rapid increase of the insane which far overtops the general increase of the population. No doubt this rise is partly accounted for by the greater care of the numeration and the better knowledge of insanity. Nevertheless, I believe we cannot any more doubt that we have actually to reckon with a very considerable increase of insanity. This is proved, not only by the alarming increase in the number of the insane, but also by the simultaneous rise in the frequency of suicides, and the contrast presented by the town and rural populations."

The learned professor observes that we are living in a time of transition, and he hopes that coming generations will be able to enter on the stress and struggle of life with fresh strength and better weapons. We need not believe that this increase of diseases of the nervous system is destined to fatal progression; it may stop or it may retrocede ere long. It would be rash to assume that we are acquainted with all the causes, and the means of prevention are not entirely beyond our control

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Meeting VI.—March 7th, 1900

MR A. GORDON MILLER, *President, in the Chair*

ORIGINAL COMMUNICATION

ON HEREDITY IN DISEASE

By D. J. HAMILTON, M.B., F.R.S.E., Professor of Pathology in the University of Aberdeen

ALLOW me, in the first place, to thank the Council of the Society for the very great honour they have conferred upon me in asking me to take part in this discussion, and more particularly for the place they have assigned to me in the order of debate. When I think of the many members of this Society who have made heredity a special study, I feel the honour to be all the more flattering.

It will be acknowledged on every hand that the subject you have chosen for debate is full of interest; an interest which has become, if anything, more intense in late years than formerly, from the vast amount of literature, biological and purely medical, which has found its way into our midst, dealing not only with concrete examples, or supposed examples, of acquired characters being hereditarily transmitted, but going to the very root of the matter in speculations as to what heredity depends upon, and of what the mechanism consists which underlies its manifestations. It is one of the commonest experiences of life that not only the general conformation of the body may be transmitted from parents to child, but that the same holds good of the minutest details of conformation. Further, it is matter of common note that the most subtle mental features and traits of character may, similarly, be handed on from one generation to another. We have little experience of how far the finer manifestations of heredity prevail in unicellular organisms, but in the higher metazoa the phenomena are so blatant as to have attracted attention in all ages.

The subject of discussion this evening is the influence of heredity in disease, and here it will be advisable to recall the fact that mere congenital transmission of disease from either parent to child is not heredity. Most of those diseases which are transmitted to the fœtus *in utero* are of the contagious type, and, consequently, are to be referred back to the action of a microphyte. Such transmission of contagious disease is, of course, to be accounted for by direct inoculation of the fœtus, in most cases through the system of the mother. It used to be supposed that the placenta was a perfect filter against the transmission of contagion of the particulate kind ; it was taught, for instance, that anthrax is never conveyed from the blood of the mother to that of the embryo. And although the approach of the placenta to perfection in this respect is undoubted, yet now and again it fails, and the contagion passes over to the offspring. Diseases of the contagious type seem to differ in the facility with which they are transmitted by this means. Thus, in the case of anthrax and tuberculosis, the infection of the fœtus through the mother occurs only very rarely, while we know that in that of syphilis the liability is extreme. These cases, however, of intra-uterine contagion hardly come within the scope of our discussion, unless in so far as they can be utilised for the purpose of proving or refuting the alleged transmission of a disease which has been acquired.

Of all the numerous theories to account for the handing down of hereditary characteristics, there may be said to be two which have met with most acceptance ; not that they have much in common, but rather because they represent the two points of view from which the matter is regarded. Indeed, it may be said that nearly all other theories which have gained currency are to be looked upon as modifications or extensions of these. The one, it will be remembered, is known as the "Pangenic Theory" of Darwin, propounded by its author, with his characteristic modesty, as a mere provisional or tentative explanation of how acquired features may be transmitted ; the other, that of Weissmann, known as the theory of the "Continuity of the Germ-plasm."

Between what we recognise as living protoplasm and its ultimate molecules, there are probably several stages of subdivision, molecular groupings, to the lowest of which various names have been given in keeping with certain theories of vitality bound up with their supposed existence. Thus these supposititious supra-molecular bodies have been termed pangens (de Vries), gemmules (Darwin), micellæ (Nägeli), idioblasts (Hertwig), plasomes (Wiesner), and biophores (Weissmann). They all refer to what may be regarded as molecular aggregations, which, by their association and interaction, go to make up what is roughly termed in biological language "living protoplasm." According to Darwin, these excessively minute particles of living matter, "gemmules," as he named them, are constantly being given off from the body-cells and circulate freely throughout the system. They were reputed by him to collect in the reproductive-cells, and to imprint upon these the features which the somatic cells had acquired through external agencies. Thus they tended to proceed centripetally, to convey to the reproductive-cells those impressions which had been received from without, and to transmit these to the offspring. The inheritance of acquired characters, according to Darwin's hypothesis, was therefore habitual.

Weissmann, on the other hand, takes the view that germ-cells (ova and spermatozoa) and somatic-cells differ, in so far, that the germ-cells are the exclusive bearers of hereditary qualities, the transmission being effected through the instrumentation of what he calls the "germ-plasm." This "germ-plasm" is comprised in the chromosomes of the ovum and spermatozoön, is continuous from one individual to another, and is, therefore, practically immortal. He would regard the germ-cells as an exclusive royal line, as it were, handed on from generation to generation, and having no community, so far as the propagation of hereditary qualities is concerned, with the cells of the soma or perishable part of the organism. All new hereditary features are the result of variation in the structure of this "germ-plasm," and are brought about by the recurrence of slight inequalities of nutrition. He ignores the influence of external agencies acting upon the somatic-cells as an element in the production of heredity; he believes that only those variations which are blastogenetic become hereditary, those which are somatogenetic are not so. His theory supposes that the particulate elements of heredity proceed exclusively in a centrifugal direction, and is therefore opposed to that of Darwin. The inheritance of acquired characters, according to his theory, is impossible.

It will be evident that the whole matter of the alleged transmission of disease hereditarily hinges upon this debated point, as to whether acquired characteristics can be inherited; in other words, whether external agencies can so impress the soma, and through it the "germ-plasm," that the effects of these impressions become hereditary. Hæckel, Virchow, and Eimer are at one with Darwin in the belief that they can, and the evidence they have brought forward in support of the assertion is voluminous, in many cases striking, but by no means in all instances convincing. Virchow is a strong upholder of the theory of *causæ externæ* generating effects which are inherited, and it matters not whether these act on germ-cell or body-cell. "A living being placed under varying circumstances changes its functions and habits, and what it acquires it can transmit hereditarily." On the other hand, we have Weissmann, Ziegler, and others, just as strongly convinced that acquired properties are not inherited, but that hereditary tendencies are due to variation in the structure of the "germ-plasm."

The supposed transmission of *mutilations* I shall pass over in a single word by asserting that there is no evidence in support of the view. The Jewish and other Eastern nations have been circumcising since the days of Abraham, and probably long before that period, and I have yet to learn that Jewish children are born without foreskins.

Can we say, however, that an external agent of any kind giving rise to a morbid state of the body, ever so impresses the cells of the body that the disease becomes established as a hereditary peculiarity? Let me take up some of the best known examples of so-called hereditary diseases with the view of discussing them, and of calling forth the valuable experience which members of this Society must be able to bring to bear upon the subject.

Perhaps no disease is held to have so hereditary a tendency, at least in the layman's mind, as tuberculosis; and it is undeniable that the disease affects different members of the same family, and that the predisposition to it can be traced back in the ascendants as a distinct family strain. If I had come before this Society previous to the discovery of the tubercle bacillus, and had asserted that the disease is not hereditary and very seldom congenital, my remarks would have met with little credence. Yet such is the general conclusion of pathologists at the present day. With extremely few exceptions—so few that they may almost be neglected—children are not born tubercular even of tubercular mothers, nor are the young of the lower animals born tubercular under like conditions. Even in these exceptional cases there is the possibility that the mother suffered from genital tuberculosis while pregnant, the transmission of the disease under such circumstances to the developing embryo being, one would say, a very likely accident. The fact, however, that such contamination of the embryo within the tissues of the mother happens so seldom, shows with what difficulty the transference is effected. It has been asserted, but proof is certainly wanting in confirmation of the allegation, that the ovum may become infected through the spermatic fluid emanating from a person suffering from tuberculosis of the testicle or its adnexa. That the ovum at the time of impregnation may become inoculated, that in a manner it may become fertilised by the spermatozoid and inoculated with the bacillus of tubercle simultaneously, I cannot imagine to be true. After conception has occurred, and the embryo is fairly advanced in development, there is a possibility of such an occurrence, but to my mind the likelihood of its taking place even then is extremely small.

Putting, therefore, the view of tuberculosis being congenital out of court, we have to fall back upon the explanation of the undoubted proclivity which the disease has to run in certain families as dependent upon one of two factors—either direct inoculation of the individual in extra-uterine life, or the inheritance of a particular predisposition, the tubercular temperament. No doubt when a member of a household becomes tubercular, the tendency to infection of other members, through family relationships, is very great, but this cannot be held to be the only cause of the disease selecting such families, and, it may be, affecting several generations in succession. Every physician and surgeon knows that the members of such families are, in a large number of instances, notable as tubercular subjects from their conformation of body and the liability they manifest to certain diseased conditions, not necessarily tubercular, but which nevertheless are sufficiently striking to confirm the suspicion of the hereditary taint. I need not enter into details of what this habit of body is characterised by ; the features I am referring to are matters of daily experience with you all. My argument is that it is this habit of body, not the disease itself, which is inherited ; and if you asked me in what the particular vulnerability consists, I should reply that, most likely, it resides in the epithelial protective coverings of the body being too little resistant, too easily stimulated by external agencies, too readily penetrated by the parasite of the disease. There is good reason to believe that, instead of Man being an animal very prone to tuberculosis, he is extremely insusceptible, otherwise it is difficult to explain how tuberculosis has not utterly decimated the human race. When tuberculosis is freshly introduced among savage communities, such decimation has been known to occur. Civilised nations have probably become in a manner hardened against the ravages of the parasite. There is no reason to believe, however, that any member of the human race is immune to its influence, in the sense that certain animals are immune to parasitical diseases which are peculiar to Man, or in the sense in which an animal may be rendered immune artificially. There is good reason for affirming, on the contrary, that there are few members of the human race who could not be inoculated with the tubercle bacillus were it implanted in their tissues.

