

week: (1) that the nutritive value of milk is largely diminished by boiling; and (2) that this is also true of sterilised milk? In regard to the case he quotes he does not say whether the "anti-scorbutic food" which caused the child's recovery from scurvy consisted *solely* of unboiled milk or whether anything else, which might have been at any rate partially responsible for the recovery, was included. Again, the absence of tuberculous disease from infection by a milk-supply, which is probably from a carefully selected source, in the boys of a "large public school" where hygienic conditions are so good and outdoor life so universal, is no very strong argument against its transmission to the less fortunately circumstanced children of our large towns.

I am, Sirs, yours faithfully,

June 28th, 1901.

M.B., CH. B.

THE CONTAMINATION OF POST-OFFICES.

To the Editors of THE LANCET.

SIRS,—As Parliamentary secretary to the Postal Telegraph Clerks' Association I shall be grateful if you can give me a little space for the purpose of explaining the motives which prompted the members of my organisation to ask Sir W. Foster to take charge of our clause in the new Factory Bill. We have no desire to attack the medical department of the Post Office, but we are anxious to secure sanitary conditions in the telegraph offices. The tone of the annotation in *THE LANCET* of June 29th seemed to convey the impression that you concurred in the view that improvement was necessary, but that the present system of inspection is sufficient to meet the needs of the case. But the question that arises is, Has the present system succeeded? Let the Inter-Departmental Committee (Tweedmouth) answer. It made inquiries at 182 offices and as a result admitted that 69 were in an insanitary condition. Defective ventilation, bad drains, and urinals and water-closets near dining-rooms were the main charges against the Post Office and they were admitted. If the system was satisfactory how came it that a committee of inquiry such as this could report, "Much still requires to be accomplished before the post-office buildings of the country will be placed in a universally satisfactory condition as regards their sanitation?"

Two instances will suffice to show that independent investigation is needed. The staff of the General Post Office, London, had complained of insanitary conditions for a long period, but they appealed to deaf ears. When, however, the Tweedmouth Committee visited the building and Dr. W. H. Corfield was asked to make a special report the building was condemned as "overcrowded, incommodious, and insanitary." The staff of the Chief Telegraph Office also complained of insanitation, unsafe exits, and overcrowded dining-rooms. Again the official reports denied the existence of the grievances. After much stir in the public press, many questions in Parliament, and a personal inspection by the Duke of Norfolk, it was found that most of the charges were true and alterations involving a great outlay were made.

The Tweedmouth Committee laid down several "canons" for the guidance of the officials. Among others were: a due amount of cubic air-space; that all outside doors should open into porches or other like arrangement and never directly into the rooms where the staff are at work; that heating should be by hot-water pipes or hot air; that lavatory accommodation should always be supplied and that they should never communicate directly with any retiring or other room; and that having regard to the dusty nature of the work of a post-office the walls, &c., of all offices should be more frequently washed and cleansed than at present is considered necessary.

My association collects facts regarding telegraph offices only, but Sir W. Foster and Sir C. Dilke have been supplied with 50 foolscap sheets of typewriting containing details of cases in offices throughout the kingdom where practically nothing has been done to give effect to these "canons." I am aware that the fault is said to rest with the Office of Works, and it was stated in 1897 that it should be "held to be imperative on the Office of Works to carry out all the demands concerning any local insanitary condition made by the medical officers through the correct departmental channel." Mr. Balfour assured the House of Commons on June 7th that every one of the Tweedmouth recommendations had been carried out, and if he is right then the Office of Works naturally obeys this "imperative" injunction.

But telegraph clerks cannot penetrate into the official secrets. We know that insanitation exists and we want it

remedied. At present we have but two methods: (1) by means of a petition; and (2) by drawing Parliamentary attention to the case. Both are cumbersome and unsatisfactory and are a source of constant friction between the supervisory officials and the ordinary staff. We also know that new post-office buildings are not constructed with a view towards studying the health of the workers. Only last year Dr. Ransom of Nottingham suggested that insurance companies before insuring the lives of post-office employés should insist upon some reform, and his remarks were based upon his knowledge of the unsatisfactory condition of the new Nottingham post-office. I cannot trespass further upon your valuable space. But I wish to say this: what telegraphists desire is that the sanitation of the offices in which they work should be placed on a proper footing. It is too much to hope that the English Government will attempt to follow the example of the French administration, but as we have so often failed to secure reform by appealing to the authorities we must now help ourselves.

The Factory Bill gives us the opportunity and we shall try our utmost to secure some concession. Failing that, on each occasion the vote for Government buildings is taken in the Commons we shall make a fight. The only difficulty that exists is that a post-office is neither a factory nor a shop and the Government may escape by means of a legal quibble.

I am, Sirs, yours faithfully,

S. W. BELDERSON.

Grantham-road, London, S.W., June 29th, 1901.

"LEGISLATION AGAINST NATIONAL INTEMPERANCE."

To the Editors of THE LANCET.

