunkards to marry? It would be futile; drunkenness often develops after marriage. How then? By preventing drunkards reproducing their like—by forbidding the procreation of children by them. If drunkards were taken before magistrates sitting in open or secret session, as the accused preferred, and on conviction were warned that the procreation of children would subject them to this or that penalty—say, three months' imprisonment—the birth-rate of drunkards would certainly fall immensely. Of course, it will be argued that many would escape the meshes of this law. But that is an argument against all laws. This law would be more perfect in its operation than any other, for if the drunken father evaded it in one generation the drunken son will be taken in the next.<sup>9</sup> Of course, also, I shall be told that my suggestion

<sup>6</sup> I hope I have made myself clear, but such has been my experience in the past that I am not particularly sanguine. My view is that alcohol is a lethal agent, that men differ as regards their capacity for enjoying it, that it weeds out most those that enjoy it most and therefore indulge most, leaving the continuance of the race to the more sober, that therefore the longer a race drinks the more innately sober does it become through protective evolution. The view commonly attributed to me is that the more a man drinks the more sober he grows. My critics naturally think me an idiot. I—well it does not matter what I think of my critics. Most medical men when they think of the subject believe that races grow resistant to disease through the transmission of acquired immunity. Professor Sims Woodhead and others suppose I have applied this erroneous doctrine—which, by the way, I have repudiated many times and with all my might—to alcohol. But it happens that I do not think that drinking makes a man immune—i.e., indifferent to drink. Experience tells me that the more he drinks the more, within limits, is he inclined to drink. Besides, I do not believe in the transmission of acquired characters. This view is therefore the exact opposite of what I actually hold. I am not surprised that Professor Sims Woodhead disagrees with me.

<sup>7</sup> It is impossible to deal at length with the abstruse question of reversion here. The author, however, has done so in his work, "The Present Evolution of Man," pp. 18 to 30 and pp. 48 and 49, and also in a paper read at the Bristol meeting of the British Association and published in *Natural Science* for December, 1898. To these sources he must refer interested or doubtful readers.

<sup>8</sup> In like manner the negroes of North America must in time lose their powers of resistance against malaria.

<sup>9</sup> I have been told that my proposal is immoral. I am not in the least abashed. It seems to me more immoral to procreate starving and

is impracticable. Certainly it is impracticable at the present time, but only because so many people believe it to be impracticable. Did they think it practicable it would become practicable at once. To-day sanitary reforms are impracticable in Mecca, but must that man be branded as a crank who strives for sanitary reform? Moral ideas change from age to age and we may hope that in some particulars at least the morality of England will undergo radical change.

But suppose it be admitted that my proposals are, and must forever be, impracticable, what then? I can only reply that in that case temperance reform as at present conducted is an enormous blunder. In that case temperance reformers are living on capital. Reversing the maxim attributed to the Jesuits they are doing good that evil may follow. They are seeking our happiness at the expense of the happiness of our children and our children's children through unnumbered generations. Metaphorically speaking, they are striving to breed a long-tailed race of dogs by carefully preserving all the short-tailed individuals and pulling vigorously at their tails. They are seeking to promote temperance by preserving the innately drunken and haling them by moral and legal methods into sobriety. They may achieve temporary success but they cannot possibly be permanently successful, for the use of alcohol cannot be banished from the world and the craving for it—or, to speak more precisely, the constitution of mind to which drunkenness is delightful—would, through reversion, gather head in the race, till, like an obstructed mountain stream, it burst all barriers, when the last state of the race would be worse than the first. "It is surely clear, therefore, 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."

Professor Sims Woodhead argued: "Typhus fever was at one time rife in our squalid population. Now there was a little of it in just that kind of population in which the greatest damage was done by drink. Would they say for a moment that because a population was liable to typhus fever under certain conditions therefore they would take care that all the people took typhus fever in order that the survivors might not be susceptible to that disease?" My critic's argument would be invalid even had I said we should promote drunkenness. Disease and drink stand on a totally different basis. No man craves for disease, and if we banished it no man would strive to bring it back; but many men crave for drink, and did we banish it increasing thousands would strive to bring it back. Moreover, a human prey is essential to the microbes of such diseases as are non-malarial in type. Their normal habitat is in man during at least part of the cycle of their existence, for which reason these diseases are never contracted away from the haunts of men. We may therefore hope by improved sanitation, isolation, &c., to utterly exterminate the microbes and put an end to the disease. But what sane person can hope to exterminate or appreciably diminish the microbes which produce alcohol? Given a sufficient temperature fermentation occurs everywhere, from the tropics to the pole.