The lung is the portal at which the organism usually gains admission, and here, it will be remarked, the protection of the surface consists simply of a delicate film of epithelial cells. In children and young persons with the tubercular habit of body, this epithelium tends constantly to be thrown into a state of germination, rendered apparent by slight attacks of bronchitis and catarrhal pneumonia, the desquamation accompanying these conditions exposing the underlying parts and thus encouraging the bacillus to take hold upon them. We have analogies bearing upon this in the ease with which the phylloxera penetrates the epidermis of the French vine, and in the difficulty it has in making its way through that of the American vine, which is thicker and more resistant. The organisms of putrefaction, and probably of septic disease, may be introduced into the healthy bladder of an animal, or that of Man, almost with impunity; while, if introduced into one whose epithelium is in a state of catarrh, and whose deep parts, consequently, are exposed, the danger of communicating septic disease is extreme. We do not know whether, in the case of individuals with the tubercular habit, the epithelia in other parts of the body, such as that covering the intestine, are to a like degree vulnerable. The fact that so many of us can consume tubercular milk without becoming tubercular, would tend to show that, under normal circumstances, the power of resistance possessed by the alimentary canal against this parasite must be very great.

What I would suggest, therefore, as inherently bound up with the hereditary tendency of tuberculosis, is this vulnerability of the protective epithelia, and this, there is every reason to believe, is handed down from generation to generation. In support of this assertion are to be taken into account certain epithelial manifestations which accompany the tubercular habit—namely, the very dark or very light degree of colour of the hair, the overgrowth of hair in the bushy eyebrows and long eyelashes, and, lastly, the occurrence of a lanugo-like overgrowth in tubercular children along the spine and over the legs. To my mind, these all point to an anomaly of the epithelial type which is peculiar to the tubercular habit of body.

Granted, for the sake of argument, that this habit of body, which is handed on from one generation to another, is the cause of the hereditary disposition to tuberculosis, and that it manifests itself in certain peculiar anomalies of the epithelia, we may ask, in reference to the subject of debate, how it is that this habit of body has arisen? Where has the inherited strain come from? What is its ancestral history? Can it be generated by vicious surroundings? I question whether it can. No doubt, once in the blood, the particular habit may be fostered by every external agent which tends to deteriorate the natural powers of resistance. But will such external agencies tend to produce a particular colour of hair, a certain narrowness of chest, tallness of stature, and other peculiarities which are distinctive of the tubercular constitution? My conviction is that they will not, and that we must go much further back in the history of the human race to get at the explanation of the matter. My own impression is that these features are the lineal descendants of a variation which took place far back in our history, that the variation has occurred irrespective of surroundings or external agencies, and that its influence has been propagated in the descendants ever since. It may be a variation which is common to many races, but one which apparently is intensely hereditary. When we see how racial peculiarities are propagated for ages, how the type of character no less than the lineaments of a race continue very much the same through all time, is it very Quixotic to suppose that a certain type of constitution has in a like manner become a race inheritance? It seems to me, on the contrary, to take a concrete example, that if the spirit of commercial enterprise exemplified in that memorable transaction between Abraham and the children of Heth, concerning the purchase of the cave of Machpelah, be still the spirit of the Jewish nation, there is no reason to believe that a variation of structure predisposing to disease may not have been handed down to us from remotest times.

The second so-called hereditary disease which I wish to bring before your notice for critical examination is gout. There is no more common belief than that this disease is engendered of high living, and that, once established, it is capable of being hereditarily transmitted. In a certain sense this notion is correct, and in a certain sense it is, to my way of thinking, entirely erroneous. There is no doubt that once the gouty tendency has been made manifest by the external agencies referred to the disease will most likely show itself in the next generation, but not necessarily so; it may skip a generation and reappear in that which follows. But are we quite sure that the gouty habit, and I employ this term in its widest sense, has originated in the abuse of articles of diet? There is such a thing as poor man's gout, and as Mr Jonathan Hutchison very properly remarks, there are certain individuals in whom no amount of abuse of either food or alcohol will excite gouty manifestations. The gouty individual is one with a peculiar habit of body, showing itself not only by the deposition of uric acid in certain tissues, but by modifying almost every function of the body. This habit is accompanied frequently by high arterial tension, and is followed, in course of time, by degeneration of the kidney and blood-vessels. Is this complex of phenomena traceable to any external agent such as high living? Or is the external agent simply one means of rendering the inherent vice apparent? The presence of uric acid in the blood and tissues has been held by Garrod to be the diagnostic feature of the disease, and this formation of uric acid has by some been looked upon as evidence of sub-oxidation of nitrogenous waste. We know, however, that reptiles and birds excrete a large part of their nitrogen in the shape of uric acid. May we not entertain, therefore, as a possibility, that the gouty constitution, so-called, is in part a reversion to some far back ancestor in which uric acid was excreted normally to a much larger extent than it is at present in an average member of the human race? It has always seemed to me that one great reason why it tends to be deposited in the tissues is the fact that the kidney, owing to its degeneration, fails to excrete it.

The conclusion I have arrived at as regards the origin and heredity of this disease is very much the same as that bearing upon tuberculosis, namely, that the gouty habit of body has arisen as a variation, and as such is hereditarily transmissible, and that excess of diet and alcohol merely render the habit of body apparent.

I come next to the great class of mental diseases, those diseases which manifest themselves in connection with what is termed the neurotic or the psychopathic constitution. In approaching this subject, it is to be borne in mind that habitude, sensation, perception, association of sensations, and images, in one word, the modality of the spirit are bound up indissolubly with organic substrata, and as these are hereditary, it follows that intellectual aptitudes are so also. As Debierre puts it—"Man thinks and acts, not spontaneously, but according to the blood which is in his veins ; that is to say, according to his heredity. He thinks, he feels, he wills much more through his ancestors than through himself." Thought is nothing more than a secretion, the result of a certain metabolism; and it stands to reason that from time to time, in the phylogenetic history of a nervous system so complex as that of Man, variations in the durability and resistance of the mechanism underlying this metabolism must have taken place, as we know to have occurred in the internal mechanism of other organs of the body. Mental derangements, we know, are among the commonest of diseases. It may be that the individual is not so far deranged as to require restraint. There are minor degrees of mental derangement which may be included under the terms wrong-headed, eccentric, fanatical, or hypochondriacal, which may manifest themselves in a line of psychopathic individuals, which are quite distinctive of the type, and which may break out at a certain period, and in a particular member of the family, in one or other of the various definite modes of what is commonly termed mental alienation. All such manifestations indicate a certain diminished resistance or power of endurance on the part of the nerve-cells of the brain ; and in this relationship, and in reference to what I have indicated as my own persuasion of the essential element in the heredity of the tubercular constitution, it must be borne in mind that nerve-cells are of epidermic origin, and that the tubercular constitution is frequently associated with the psychopathic. I have attempted to trace the predisposition in tuberculosis to a want of resistance on the part of the epithelia to the encroachments of the tubercle bacillus. May not there be a certain connection between the two classes of disease in respect of their representing, in reality, the same hereditary peculiarity, the same dyscrasia, the same tendency to decay on the part of nerve-cells and epithelial-cells ?

I have often thought that another factor predisposing to mental derangement is, that nerve-cells show so little capability, if any at all, of regeneration. We are born apparently with the nerve-cells which will serve us throughout life. Is it a matter of wonder that frequently these break down under unusual strain, or what is probably much more likely, as an inherited peculiarity? The germ-track followed in the ontogeny of the nerve-cells is very short, far shorter than in the case of many other cells throughout the body, and hence a state of maturity is reached at a comparatively early period, with an inclination to premature decay.

But in this class of diseases, as in others which may be considered in the ordinary acceptation of the term to be hereditary, the great question continues to assert itself, namely, whether external agencies can bring about a state of the nervous system which is distinctly morbid, and which can be transmitted for generations in the offspring. The evidence in support of the positive view of this question which is usually quoted, is that of the production and hereditary transmission of epilepsy in guinea-pigs. Brown-Séquard's experiments appeared to show that after hemisection of the cord or division of the great sympathetic in the neck, not only did the animals become epileptic, that is to say, not only could they be readily thrown into a convulsive fit, but that this peculiarity of constitution, thus engendered, could be transmitted to their young. The operation on the sympathetic brings about a trophic lesion of the eyeball which it was asserted is also transmitted. I must confess it has always seemed to me that an element of fallacy has entered into these experiments which would require to be eradicated before we can found any conclusions upon them. Have we crucial evidence to show that a mental disease may be excited through external agencies, as, for instance, by the abuse of alcohol, in a person free from any ancestral taint, and that this disease so excited can be transmitted through several generations. My own impression is that we have not; but it would be a matter of extreme interest to me, as I daresay to other members of this Society, to hear of any confirmation of the allegation? So far as I am personally informed, I feel that, in mental derangement, and in excess of perhaps any other form of disease, we have to do with an inherited peculiarity or variation, a variation which may have occurred in a far back ancestor and lain dormant for many generations, but which inevitably manifests itself under conditions of unusual external stimulation, and which is in no respect bound up etiologically or necessitated by this stimulus. The substratum which underlies the mental peculiarity is allied to that underlying the predisposition to tuberculosis or gout, and, probably, is referable to a fault in metabolism excited, it may be by an inherent bias towards degeneration in the nerve cells of the brain, and this is eminently hereditary.

From what I have already said it will be apparent that I do not regard syphilis as a hereditary disease. Congenital it certainly is, but I fail to note that, once acquired, it can be transmitted as a truly hereditary affection. As in the case of most contagious diseases, the contagion can pass from mother or father to child, but has an inevitable tendency to die out in the early years of life in the first descendant. This is not heredity but intra-uterine contagion.

Hæmophilia appears to be, in many instances, a truly heritable disease, and so far as our lights go in pointing its pathology, they seem to indicate that it is more a disease of the blood-vessels than one of the blood. The vessels do not seem to contract as they ought to do, and their coats are peculiarly friable. One could quite well understand this peculiarity being transmitted, just as the tendency to hæmorrhoids is said to be transmissible on account of the veins concerned having unusually thin walls. I have often thought, however, that there is something more in hæmophilia than this mere anatomical peculiarity, and that there may be a close relationship between it and menstruation. The fact that it is transmitted in the male line is significant. It has often been alleged that menstruation occurs in the male as well as the female; that although there is no actual discharge of blood, there exists the monthly constitutional disturbance of the general functions in the male as well as in the female, accompanied, it may be, by rise of temperature, a tendency to undue relaxation of the blood-vessels supplying the organs of generation, and possibly also of the hæmorrhoidal vessels. There seems to me to be a good deal of truth in this allegation. May not hæmophilia be simply a manifestation of this menstruation in the male?

Many more examples might be brought forward illustrative of the thesis I have started with in my remarks, but the above will probably suffice to elicit the opinions of others.

Turning now to another matter, I would ask the attention of the meeting to the consideration of what evidence we have of a hereditary tendency to disease becoming eradicated by inbreeding with individuals having no such tendency. How soon can we assert that the hereditary psychopathic constitution becomes eradicated in the descendants under such circumstances? Or, does it become eradicated at all? If we accept the statement that the extrusion of the polar cells with their quota of chromosomes before fertilisation occurs represents a true reduction process, that at this time a certain part of the maternal heredity is got rid of by this means, have we any reason for believing that, in course of time, a hereditary vice of constitution may entirely disappear? And here the facts of atavism come in to our aid. Polydactylism in man is supposed to be a reversion to a heptadactylous ancestor; the striping of mules is said to point back to an early equine ancestor, and horses and asses are said to contain solitary "zebra" determinants in their germ plasm; the three-toed horse anomaly is alleged to be a reversion to the fossil hipparion of the eocene period; the lanugo of the fœtus, a reversion to a hair-clad ancestor. I am told by a breeder of black polled Angus cattle that the progeny of a perfectly black polled bull and cow is sometimes a red calf with horns; indeed, unless the pedigree of mother and sire is pure for generations back, that no reliance can be placed upon the progeny they may beget. If these statements be true, what are we to say of the transmission of variations in constitution? My firm conviction is that if a vicious line is introduced it may die out, and probably does in most cases die out by interbreeding with a series of pure stocks, but that no reliance can be placed upon its not recurring atavistically, it may be, generations afterwards.

Lastly, let me refer briefly, and it must be briefly, for I am afraid I have already exceeded the limit of time put at my disposal, to the very interesting subjects of maternal impressions and telegony in disease. The influence of the mother upon the fœtus *in utero* must be very great, perhaps far greater than is generally admitted, in so far at least as nutritional factors are concerned. The subject of maternal impressions, however, is open to many fallacies, mere coincidences being interpreted as evidence bearing upon the matter. For my own part, I have little belief in maternal impressions as a cause of malformation or other peculiarity. As an example of the sort of evidence usually brought forward in support of the theory, I may refer to the case mentioned by Hippocrates of the white woman who bore to her white husband a black child, and who explained the occurrence by there having hung in her bedroom a picture of a black man—and so on it goes. Have we any proof of the occurrence of telegony in disease? From the evidence collected by Romanes and others, there seems to be no doubt of the mother being impressed by the male element of a first pregnancy, in such fashion, that the subsequent progeny by another male partake of the features of the father of the first born, and I daresay Professor Cossar Ewart may have something interesting to tell us on this point. But what I would endeavour to elicit from the discussion, this evening, in this relationship, is whether we have anything analogous in the transmission of hereditary diseased states of body. Are there any grounds for believing, for instance, that a father with a strong tubercular hereditary history, and bearing the lineaments of such in his person, can so affect the system of a healthy mother that the children subsequently born to the same woman by a second husband, with no such strain in his blood, and no such lineaments, have the tubercular characteristics of the first husband impressed upon them? I know of no evidence bearing on the problem, but possibly some members of the Society here this evening can throw light upon it.

In conclusion, allow me to formulate the substance of my remarks in the following synopsis :—

(1) There is no evidence proving that diseased conditions of body, excited by external agencies, using the term in its broadest sense, can be transmitted hereditarily through generations.

(2) That the various hereditary tendencies or predispositions to disease of the hereditary type have arisen as variations in the germ-plasm.

(3) That these predispositions to disease probably extend far back into the history of the human race, and break out only occasionally, in accordance with the laws of atavism.

(4) That external agencies are merely the means of bringing them to light.

(5) That there is little if any reliance to be placed in the evidence bearing upon the influence of maternal impressions.

(6) That there is no reason to believe that telegony may

not prevail in the case of hereditary predisposition to disease as it evidently does in regard to other characteristics.

Discussion

Dr Clouston, in opening the discussion, said that Prof. Hamilton had introduced a very vast and intricate subject, indeed a series of vast and interesting subjects, which to discuss thoroughly would take at least a winter session of this Society—subjects about which half a dozen treatises could be written. He was sure he expressed the minds of members of the Society that they were all deeply indebted to Prof. Hamilton for the way in which he had brought the matter forward.

With regard to the theoretical view of heredity now most generally accepted among biologists—that of Weissmann—it seemed to him that Weissmann had made three very striking admissions in regard to his general thesis that the effect of acquired characters cannot possibly be transmitted. In the first place, he admits that the germ plasm can be infected by such a disease as syphilis, so that thus the disease may pass from parent to child. His second admission is that the law of non-transmissibility of acquired characters does not apply to the very lowest organisms. The third admission is most important to medical men, who, he assumed, looked upon the matter from a practical rather than a theoretical point of view. It is that environment, especially climatic conditions, has the power of influencing the nutrition of the germ cell itself, and so altering its constitution that you may have an organism produced which is in some respects different from its parents. There was no doubt these admissions went some way to satisfy the universal experience and the universal instinct of the profession as to the heredity of disease.

In regard to the psychopathic constitution, he thought there was no doubt on the part of all who had looked into the subject, that it was well proved to be most hereditary. For two hundred years or more one of the royal families of Europe, whose history is well known, have undoubtedly exhibited this constitution in all its enormous varieties. Confining his remarks to the tendency of hereditariness in the neuroses, it always seemed to him to be an important admission on the part of Weissmann that environment and evil climatic conditions, and among these we must include evil dietetic conditions, may affect the germ plasm. Looking at heredity, particularly from a physiological and pathological point of view, what is the most important thing that good heredity can give the individual? He would say, in the first place, the power for the individual—the product of the germ cell and the sperm cell—to integrate, to develop and maturate on normal lines, from the moment of the first contact up to the full development of the individual. Assuming that you can have a certain debasement of this power, such an alteration in the nutrition of the germ cell as to affect it unfavourably, had we not in this way a practical explanation of a vast number of the neuroses? Taking the neuroses one by one, it would be found that by far the most important arise during the period of development through some defect in the maturation of the cells and of the tissues. In the case of epilepsy there was no doubt whatever that at least three-fourths of all epilepsy occurs during the period of development, and this was also the fact with regard to St Vitus' Dance and the great crowd of other nervous diseases, irrespective of the great number of congenital defects, idiotcy, imbecility, and teratological defects generally. In regard to insanity also, there was no doubt that mental alienation occurred in its most marked and characteristic forms during the developmental period. Looking at the brain cells in the mental area, if we examined a typical cell by the three or four processes best adapted for showing its constitution, it would be seen (and he thought Professor Schäfer would bear him out in this) that there was no more integrated and complicated structure in the human body than one of these larger brain cells. The Nissl's bodies, the reticular network, the fibrillation, the peculiar structure of the nucleus and even in the nucleolus—all this made one amazed at the enormous complication of this one cell; and there are in the brain something like three or four hundred millions such. Considering the integration of such a cell from the embryo up to the age of twenty-five, and thinking first of its marvellous structure, and secondly of its function, mentalisation in its numerous forms, and the connection of mentalisation and emotion with nutrition and with all the bodily functions, we can imagine how very small a deficiency in the nutrition of the germ plasm—how very small a change may mean some arrest, some debasement, or stoppage of the developmental process in groups of these cells; and such a small change he assumed to be possible on Weissmann's admission. In this he thought we had a theoretical explanation of certain hereditary defects. They were defects in the development of the nerve centres, arrests of maturation of the cells that constitute these centres. In this way we had a basis for the heredity of a large number of the neuroses.

There was no doubt whatever of the non-resistiveness of certain tissues about which Professor Hamilton had spoken. Take for example ringworm. It occurs in the child simply because its epidermis cannot resist the spores, while that of the grown-up individual can do so. Applying the same principle, he thought we should find that a very slight change indeed in the resistiveness of the brain cell would explain the well-known want of resistiveness to the numerous mental and bodily disturbing influences that act on the developing brain cells and cause adolescent insanity and other neuroses. Looking at the influence of the sexual and reproductive instinct, it was known that at a certain period of development there is an enormous strain on the constitution of every girl and every lad. Assuming that we had a slight alteration or even a germ infection of the germ plasm causing an arrestment of the developmental process; we had, he thought, an explanation in that way of a hereditary tendency. Certain individuals had weakness in the nutrition of the germ cell, and on account of this had not the power to develop into full maturity, this defect recurring in successive generations.

Then taking the other process—that of decay—the normal physiological process of growing old, coming to an end, and dying; we know that in certain families there are many individuals who become insane at the turn of life, as they become old, simply from an unphysiological method of nerve disintegration. From this cause there was a whole set of neuroses of what one might call the decadent period. Assuming that through defective germ plasm the brain cells are deficient in another great power of heredity, viz., that of carrying the individual physiologically through his decadence; there was here a theoretical explanation of the heredity of these diseases of the decadent period.

With Professor Hamilton, he did not believe, nor did he think anyone did, that mere personal mutilations, or the effects of surgical operations could be transmissible. Nor did he believe in Brown-Sequard's experiments, and he thought that Weissmann upsets to a large extent the conclusions that were founded on these experiments.

With regard to the alcohol habit, in which they were all profoundly interested, there was no doubt as to the actual fact, whatever might be the explanation, that in certain families there runs a tendency to the abuse of alcohol. He would so far agree with Dr Reid, writing lately in the medical journals, that no man has a born tendency to take whisky; but he thought a great many men unfortunately were born to what might be called a lack of higher inhibitory mental powers. He held that this was strictly analogous to the lack of developmental power in the man who was affected with adolescent insanity. There existed no doubt in these individuals a morbid reactiveness to the alcoholic stimulant. A young man with this highly neurotic tendency taking two glasses of whisky may go clean off his head, and he would lose mental power and strength of mind with each repetition of the dose; and would settle down at length into a dypsomaniac of the very worst class, viz., one that begins during the period of development—a class of which he was sure all present must have had experience as a nuisance to themselves and to the world. It was not the craving for alcohol that was inherited, but a general psychopathic constitution in which the alcoholic stimulus is an undue stimulus, and the mental control deficient. He thought it must be admitted that it was essentially a hereditary weakness in the brain reactiveness to alcohol; some people being able to take a large amount with impunity, others not being able to take a drop with any impunity whatever. A neurotic heredity is thus seen to resolve itself into general morbid tendencies rather than direct proclivities to special diseases; and this seems quite compatible with the Weissmann theory of heredity.

He was struck with one of Professor Hamilton's observations —that with reference to the Jews. Now in Jerusalem he once counted four distinct types of Jews—all quite different—so that he thought the Jewish element of permanence was really not so great as has been supposed. He saw the common type of dark complexioned, hook-nosed Jew; the red-haired, turn-up nosed Jew; the Georgian, tall, proud-walking Jew, fully armed with pistols and poniard; and the Indian Jew of the Hindoo type.