Two or three matters more I may allude to. It has been asserted that parental drunkenness tends to cause "nervous instability"—whatever that may mean; these vague terms are the bane of science—in the child which, in turn, leads to epilepsy, insanity, and what not. Now if this be true, then alcohol weeds out the unfit much more rapidly than I supposed and my contention is greatly strengthened. But truth

neglected children, who, if they survive, will be drunken and will starve and neglect the next generation, than not to procreate them. Moreover, the charge is brought against me by the very people who have rendered poisoning by venereal disease a non-illegal offence. Think of it, here in England at the present day if we poison by means of arsenic or strychnine and death results we are hanged by the neck; or if death does not result we suffer long terms of penal servitude; but if we do our poisoning in a particularly cruel and treacherous way, by venereal disease, we escape scatheless. For instance, it is open to any diseased drab to tempt and poison as many weak and ignorant boys as she can entice. It is open to any scoundrel wilfully and knowingly to poison even to death his innocent bride and the children he may beget on her: Thousands, nay, tens of thousands, of men and women are poisoned thus; thousands, nay, tens of thousands, of innocent children thus perish. We precipitated the great Indian Mutiny by forbidding infanticide in Oude, but of how much vaster proportions is the infanticide in our midst caused by syphilis—a particularly preventable disease.

compels me to admit that I have seen no real evidence bearing on this point. Statistics without end I have seen, but the old confusion between *post hoc* and *propter hoc* is ever perpetrated. If it be argued that inebriates very frequently have offspring insane or epileptic I must reply so have non-inebriates. If it be further argued that inebriates have a higher proportion of offspring so afflicted I must retort that it is precisely from those who have a tendency to insanity or epilepsy that one would expect inebriety, and that though this tendency might not find expression in the parent and may result only in drunkenness, yet it is to it, and not to the parental inebriety, that the filial epilepsy or insanity is probably due. Moreover, in these statistics no attempt has been made to differentiate between the effect of alcohol on the germ and its effect on the embryo and the foetus. Doubtless this is impossible, for mothers drunken before pregnancy are usually drunken during it, and often the fathers are drunken also. Still, unless it be done the statistics are inconclusive. No one doubts that alcohol is a poison. Very probably it does in many cases injure the developing brain of the child, with the result that subsequently epilepsy or insanity may supervene. It injures the mother's nervous structures we know and there is no apparent reason why it should not injure the child's. But this is one thing—it is not a problem of heredity; the effect on the germ of the alcohol circulating in the parent's blood is another thing. That, indeed, is a problem of heredity. If alcohol does so affect the germ that the child is mentally abnormal then here we have a variation, not a modification, as when the foetus is affected. Variations tend to be transmitted and therefore a race that used alcohol would on that hypothesis become more and more insane and epileptic till it became extinct. I am not aware that South Europeans have become so insane and epileptic as to be threatened with extinction. We must remember that there are no nervous tissues in the germ.

It is said that the drinking of pregnant women predisposes the children to drunkenness. I have no evidence that it does not, but I know of none that it does. I am aware only of a vast amount of guess-work and loose thinking. To me it appears at least doubtful that acts of involuntary drunkenness perpetrated by the embryo or foetus should predispose it to drunkenness a score or more years after.

The tendency to drunkenness is frequently spoken of as a disease. The term appears to afford satisfaction to the charitable, but it is hardly correct. Otherwise all races which have not been weeded out by alcohol must be held to be diseased—when the paradoxical conclusion is reached that alcohol, which causes the disease, cures it.

Southsea.

## A CASE OF TETANUS SUCCESSFULLY TREATED WITH ANTITOXIN.

By JOHN D. RICE, M.B. R.U.I.

ON June 9th, 1899, I was asked to see a boy, aged four and a half years, who had been "biting his tongue at night." On examining him the only pathological lesion which I could find was a stomatitic condition of the tongue and palate. He had no pain, no albumin in his urine, no sore-throat, and no enlarged glands. His temperature was normal. The mouth could be opened to its fullest extent and the tongue protruded for several minutes; there was no risus sardonicus nor apparently any rigidity of the muscles, except on walking, when the movements were rather stiff. The reflexes were exaggerated. The boy took his food well except anything pungent which made him cry owing to the sores in his mouth. I prescribed a calomel aperient and a stomachic. The child on this day went for a walk and complained of no exhaustion or pain except that from the stomatitis. At 10 P.M. I was again sent for as the boy had waked up screaming. His condition had not altered. He had taken bland foods well and remained up till his usual bedtime. He soon fell asleep, but woke up screaming several times that night; his face twitched slightly during sleep. The next morning the boy seemed perfectly normal; he was up and dressed and had taken his breakfast on my arrival and the calomel had acted efficiently. He was up all day and had no screaming fits until about 10 P.M., when he woke as on the previous night. Suspecting worms I gave him a