DR JOSEPH BELL was not prepared to take part in the discussion, but thought that in the course of a few minutes he might bring out that there was some ground for the belief in heredity from the surgical as well as from a public point of view.

A Board of Insurance would not pass a man who had three aunts and two uncles dead of tuberculosis without a certain amount of loading. If his ancestors have had cancer, they will take a similarly serious view of the case. He thought, however, that tuberculosis, syphilis, and cancer might be put aside as not being hereditarily transmitted diseases, but which might be transmitted in the germ cell to the germ cell as has been mentioned.

There was one little point in surgery which always struck him as interesting, and about which he had no doubt; that was the hereditary tendency in certain families to the formation of those small cystic or encysted tumours called wens on the head and upper part of the body. On every opportunity he had inquired of the possessor of one of these wens as to their occurrence in his ancestors, and had nearly always found this to be the case. Now the child was not born with one of these tumours, but what was transmitted was the hereditary tendency, some defect in the epithelium or in the sebaceous follicles, which at the age of thirty-five, forty, or forty-five, began to show itself in a large number of members of certain families. In connection with these matters of heredity there were, however, certain fallacies. If a woman thinks she is going to have cancer, she will be pulling about her breasts and brooding over the matter, and will be much more likely to get the disease than if she did not think of the matter at all. He had seen many cases in which a perfectly healthy woman had worried herself into the belief that when she came to the age of forty-five or fifty she would have cancer, because some aunt or other was afflicted at that age—at anyrate, something was produced which had to be removed, and which recurred and gave a lot of trouble.

There was a great tendency towards the inheritance of certain abnormalities of development, especially to hare-lip and cleft palate. But even here there might be a fallacy, as was instanced by the case mentioned by Professor Haughton of the young cubs which all died of hare-lip, but which was at length explained by the fact of the keeper having fed the lioness while in the family way on tit-bits, but without bones; and when the bones were subsequently supplied, the *hereditary* tendency to hare-lip entirely disappeared.

The whole subject was full of fallacies; he had, however, never heard a more interesting paper on the subject than that of Professor Hamilton, nor one which had introduced such a vast amount of discussible matter.

Professor COSSAR EWART would first congratulate Professor Hamilton on having arrived at a very orthodox view of the subject. He thought that on nearly every point Professor Hamilton's opinion was, from a biologist's point of view, as nearly as possible orthodox. On the other hand, he should say that Dr Clouston represented the opinion still held by the vast majority of medical men—a belief in the transmission of acquired characters. In Dr Clouston's remarks, one felt that all through he argued in favour of such transmission. There was, however, no single point that he could take hold of to controvert; he would therefore leave him to be dealt with by other speakers.

With regard to Professor Hamilton's first point—that in the case of gout, tubercle, and other diseases, they referred to a long time ago variation, which led to the appearance of gout in one family and tubercle in another; and that all the tubercle in the world, or most of it, has been inherited from previous variations. I don't think it is necessary to go so far back for either gout or tubercle. The variation that occurred a million years ago or a thousand years ago might also occur to-day. There was nothing that struck him more than that variation was enormously common. It was the easiest matter to produce a perfect epidemic of variation. Domestic animals reproduce themselves with great uniformity if kept apart; but the moment one mixed up two different races, strains, or breeds, one did something that was difficult to put in words, but the result was what had been best described as an "epidemic" of variations. In the human family there was a constant mixing up in all parts of the world, and the result was always a certain amount of variation. Hence he thought that the particular condition of things which predisposes or is favourable to the growth and development of the germ of tubercle might appear in any country, and might go on reappearing for ever.

Then coming to the question which ran through all this discussion—the influence of the surroundings or environment on the germ-cells, he understood that Dr Clouston contended that Weissmann admitted that the germ-cells might be altered by environment. He was quite sure of this, that Weissmann holds to the continuity of the germ-plasm; that the power to vary we find in the higher animals was acquired originally by their remote ancestors; and that the variation required to produce new types is obtained by what he calls a "reduction" of the germ-plasm, *i.e.* by a part of the nucleus of one germ-cell (the ovum) being got rid of to make room for another germ-cell (the sperm) having different potentialities. He (the speaker) thought Weissmann would not admit that the germ-cell is directly influenced by environment—that the environment is the actual cause of variation.

All the work which the speaker had been doing lately pointed to the fact that the germ-cells are excessively sensitive, but not to the fact of there being any transmission of acquired characters. If, for example, he mated a very young pigeon with an old pigeon, taking those breeds that have peculiar characters such as frills, hoods, etc., the first produce of the practically immature pigeon are all perfectly smooth-headed and smooth-breasted. But the next set of young, when the bird is in good form and feather—has fully reached maturity—may have all the characteristics of the parent. That meant that the first germ-cells are in an immature condition; so feeble or impotent that they are incapable of handing on the highly specialised characteristics of the parent. Give the germ-cells time and they will become so prepotent that they may hand on all the peculiarities even when the pigeon is crossed with another breed. In the same way if a rabbit that is still immature is put to an old buck—the buck may serve her, but the germ-cells being immature, there is for a time no ovulation. The sperm-cells lie waiting, sometimes for over a week, and the moment ovulation takes place the eggs are fertilised ; and in this case the young all take after the male. If on the other hand a doe is served, not at the right time, but a week or ten days after, when the next young come they are all exactly like herself. In the first place the eggs are hardly ripe, in the other they are over-ripe. He might give many instances to show how much the maturity and ripeness of the eggs determines the condition of the adults developed from the eggs. This was of course altogether different from Weissmann, and he thought it would, when thoroughly understood, enable those who cannot live without the transmission of acquired characters to feel happier.

Professor Hamilton had, all through his paper, escaped the fallacies which had so long prevailed ; but until others had reached the same stage there was not much prospect of progress.

He might, perhaps, say a few words on another subject, viz., reversion. It had been recently alleged that there was no such thing as reversion. The subject was originally discussed in a scientific way by Darwin, who thought that reversion had resulted from a sort of antagonism. That reversions occurred the speaker had no doubt. By crossing a pure white fantail having thirty feathers on its tail with a cross-bred pigeon, he had obtained a slaty-blue bird with only twelve feathers in its tail, which closely resembled the wild blue rock pigeon.

In the same way he had evidence of reversion in the Equidæ. In his zebra hybrids he expected to get something between horses and zebras, but had got something in its colouration very unlike either a horse or a zebra. The question arose—was it a new creature or an old creature? By making careful investigation as to stripes, etc., he came to the conclusion that his hybrids in their markings were a restoration of an extremely old type of horses. This had been verified to a certain extent by the discovery in Tibet recently of ponies almost as striped as some of the zebras. The Himalayan ponies and certain ponies found in Norway had characters which agreed in a general way with the hybrids—so that in these hybrids he believed we had again a very marked instance of reversion.

Dr J. W. Ballantyne said that he might best begin his remarks by referring to one point in Professor Hamilton's communication with which he could fully agree, namely, his disbelief in the potency of maternal impressions to cause conditions in the fœtus resembling the impression. At the same time he thought it might be said that to whatever extent we believed the mind capable of influencing the state of a part of the body, to that same extent, or to a degree rather less, the mother's mind might influence her parasitic growth (*i.e.* the fœtus *in utero*). But this amount of belief would of course vary very much in accordance with the elasticity of our belief regarding the influence of the mind over the body. Personally he lamented very much the intrusion of the subject of maternal impressions into biological problems, and more especially into antenatal pathology; it had kept that subject back, and had spoiled many case records which would otherwise have been most valuable.

Having agreed with Professor Hamilton on this point he was now forced to differ from him on two others. He did not think that we could separate off clearly the diseases of the fœtus due to intra-uterine infection from the hereditary diseases. Take first the case of small-pox, about which there could be no difference of opinion. When a pregnant woman had small-pox her fœtus *in utero* might take it. Manifestly the fœtal variola was not passed to the fœtus by heredity. Take another condition which could not be so easily divided off—fœtal tuberculosis. He thought there was no doubt that fœtal tuberculosis truly occurred, but very rarely; it was not to be expected that it could occur often, for the fœtus cannot be infected through the air (*i.e.* by the lungs) but only through the umbilicus; and as it was seldom that the tubercle bacilli were in the mother's blood, it was seldom that they reached the fœtus. Cases of fœtal tuberculosis had been reported, and he thought that (*e.g.*) the one described by Auché and Chambrelent formed an absolutely complete proof. In that case the mother had generalised tuberculosis, and tubercle was found in large amount in the fœtus in its liver and spleen (just where it was to be expected, considering that it entered by the umbilical vein), and to a small amount in the lungs. It was found also in the heart, in the form of tubercular endocarditis. Now, here was one of the points which he wished to emphasise: heart disease was one of the conditions admitted to be sometimes transmitted hereditarily. A mother with tuberculosis might give birth to a fœtus not with tuberculosis but with a cardiac morbid condition. He was quoting Hanot when he said that it had been advanced that the offspring of tubercular parents might escape tubercle by having a cardiac malformation or a congenital condition other than tubercle; it has been said that in this way the child might get rid of the heredity of tubercle. Was it not possible, however, that this was a case in which the disease was not transmitted from mother to fœtus in the same form, for it did not follow that a mother must transmit the disease to her fœtus in the same quality as she had it herself? She can and does transmit something to the fœtus which is not exactly the same as she has herself. For instance, there was Bidone's case of erysipelas in the last days of pregnancy: the mother had it showing itself in the usual way in inflammation of the skin, but the fœtus was born without any signs of erysipelas in its skin, but it had endocarditis and on the valves of the heart streptococci were found, and streptococci were also discovered in the placenta and in the mother's skin. Could it be doubted that the erysipelas in the fœtus was due to the streptococci, and took the form of endocarditis instead of dermatitis? To carry his argument a little further, it might be maintained that all the things he had named were not *hereditarily* transmitted but by infection; but could any line be drawn between things so transmitted (taking into account the peculiarities of the fœtus and the change in the nature of the handed-on morbid process caused thereby) and the transmission of toxines which did not produce any special disease but simply made the fœtus less well nourished and more likely to take infections?

To go a step further, the embryo might be cited. The modifying effect of environment on the development of the embryo was one of the most clearly established of the phenomena of teratology; malformations could be thus experimentally produced (those at any rate characterised by arrest of development). Toxines of various kinds, hydrocyanic acid very powerfully, nicotine, the alcohols in the degree of their toxicity, these when injected into the hen's egg caused teratogenic effects. In the human subject there was evidence of the clinical kind to support this, for malformations were found more commonly in the descendants of individuals who had saturated themselves with alcohol or had suffered from lead poisoning or other toxic condition or from the infections. Now many of these things were transmitted hereditarily. So he had passed in his argument from small-pox, which no one regarded as transmitted by heredity, through a series of connecting links to conditions such as malformations which were in many cases markedly hereditary in their transmission. Further, the malformations agreed with the neuroses and with gout, rheumatism, and other things, in obeying the same laws in heredity, such as family prevalence, dissimilarity, etc. These laws applied to the various morbid phenomena of antenatal pathology, viz., malformations, monstrosities, prematurity, dead-birth, congenital debility, twins, etc. So the first point in which he differed from Professor Hamilton was that he thought it not possible to draw a sharp line between what everyone admitted to be a fœtal disease (an intra-uterine infection) and a hereditarily transmitted condition.

In the second place he differed with respect to the value of the experiments disproving the transmission of acquired characters. He thought that so far they had not been tackling this subject in the scientific sense, for they had apparently always begun with the idea that the character to be acquired must be acquired after the birth of the individual. That did not seem to him to be the proper scientific way of looking at the subject. The individual lives before he is born, and it may be that experiments failed to give positive results because they (*e.g.* mutilations, etc.) were begun after birth. To test the matter fairly, ought not the processes to be commenced while the embryo or fœtus was still in the uterus of the mother-animal?[1] If this were done, positive results might be looked for. But apart from this there were Kohlwey's experiments on pigeons: he cut off the posterior digit of the foot, and the mutilated bird got in the habit of turning the fourth digit backwards and using it in perching; he got no descendant of these mutilated birds without a posterior digit, but he got a descendant of one of the pairs with its fourth digit turned backwards like the first. The mutilation was not transmitted, but the physiological adaptation to meet it was.

How were we then to get at this problem of heredity, for there was no possibility of avoiding it? Attempts had been made, so to speak, to outflank its position, and the solution of heredity had been sought among the unicellular animals. It had been attempted to circumscribe the field of inquiry to characters transmitted by sexual generation, and the problem had become only more complex. He thought we must just keep on driving straight ahead, passing towards heredity through the series of antenatal morbid phenomena which lead up to it. In that way one would get back to the germ, and by considering in turn the various links he thought that at length a solution might be found. In conclusion he might state that to his mind two of the special problems of heredity were most inscrutable: one of these was telegony; the other was expressed by Montaigne in the sixteenth century, when he puzzled over the riddle of why he should develop a stone in the bladder at the age of forty-five as a legacy from his father who, however, only developed the same thing when sixty-seven years old, or twenty-five years after his son had been born.

Professor STEWART STOCKMAN said he would only take up a few minutes. In the first place, with regard to tuberculosis. Not so long ago, when he was a student, he was taught to go into a byre, look at a cow, and say whether she was likely to become tuberculous. Since the introduction of

[1] As Charrin and Gley have done with blue pus and rabbits.

tuberculin it was now easy to diagnose what are called occult lesions. With the help of tuberculin it had been found that really these cows were not predisposed to tubercle—they had already got it. He had often wondered what the effect would be of submitting the human race to the tuberculin test. He thought a great many of those individuals who were looked upon as predisposed would be found to be really tuberculous.

As regards congenital tuberculosis, his opinion corroborated that of many others. Nearly every week for the last eight years he had gone with his class to the slaughter-house to examine diseased organs and carcasses; and he had been on the look-out, and the inspectors had also been on the look-out, for cases of congenital tuberculosis.

They in Edinburgh slaughter something like six thousand calves a year, and he had found only one case of tuberculosis among them that was undeniably congenital. Of the cows slaughtered, at least twelve per cent. were tuberculous. This was a low percentage, because they were so strict in Edinburgh that most of the doubtful cases were sent to be slaughtered elsewhere. The general average would be about twenty-five per cent., but in some herds it mounts to eighty per cent. of the effective.

Now, if you take the bullocks and heifers, the tuberculous animals do not amount to one per cent. Surely, if even a predisposition were transmitted, it ought to be to the bullocks as well as to the cows. The question was really one of external predisposing causes.

With regard to the so-called predisposition to tubercle, there was one point to which he thought too little attention had been paid—the influence of heredity, not upon the patient, but on the tubercle bacillus itself. The bacillus taken from one species often fails to affect another. For example, they all knew that it was very difficult to give human tuberculosis to fowls, but occasionally a fowl may be inoculated successfully. It has been shown lately that if the tubercle bacillus from a human being be grown in glycerine broth, put in a collodion capsule, and placed in the abdominal cavity of a fowl (the collodion prevents phagocytes acting on the bacillus), it after a time acquires the characters of the avian tubercle bacillus, and in the course of some months you obtain a variety which is deadly enough to the fowl.

He had inoculated horses and donkeys with different kinds of tuberculosis, and others had done the same. The first impression was that the horse was very insusceptible, and the donkey absolutely immune. The horse is now known to be susceptible, though no one had ever pointed out a horse that was likely to become tuberculous; the study of equine tuberculosis dates from the discovery of the tubercle bacillus. If one take the bacillus from the cow and inject it into the donkey, lesions are produced, but these are cured in about a month; and in his first experiments he thought the view was quite correct that the donkey was not susceptible. It happened, however, that one day the tuberculous spleen of a horse was sent him for examination. With material taken from this he inoculated a donkey, and the donkey died of tuberculosis in the ordinary way. He thought that when talking about one species being predisposed and another not, we should remember that if the bacillus does not act on this or that species, it may not be due so much to the species as to something in connection with the bacillus.

As regards cancer, he had never heard it seriously said among veterinarians that it was hereditary. Yet they saw a good deal of cancer. No one, however, had observed a series of cases in respect to its being hereditary in animals. Glanders also was a disease which affected horses. In its course it was rather like tuberculosis. It was now known that it often ran a very chronic course, and that the lesions might remain for a long time occult; but it had never been seriously said that it was hereditary. When he heard Professor Hamilton express the view that there was no hereditary tendency in the case of syphilis, he thought he would like to have it explained why this should be so in regard to syphilis and not with regard to tuberculosis?

Lastly, there was one nervous disorder in horses, termed "roaring," *i.e.* paralysis of the left recurrent laryngeal nerve. It was a rank heresy among the majority of horsey men to say that roaring was not hereditary, and yet there were far more tangible explanations of this affection. The famous horse "Ormond" was a roarer, and his sire was a roarer. He was sold for twelve thousand pounds because he was of a roarer strain. He has been sire to a number of colts in this country. Some have become roarers, others have not. It has been said by veterinarians that in such diseases as strangles, influenza, and suppurative diseases of the throat, the bacterial toxins may produce a condition of the nerves which results in roaring. There are also other good explanations of the condition. In the Argentines, where Ormond first went, it is said that none of his progeny have become roarers. Evidently the hereditary influence plays only a feeble part, if it play any at all.

Dr JAMES observed that it had been stated by a great man many years ago that while philosophers and statesmen were cogitating as to how the world should be governed, hunger and love were performing the task. He thought it very fortunate indeed that humanity out of instinct and common sense should know how to conduct a good life without getting any assistance from scientists in the way of physiology. It was also a good thing for humanity that it could go on reproducing itself to good purpose without scientific aid as regards heredity. He said this because within the last few years he had heard a great deal coming from scientists which was against instinct and common sense, and it would be very bad for humanity were it to adopt these dicta practically. He referred in this connection especially to the statement that acquired variations cannot be transmitted, and to the statements that we have heard lately that much less importance should be placed upon heredity than common-sense people are inclined to allow.

Of course they knew that according to the Weissmann theory acquired variations could not be transmitted. But although Weissmann had begun by saying this, when they looked into all that had been written by him subsequently, they could not but conclude that what he had said after all was nothing very different from what had been said before him. Weissmann seemed to regard a variation rather as a development of something that was there before, than as the occurrence of something new. But is it not the case that evolution means simply the better adaptation of the organism to surrounding conditions? Hence in every generation a new step in progress is, as it were, made. Dr James remarked next that he was not going to follow Weissmann in his metaphysical processes, but he would just quote a sentence which he remembered to have read in one of Herbert Spencer's books. At least he thought he had seen it there. This was that, amongst living beings, the conditions which favoured growth and development were precisely the conditions which, given time enough, would bring into existence.

As regards examples of the non-transmission of acquired variations which had been brought forward, he could not help saying that to imagine they proved anything was simply childish. For example, as regards mutilations.

Certain tribes of Indians had for years, by pressure in early life, flattened their heads. It was said, "Why was not this perpetuated?" The Chinese women had for years distorted their feet. Why was not this perpetuated? Well, in the first place, the years during which those mutilations had been going on were as nothing compared to the duration of life of human beings on the surface of the globe. But further, and he thought that this was the better answer, Nature was not a fool, and Nature, in order to enable human beings to rise in evolution, brought about the elimination of the unfit as well as favoured the survival of the fittest. Nature was not such a fool as to perpetuate flattened heads and distorted feet, but had eliminated or would eliminate the people who practised those mutilations.

Brown-Sequard's experiments on the epileptic guinea-pigs, and the observations on pigeons which Dr Ballantyne had quoted, were, if corroborated, of value as against the Weissmann theory. But Dr James contended that laboratory and experimental work were not likely to give valuable data in connection with the transmission of acquired variations. At any rate, as yet he held that they had not afforded much information. On the other hand ; Dr James held most emphatically that doctors in practice had much better opportunities of forming an opinion on this point. Doctors had opportunities of examining for themselves, and of tracing variations in a way that the laboratory scientist never had. He remembered hearing it said by one of his personal friends that, at a recent meeting of biologists, all had declared in favour of Weissmann's theory, except a few doctors. His contention was that the doctors were probably the individuals best capable of judging.

He wished now to explain what he meant.

In medicine it is an old idea—and he thought there was a great deal of truth in it—that the immunity which an individual acquires as the result of having a disease such as typhus, scarlet fever or small-pox, may be transmitted. Personally, he could say that the most malignant examples of small-pox and scarlet fever which he had seen were in the Edinburgh Infirmary, in people who had come in from the out-lying districts of Scotland, where these diseases had not been seen for generations.

As regards syphilis it had often struck him that some of their surgical friends have opportunities which should not be neglected. A man has had syphilis and has been carefully treated. His offspring do not get syphilis—but are those offspring less easily acted on by the syphilitic virus than ordinary individuals, or are they not? We know if a man has acquired syphilis—though the wife may not get the disease—his child may be born syphilitic. The mother in this case is immune (Colles' law), and may not such immunity be transmitted?

Again, do those individuals whom we see presenting the marks of congenital or hereditary syphilis take the disease if exposed to infection in as virulent a form as those who have not had it in this way?

Next, as regards alcoholism. Dr James was strongly of opinion that alcoholism in a parent produced diseases of a good many kinds in the children in this country. Of course it might be said that the alcohol which the parent had taken had simply brought into prominence what was there before. He was not going to enter into this precisely, because he believed it could do no good, but he held that whether the predisposition to alcoholism was in the parent or not, the practical indulgence in it was very injurious to the offspring. For example, he had, in looking up some of his case books, gathered together some statistics in connection with two diseases, epilepsy and phthisis. In order to limit himself to heredity, he had taken only the epileptic cases under twenty years, and the phthisis under thirty. He found, curiously, that a phthisical family history prevailed in the same proportion in the epileptic as in the phthisical cases. But he had no hesitation in saying that in a large number of the epilepsy cases in children in which it was stated that the parents were quite healthy, one parent, usually, of course, the father, was often found on inquiry to have been alcoholic. Again and again he had found that an alcoholic parentage with perhaps a fall or an injury to the head in childhood, had produced epilepsy, and he looked upon the epilepsy in such cases as an evidence of the transmission of an acquired variation.

Another disease which he had looked into was gout. An insurance friend of his had once informed him that the heredity of gout had been much over-rated, because it had been found that individuals who had been rated up for a family history of gout, had proved to be specially good lives. Now, at first sight this might seem to detract from the importance of heredity, but when we look at it properly, it proves rather how prone we are to draw false conclusions. That individuals with a family history of gout should often prove to be the best lives we can quite easily explain. A gouty heredity means specially good digestive and assimilative powers; and specially good digestive and assimilative powers very often means that organic sensation of well-being which is apt to lead to excess. As Sydenham said long ago, "More wise men than fools have suffered from gout." Now, the men who insure their lives are usually men who have, and can exercise self-control. Hence a gouty heredity in connection with insurance means specially good digestive and assimilative powers, plus the self-control that prevents excess. The statement, then, that heredity is of no importance, because lives rated up for gout often prove the best lives, is a false one. What we should say is that heredity is important, but that we must make certain that we are interpreting its data correctly.

Professor SCHÄFER said he would content himself with saying a word or two on certain special points on which he had been appealed to. With reference to the complexity of the nerve-cell of course he agreed entirely with Dr Clouston as to its complex structure, but that fact he thought rather accentuated Professor Hamilton's position. No doubt with the early maturity of the nerve-cell it was hardly conceivable that it would transmit any acquired character. He was quite sure that Professor Hamilton would admit that the discussion was to a certain extent a groping in the dark, because we were completely ignorant as to the causation of disease except such diseases as were due to parasites. He thought he was right in saying that we knew nothing as to the actual causes of other diseases. It was, therefore, very difficult to say what they were dealing with when they talked of heredity in disease.

One word with regard to Dr Ballantyne's proposal to make experiments on the fœtus *in utero*. Of course he must be aware that such experiments have been and are being made at the earliest possible stages of development. With frog embryos, for instance, many experiments have been made on the first cells produced from the egg. (Dr Ballantyne agreed that this was being done: he understood with positive results as to the transmission of acquired characters.) The speaker only wished to point out that there was no difficulty in experimenting upon the earliest stages. As to the guinea-pig experiments of Brown-Sequard, he might say that, at the suggestion of the late Mr Romanes, Dr Leonard Hill made a certain number of similar experiments in his (the speaker's) laboratory in University College, but so far as they went it was not found that the guinea-pigs in which this neurosis had been artificially produced transmitted it to their offspring.

Professor HAMILTON, in reply, said he was sure that Dr Clouston must have had a large experience of cases bearing either for or against the question of the acquisition and transmissibility of mental diseases, and from the tenor of his remarks he was led to believe that he favoured the view that mental disease, in the ordinary sense of the term, may arise from external agencies and be transmitted. With regard to the Jewish types, he was quite right—anywhere on the Continent you might see different types of Jews—but anyone who had had sufficient experience could pick out these to be Jews. One inherited Jewish characteristic is the mental proclivity to commerce which has continued for all time. There are also bodily characteristics in the way of the thick under-lip, and a peculiar conformation of the legs which mark them off.

With regard to Professor Cossar Ewart's remarks, he was pleased to find that, looked at from a biological standpoint, what he had said had been so orthodox ; he had been prepared to hear his ideas characterised as extremely heterodox.

Referring to Dr Ballantyne's remarks about congenital tuberculosis, there might be an element of truth in what he said. It was a well-known fact, which he had verified all his pathological life, that a person suffering primarily from valvular disease of the heart seldom, if ever, died from pulmonary tuberculosis. What the explanation was he did not know.

There was certainly a difficulty in separating the congenital from the hereditary, but he thought we might say that a condition is hereditary when we find it pass through generation after generation; not merely through one generation, or even a couple, but where the tendency is inherent, like the colour of the hair, or shape of the nose, though perhaps skipping a generation now and again. Of course, the infection of a child with syphilis while in the uterus is not heredity, and he thought that perhaps Dr Ballantyne might have misunderstood what he had said regarding this.

Touching upon the early experiments *in utero* advocated by Dr Ballantyne, Professor Schäfer had in a manner anticipated what he had to say in reply. The beautiful experiments on merogony carried on by Roux and Delage upon the ova of frogs and echinoderms might be mentioned in this connection. These experiments went to prove that the ovum might go on developing into a perfect larva, even though a number of the early blastomeres had been removed; it was only occasionally that any deformity was met with. Even this radical method of applying external agency had comparatively little influence on normal development.

Professor Stockman had contrasted the difference in frequency of occurrence of tuberculosis in cows and bullocks. He would point out that cows were under different conditions from bullocks, which, it might be, predisposed the former to the infection. The cow was kept in a warm, close, stuffy atmosphere, while the bullock might be, for a considerable time, in the open; and at the same time, the cow, through excessive discharge of milk, gives off from its body a large amount of nourishment likely to impoverish its system.

On the alcohol question in relation to insanity, there were no doubt a number of fallacies. We might group with the alcoholic habit the tendency to criminality and to insanity, and regard them as manifestations of the same dyscrasia. He thought the tendency to alcohol might be looked upon as an effect rather than a cause of insanity.

Note.—The foregoing reports were corrected in MS. by the various speakers.

Undelivered Speeches

1. By Dr Wilson, Mavisbank.—For the sake of brevity one's statements must appear dogmatic; but our special effort ought to be to keep an open mind on this question. Most of our difficulties are of our own creating, because we have not taken care to translate biological propositions in terms of physiology and of pathology.

The idea of continuity of germ-plasm is not strange and it is not relevant. All living substances are as "continuous" as the germ-plasm until they cease to be; that is, they come in direct line from ancestral substance. The essential substance of germ-cells, however, is continuous forwards as well as backwards and (on paper) is *ad infinitum*. Weismann contends that somato-plasm, the body substance, is not so, that it does not "continue" into our offspring. That may or may not be true —a question which we may waive for the present. This perpetual idioplasm idea is beside the point. The relevant idea propounded by Weismann is, that the germ-plasm which organisms convey in their germ-cells is physiologically secluded; that it is essentially immune to its environment. The germ-plasm, we are taught, undergoes no essential metabolism until the time of maturation. That *may* be so. The characteristic energy of germ-plasm *may* be quite latent for a time. It *may* be that all that germ-plasm requires for its preservation is moist warmth, just as wheat is latent for months in dry warmth. And it *may* be that, every little while, germ-plasm maturates automatically and becomes manifest in the kinetic energy of the spermatozoa and of ova. All these propositions are still open to grave question. But, at all events, this notion of physiological isolation of the germ-stuff, and of a periodic metabolism in it of an automatic kind, is quite apart from the idea of "continuity."

Weismann admits, and everyone must, that disease has some effect on germ-plasm. The katabolism of somatic death stops the germ-stuff conveyed in our bodies. And death is a relative term. Short of complete somatic death, various degrees of change occur which affect the germ-plasm—degrees of hyperpyrexia, for example, and degrees of toxicity of lymph. But we must state the question at issue more exactly. Physicians have been at fault in not discerning variations in disease. For example, when a man with tabes begets a child with epilepsy, the physician is too ready to speak of the hereditary factor in the child's case. Many similar instances will suggest themselves—cases, not of heredity, but of variation in disease. This idea is important. Diseased organisms are apt to breed disease, but not always, though sometimes, their own disease.

We must relinquish the idea of a hard and fast line between "acquired" and "idiopathic" diseases. All diseases are to some extent "acquired"—occasioned as reactions to environment. In some cases the agencies within the organism are of more obvious importance than in others. Let us take a case in which both factors are important—a case of tuberculosis. To obviate the fallacy of maternal infection, suppose the disease to be of the avine variety, and that the yolk is not infected. Assume that the last egg of a fowl dying from tuberculosis is fertile. Weismann would admit—everyone would—that the chick is likely not to be full-grown and robust. It will fail of "nutrition," of a full capacity for regeneration, and of normal resistiveness to environment (terms which require fuller consideration). It would appear then that this chick has an idiopathic susceptibility to all and sundry, or at least to several, diseases. But it is mere slackness to call that heredity in disease. It is equally apt to be variation; the chick turning out to be epileptic, or deformed, or liable to cholera. That is all that Weismann contends for. The disease has not bred itself. And when we come to consider human diseases, and have regard to all the possibilities of infection, we must admit that there is no proof that "acquired" diseases ever become "idiopathic." Yet, with others, I venture to believe that the last word has not, by a long way, been said upon this subject. Idiopathic diseases are, for the most part, due to premature or focal arrests of development, or excesses of development or of growth, or senility or involution in the tissues. And we may with reason hold that it is *not proven* whether, for example, an arrest of cerebral development by alcoholism does or does not induce a similar arrest (among others) in the offspring. The idioplasm of the germ-cell nucleus, as Weismann conceives it, is a molecular substance. The exigencies of debate make it necessary for biologists to whittle away the conception, and it is a fair retort to say that they are hiding a theoretical unit. Weismann would himself admit that the germ-plasm and its determinants are mystical. All delimitations of tissue, including the conception of the cell as unit, are arbitrary. There is no permanent truth, but merely convenience, in the conception of a unit and its environments. The whole organism and all nature are but parts and the relations of parts. If we push the biologist to extremes, he must eventually define the germ-plasm as that substance (in or near the germ-cell) which is independent of its surroundings. That is simply to conceive a substance as capable of what the argument requires. But physiologists will be slow—and philosophers will refuse—to entertain the conception of a living substance which has no relations.

The invaluable contribution of Weismann to the practice, as well as to the theory, of medicine still remains—that we must look to the environment, and to the reactions, of the idioplasm to explain disease and to prevent it. That should be part of a 3
general conception. Taking for the moment, any one organism as unit, its environment is undergoing selection, and is evolving, much more obviously and rapidly than its idioplasm. It is in the selection and in the evolution of environment that the future of medicine lies.

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2. By Dr W. LESLIE MACKENZIE. — Professor Hamilton's paper appeared to me an admirable application of the doctrine of natural selection to the problem of inherited disease. On one point—telegony—his propositions outran the evidence; for Professor Cossar Ewart has shown, in the Penicuik experiments, that the supposed proof of telegony does not stand analysis, and experiment has not yet furnished a single confirmation. The discussion, it seems to me, somewhat lost sight of the precise issue so clearly raised by Professor Hamilton, namely,—Is a disease that has been acquired by the individual capable of being transmitted as such to his offspring? Toxic diseases, which are capable of infecting the germ-plasm directly, are, of course, irrelevant to the question of inheritance. Why is there any difficulty in answering the question? Mainly because the two—or more—sides to the dispute did not make explicit their fundamental assumptions. Both sides, I think, admitted the abysmal difference between a somatic-cell, which is capable only of producing its like, and a germ-cell, which contains in itself the elements of the whole body. No one seriously disputed the propositions that the material of the germ-cell, more properly the germ-plasm, is continuous directly from person to person, and that the non-germ-plasm is not thus continuous. Further, it was assumed that the germ-plasm is the bearer of all that the child inherits from the parent. Once more, it was assumed that all the characters existing in the germ-plasm that the parent sprang from may be transmitted to the offspring; that some of the characters may lie latent; but that, on account of the continuity of the germ-plasm, these may be handed on to a third and a fourth, and an nth. generation. Then all appeared to assume that if the germ-plasm varied in any way, natural selection will at once operate to develop and preserve useful variations. One assumption, however, did not seem to be constantly present to the minds of the disputants, 4
namely, that there is a fundamental distinction between a

variation (however produced) in an individual's germ-plasm, and a modification (produced by use, or environment, or disease) in the body (that is, the non-germinal-plasm) of the same individual. The variation in the germ-plasm will result in the production of a new character in a new individual springing from that germ-plasm. But a modification due to use, or environment, that is, a character acquired by the individual, will not be transmitted to his offspring unless it first produces a definite variation in the germ-plasm. Darwin, assuming as a fact that characters acquired by an individual were transmitted, invented pangenesis to show how the transmission was possible. But Galton found reason to believe that pangenesis, that is, concentration of representative gemmules in the germ-plasm, was mythical, and also found reason to doubt whether acquired characters were inherited to any but an extremely small extent. Weismann drove the analysis further, and not only showed that many acquired characters supposed to be inherited were not inherited, but that the thorough-going application of natural selection made the hypothesis quite unnecessary. Many difficulties exist on both sides; but in the problem of hereditary disease, it is, I submit, illegitimate to assume that the inheritance of acquired characters has been proved. On the contrary, it remains entirely unproven. So far, experiment has failed to make it even predominantly probable. By "acquired" is to be understood "acquired by the parent during his own life-history." From conversation, I gathered that some understood "acquired" to mean acquired by the germ-cell. The germ-cell, or germ-plasm, is an organism within the body-organism. Like cell organisms, it depends on its environment (the body) for nurture; but nurture affects function and growth, not structure.

Consequently, it is a very "cheap" and, only in appearance, a very simple assumption to say that any structural modification in the germ-cell's environment (the body) will create in the germ-cell such an alteration that when the germ-cell grows to an adult, that adult will have the same structural modification as the parent. But to judge by the instances produced at the discussion, the inheritance of modifications acquired by the parent is a superfluous assumption. In speaking of Weismann's "admissions," Dr Clouston left on one the impression that he considered the admissions somewhat inconsistent with the fundamental assumption of the continuity of the germ-plasm from generation to generation. But when Weismann admits the "transmission of disease," he means such diseases as may infect the germ-plasm; but, as Prof. Hamilton showed, the direct infection of the germ-plasm by a poison circulating in the body is quite irrelevant to the question of inheritance. Weismann would not, I take it, admit that (say) cardiac hypertrophy due to over-work would produce congenital cardiac hypertrophy (or even a tendency to it) in the child. Of course, the constitution that made the father liable to hypertrophy would also make the child liable, but this is inheritance of a constitutional (non-acquired) character—a thing no one disputes. Then "the laws of transmission do not affect the lowest forms" is said to be another admission. I assume the general accuracy of the statement. The explanation is simple: in the unicellular organisms there is no division into germ-plasm and body-plasm. There is, consequently, no heredity in the sense applicable to higher organisms. In the lowest forms the whole body of the parent divides into two daughters. But, in the higher, only a minute fraction of the organism is set apart for reproduction.

Again, a third admission is that "the environment, as climate, has the power to alter germ-cells." Prof. Cossar Ewart corrected this statement, which certainly does not completely represent Weismann's doctrine. The word "alter" is ambiguous; it may mean alter functionally, or structurally. The germ-cell is, of course, affected by its own environment, as I have said above; but Dr Clouston seemed to indicate his belief that climate might produce in the germ-plasm directly elements that did not before exist in it, so giving rise to new variations in the germ-plasm. This proposition needs a vast amount of proving. I have not found Weismann maintaining it, but he admits that, in certain butterflies, climate may stimulate the growth of particular elements of the germ-plasm—a very different thing. None the less, Dr Clouston is, I think, right in concluding that Weismann's hypothesis includes everything essential to the explanation of hereditary insanity. I felt, however, that in speaking of the "royal family insane for three hundred years," Dr Clouston seemed to suggest the inheritance of acquired characters. Did he imply that the "psychopathic constitution" inherited by this unfortunate family was first produced in an ancestor three hundred years ago, wrought somehow—by pangenesis, or other unexplained mechanism—into the structure of his germ-plasm, and so transmitted to his descendants? If yes, then I say this begs the whole question. If no, then the "psychopathic constitution" was not an acquired character, but a character already existing in that ancestor's germ-plasm. He transmitted the constitution he was born with; but that brings us no nearer an explanation of how he came to be born with it. But, incidentally, the stability of this dyscrasia in the germ-plasm for three hundred years on the whole confirms Weismann's doctrine of the continuity of the germ-plasm. Again, Dr Clouston strongly maintains that many of the insanities are manifestations of arrested development. But Weismann never denied the possibility of arrested development, which may arise from a thousand causes other than inheritance of acquired characters. But Dr Clouston naturally asks why so many bad variations occur in the same family? This problem of the occurrence of variations that have selective value is tackled by Weismann in his "Germinal Selection," and most of the difficulties seem to me better correlated by that formula than by the doctrine of use-inheritance. In any case, even if use-inheritance were accepted, how can the transmission of so useless a variation as insane neurosis be accounted for? Dr Clouston says he does not believe that the child of an alcoholic parent is ever born with a taste for whisky; but he may be born with a lack of inhibition. But such lack of inhibition may very well have existed in the parent's constitution before alcoholism asserted itself; and, in any case, the nutrition of the germ-cells may be interfered with by the alcohol circulating in the parent's body, and germinal decrepitude may thus be produced. It is even *possible*, theoretically, that the nerve-elements of the germ-plasm (the nerve-determinants) may be so affected by alcohol that they grow into imperfect neurons or nerve-cells. But here again it is a case not of the transmission of characters acquired by the father, but of the effects of alcohol imbibed by the germ-cell itself. The case is on the same footing as a case of syphilitic infection, with this difference, that the poison of syphilis is a specific infection. The results of infecting or poisoning the germ-plasm may be anything from a temporary "illness" of the germ-cell to complete disintegration and death. The poisoning of the germ-plasm, however, has nothing to do with the transmission of a modification acquired by the parent. Other illustrations by Dr Clouston seemed equally explicable from Weismann's standpoint. He produced no case that contradicts the continuity of the germ-plasm or necessarily involves the inheritance of acquired characters. On the contrary, his cases are more simply explained without that assumption.

Dr James' precise position I was unable to grasp. I gather that he asserted the inheritance of acquired characters as too obvious to need discussion. But his illustrations—alcohol, phthisis and epilepsy—did not bear out his argument. Professor Hamilton's suggestion that these—and others—are all parts of the same constitutional dyscrasia seems to be admitted by Dr James. Why he should burden himself with the obscure and extravagant hypothesis of use-inheritance, I do not understand. Dr James says medical men have much more opportunity than biologists of knowing whether acquired characters are inherited. Possibly, but the characters are usually too complex for analysis. If they are inherited to the extent he assumes, cases ought to exist by the hundred thousand. It is somewhat disappointing that, with the single exception of Brown-Sequard's rough and inconclusive experiments on guinea-pigs and Dr Ballantyne's illustration of a possible case in pigeons, not one clear and criticised example of use or mutilation-inheritance was brought forward by any speaker. There are many examples much more plausible than Dr James'. The only definite biological facts produced (by Professor Ewart) told quite distinctly against such inheritance. As to Dr James' logical canon that what contradicts "common sense," and "common instinct" has something wrong with it, one is provoked to ask, with Cyrano, "*Que diable allait-il faire en cette galère?*" The discovery of the barometer, the application of the laws of gravity to the solar system, the doctrine Galileo was tortured for, the conservation of energy, natural selection, and many other great inductions, contradict common sense, which plays a very small part in the solution of any difficult and delicate problem.

Dr Ballantyne's remarks on "maternal impressions" are valuable, because he has pretty well done for that department of uncritical belief what Weismann did for the alleged inheritance of mutilations. Yet the so-called evidence for such impressions seems to me frequently stronger than the evidence for inheritance of acquired characters. When, however, Dr Ballantyne suggested antenatal mutilation for experiment, as more hopeful, he gave the general case away; for the acquired characters alleged to be transmitted are not the simple conditions induced by the very primitive environment of the uterine fluids, but the immensely delicate and complex integrations, dexterities, ideas, etc., developed, or *de novo* fashioned, in response to the complex environment of actual life. The case of Montaigne's calculus, which supervened at the same age as his father's, is an excellent example of the appearance of a germinal peculiarity in two branches of the same tree. Professor Schäfer's very important remark should give the "medical transmissionists" pause: infections are, by hypothesis, ruled out, and of the ultimate causation of other diseases we know little or nothing. For practical ends, pathology is an organised department; but biologically, disease is a *plus* or *minus* variation or modification, sometimes having selective value, sometimes not. Personally, I should be glad to believe, with Dr James, that much of these refinements in distinction are merely verbal, were I not persuaded by daily experience that the nightmare of the specific inheritance of acquired diseases overloads the spontaneity of life, paralyses the will, and hampers the preventive service in its efforts to improve the environment. Weismannism exalts the social inheritances, which, as the great organs of selection, constitute the basis of preventive medicine. But there are no data for a dogmatic conclusion on the modes of hereditary transmission of acquired or non-acquired characters.

Remarks by Dr GRAHAM BROWN.—It appears to me that the discussion turns very largely on a play on words. What do we mean by acquired characteristics? Do we mean such as were not present in the ancestry of the person in question? If we do mean some abnormality or divergency from the normal type, some alteration of tissue which may perhaps only show itself by an increased susceptibility to the action of external agency, then I am firmly of the belief that such acquired characteristics may be and often are handed down from father to son for generations.

Take, for example, the case of rheumatism. No one who has had any opportunity of studying this disease, and of acquiring a knowledge of the facts concerning it, can deny that three points at least are clear and well established.

1. That it prevails very much more in some families than in others.

2. That there is no evidence to show that it passes from person to person by infection or contagion.

3. That it is of the nature of an infective complaint.

Taking these points as admitted, it follows, to my thinking, that the occurrence of rheumatism in certain families can only be due to the transmission of some peculiar susceptibility of tissue from father to son.

Or take the case of alcoholism. As Dr Clouston has so admirably pointed out, an increased susceptibility to the action of alcohol (apart altogether from the alcohol craving) is noticed in neurotic families. This unusual vulnerability of the tissues to the action of alcohol can only, so far as I can see, be an acquired characteristic. I have certainly seen it in process of being acquired. That is, I have seen men who at first were able with impunity to take a fair quantity of alcohol who subsequently became alcoholic, and who having broken off the habit, found that in future even small quantities were apt to produce disagreeable results. The onus of proof lies on those who hold that these two forms of transmitted susceptibility of tissue are not acquired characteristics.

Remarks by Dr WILLIAM RUSSELL.—In the first place, I wish to express the pleasure it is to me and to many others here to see Professor Hamilton amongst us again. In listening to the discussion it has appeared to me that we have suffered from a want of definition in the terms used, and also a want of concentration on what seems to me to be the real point implied in the title of the opening paper. The existence of heredity is not questioned, but we have a biologist like Professor Cossar Ewart speaking as if nothing were to be regarded as heredity, unless you could count it or figure it as number or arrangement of skin markings, the colour of feathers, or the number of them in a pigeon's tail. We are all interested in Professor Ewart's experiments, but most of them have no special bearing upon the question under discussion. The heredity which manifests itself in cross-breeding has only a general bearing on the influence of heredity on disease. We all know of curious hereditary anatomical characters in families: I know a family where webbing of fingers or toes prevails; but all that class of thing is not disease any more than are the colour of the eyes or the hair, or the size of the hands or feet. Another biologist tells us that we do not know what disease is; this is a strange statement, for those of us who deal with disease would say that in diseases not belonging to the infective series it begins as disturbed function, and the limit at which that disturbance begins is determined by the vigour of the cells of the part or organ affected. In the common matter of gastric digestion this is true; it is equally true of heart muscle. This is practically the same point as Dr Clouston so ably elaborated; it is the attainment of a "maturity" which is on a level at least with the average. This maturity is functional vigour. And will the biologist tell us that he knows an anatomical distinction which will enable us to say that one stomach is $x-$ and another $x+$? And because he cannot do this are we, as physicians and pathologists, to ignore this whole realm of observation as if it were a chimera, a mere dream? If biology is not prepared to take this teaching from us, it is doubtful what practical value biology can be of to us. I unhesitatingly range myself alongside Dr Clouston and say that the future of medicine depends more on this class of observation than on variations in pigeons' tails. The question before us seems to me to be this: (1) Can the chemico-vital activity and vigour of cells be influenced by the environment of the individual; can it be lowered, or modified, or altered? (2) Does that lowering or modification in any way influence the offspring along the same lines? There is surely only a positive answer to these two questions. Doubtless this modification may take generations to become fixed, for fortunately the *vis naturæ* tends to strengthen the weakened and to correct what has been modified, if the conditions are rendered more appropriate, more physiological, more hygienic; call it by what name we like, it means a favourable environment, and includes, as Dr Clouston has said, climate, food and drink. The variations and subtleties of the chemico-vital composition of individuals is vulgar knowledge to those of us who deal with deviations from the normal in living human beings. Take some examples of this. Some people are phenomenally vulnerable or susceptible to infective influences, while others are the reverse.

I knew a surgeon who could not make a *post-mortem* examination, or operate on a septic case, without getting poisoned. Many men work for years in the *post-mortem* room, and handle all things freely and are never poisoned. I know a medical man who took infective diseases whenever they became prevalent. He had, I believe, scarlet fever and typhoid several times; other medical men never have had either. Take what we call idiosyncrasy, even in the matter of food. I know a leading physician to whom a certain white fish is an emetic; to an old gentleman whom I have seen professionally, egg, in the smallest quantity and in any form, has always acted as an irritant poison, producing vomiting and diarrhœa; in another case a couple of strawberries produces a general urticaria. Passing from food to drugs, I once made a man as red as a lobster from head to feet by a small hypodermic of morphia. Within the last year I produced in an adult male private patient severe diarrhœa by giving him one or two one-drachm doses of the camphor water of the pharmacopœia. I presume we all meet these curious phenomena. Then if we turn to dyscrasia, are there no rheumatic and gouty people? Do we not recognise that these people have a modified vital chemistry which, under conditions to us harmless, manifests itself by what we clinically know by these names. In all these regions we are face to face constantly with what Professor Ewart would call an "epidemic of variation," not of coarse and palpable anatomical change, but of chemico-vital composition, which neither the biologist nor the physiological chemist can express even in symbols. Because they cannot do this, are we to regard this whole aspect of life as unreal, and existing only in our medical imaginings? I do not think we can too strongly repudiate any such suggestion.

There is certainly variation enough; how much of it is inherited, how much due to blends between male and female not physiologically successful, and how much acquired, are questions we cannot always answer. That acquired characters can be propagated there is strong proof. It is generally accepted that the two commonest infective diseases in this country, namely scarlet fever and measles, are much less virulent than they used to be, and this is almost certainly to be attributed, not to an attenuation of the virus, or to improved treatment, but to a measure of immunity acquired by a population whose progenitors for generations have passed through the ordeal of these infections. It is a historical fact that measles introduced into a virgin soil, unmodified by a degree of immunity acquired by transmission, proves a most virulent and fatal malady. Another historical instance is that of the company of Esquimaux who, taken to Berlin, were vaccinated to prevent them taking small-pox, and they nearly all died from the modified virus, used without any risk for protective purposes by the ordinary European.

We can now pass to Professor Hamilton's proposition that there is a hereditary anatomical type prone to certain definite diseases, and he illustrated this by reference to the general body type and the chest forms in persons liable to pulmonary tuberculosis, and which are well known. But this type and form constitute but a very small proportion of cases of pulmonary tuberculosis; and even where it does, the question is: Does not acquired tubercle determine the anatomical type in the offspring rather than the other way about? Even granting that the individual factor which allows entrance to the tubercle bacillus be an epithelium of low resisting power in the respiratory tract, is it for a moment to be allowed that that enfeeblement cannot be induced on the one hand, or strengthened on the other hand, by environment? Every physician says "No" to this, and the facts are so conspicuous that the matter need not be argued. While this enfeeblement of epithelium may be postulated and accepted as a working hypothesis, let us be perfectly clear in our own minds that this is not an anatomical distinction. No biologist or pathologist can say of a bronchial or alveolar epithelium that it has anatomical characters which distinguish it from another. We cannot move without postulating chemico-vital modifications and modifications of metabolic vigour which determine the beginnings of disease. I wholly believe with Dr Clouston that these modifications are acquired and transmitted. At an institution in this city I see a great many habitual alcoholics, and our inquiries show that in the greater proportion of them there is a marked family history; but the cases I see differ from those which come into Dr Clouston's experience, for comparatively few of them belong to the neurotic type. In my experience there is a periodic craving which the victims will do almost anything to satisfy, and which is a pathological condition only stateable as a chemico-vital perturbation, probably in nerve endings, which is transformed into conscious craving in cerebral cells. This tendency must be acquired, unless it is to be traced back to Noah; and even were this possible, the hereditary modification is not present in all his descendants, and must therefore have been eliminated in some by evolution. This morbid reaction to alcohol can, it seems to me, be, without any question, acquired, and it is as certainly transmissible. The same line of observation and of reasoning can be followed in rheumatism and gout —a vicious chemico-vital variation is transmitted, and most of us, I think, assume that the acquirement of this variation need not be thrown many generations back. The transmitted variation determines a great variety of anatomical change and of clinical phenomena, but the vitiated chemistry is none the less a real inheritance, because the coarse anatomical changes are not congenital, but supervene in adult life. To me the whole 7 8 question of the influence of heredity in disease centres in and radiates from this point of acquired and transmitted chemico-vital variations in cells, tissues, and organs.

Meeting VII.—May 2nd, 1900

MR A. GORDON MILLER, *President, in the Chair*

I. EXHIBITION OF PATIENT

Mr J. M. Cotterill exhibited a patient after LAMINECTOMY FOR PARALYSIS of two years' standing, the result of spinal caries. The symptoms of spinal mischief began when the patient was æt. 36, and signs of pressure on the cord developed rapidly. For six months before the operation there had been complete motor and sensory paralysis of the lower half of the body. There was incontinence of urine and fæces, and two large bed-sores had appeared on the buttocks. There was also cystitis. As counter-extension had given little relief, operation was decided upon, and the laminæ and spines of the eighth, ninth, and tenth dorsal vertebræ were removed. It was then found that pressure was due, not to granulation tissue, as was usually stated to be the case, but to exostoses springing from the bodies and arches of the vertebræ, which narrowed the spinal canal down to the diameter of a large quill. Improvement was manifested sixteen days after the operation, and the patient progressed steadily until he could now (a year after the operation) walk about with the aid of two sticks, and had complete control of his bladder and bowel.

II. EXHIBITION OF SPECIMENS

Mr J. Shaw M'Laren exhibited (*a*) a specimen of CARCINOMA OF THE ŒSOPHAGUS, and the stomach from the same case after gastrotomy had been performed. The patient had recovered and survived the operation some months. Considerable leakage had occurred for a week or two, though Witzel's operation had been performed. Mr M'Laren had not met with this before, but it was accounted for by the fact revealed at the *sectio* that the attempt to form an "artificial œsophagus" had failed in some way difficult to explain. The operation had been performed under eucain anæsthesia. The œsophagus was narrowed by the cancer, the tracheal mucous membrane