SIRS,—Dr. Archdall Reid seems to me to be much too dogmatic in his statements. In discussing a question which is so far from being decided as that of the heredity of acquired characters I think that it is the part of a wise man to state his conclusions as probably or apparently true, rather than to make positive statements which may be upset by fresh evidence as soon as they have been made. Dr. Reid says that there is no more reason for supposing that muscle or liver or any other of the somatic cells are concerned in reproduction than for supposing that germ cells are concerned in the production of motion or of bile. The argument does not touch the problem. The question is not whether somatic cells are concerned in reproduction, but whether changes in the somatic cells may affect the powers of development or, to use a convenient term, the determinants, of the reproductive cells. Now, it seems to me that if one says that there is no more reason for supposing that the somatic cells can affect the properties of the germ cells than for supposing that the germ cells can affect the properties of the somatic cells, it may be replied, "Just so, but there is very obvious evidence that the germ cells, or at any rate the reproductive organs, can affect most profoundly the development of the somatic cells." I refer, of course, to the well-known but most remarkable effects of castration. If the somatic cells were absolutely independent of the germ cells, I fail to see why the antlers should not develop normally in a castrated stag or why the larynx should remain in the puerile condition in a castrated man.

With regard to alcohol, I could scarcely venture to discuss the evidence in detail, but Dr. Reid's arguments do not seem to me convincing. The idea of alcohol causing extinction by injuring the germs seems to me again beside the question. The almost universal use of alcohol in the majority of human races proves that in moderation it does not necessarily produce evil effects. The *excessive* consumption of alcohol is known to produce neurotic degeneration in the individual. My own conclusion is that excessive indulgence in any person necessarily causes an inordinate appetite or craving for alcohol, and that this craving, with the neurosis underlying it, is often strongly inherited. To my mind the neurosis is an acquired character and the inheritance beyond doubt. I know of instances from my own observation. When the craving is indulged the nervous degeneration becomes greater in successive generations, resulting in insanity, paralysis, idiocy, or cerebral defect of some kind. Dr. Archdall Reid, if he wishes to convince his opponents, should prove that the children of habitual drunkards are normally developed both mentally and physically.

Another consideration of great importance which is generally overlooked is the following. Intemperate habits are

very often acquired, or developed, rather late in life, after the victims of them have produced all their children. The children cannot in such cases inherit any of the effects of such habits on the constitution. It is the general experience of breeders of animals that the offspring inherit characters more strongly when the parents are more mature, that is to say, characters which continue to develop until an advanced period of life, e.g., the peculiarities of the carrier pigeon. The mental powers in man are an example of the same kind. They continue to develop long after the time at which most men beget their children, and there is some reason to believe that younger sons generally inherit more brain-power as well as less property. There is also some reason to believe that children produced of aged parents are, in a sense, born old. Such facts, if they are facts, cannot be explained on the assumption that the germ-cells are independent of the history of the soma, and in investigating the inheritance of degeneration due to alcoholism the duration of the alcoholism in the parents *before* the generation of the children must be ascertained. I am, Sirs, yours faithfully,

June 21st, 1901.

J. T. CUNNINGHAM.

To the Editors of THE LANCET.

SIRS,—To judge by Dr. Andrew Wilson's letter in THE LANCET of June 29th, p. 1861, I do not seem to have made my position as "clear" as he asserts. I am sure I do not know whether "the germ-plasm is utterly independent of the body-plasm," or "whether the great divergence between germ and soma contended for by Galton (?) and Weismann is a reality." I know absolutely nothing about all that, and, notwithstanding the vast flood of mystical chatter indulged in by various eminent biologists, I am equally sure that nobody else knows more. But I do positively know (1) that a child (for instance) is derived from a single cell of each parent and (2) that the soma (i.e., the other cells) of each man acquires millions of traits. It seems to me incredible that each of these millions of acquirements can so affect the germ that every particular acquirement tends to be reproduced by the child. In other words, the assumption that the offspring of the germ cell tend to reproduce the acquirements of the other cells seems to me as improbable as the assumption that the offspring of Brown tend to reproduce the acquirements of Jones and Robinson. I know, moreover, that for 20 years search has been made for indubitable instances of transmission without success. When, therefore, the transmission is alleged I ask for proof. Dr. Wilson, in reply, refers me to the works of Darwin, Herbert Spencer, Eimer, and others—to this gentleman and that. He adopts an impossible method of controversy. I cannot review the works of Spencer, for instance, in the correspondence columns of THE LANCET. I am prepared, however, to make Dr. Wilson a reasonable offer. Let him describe (giving the necessary details) any instance of transmission alleged by the gentlemen he has named and I will undertake to demonstrate the fallacy. Indeed, so strong do I feel my position to be that I willingly grant him the whole literature of the subject to delve in.

The last sentence of his letter is characteristic. In effect he asserts that nobody has proved that acquired traits are *not* transmitted. I quite agree, and I may add that nobody will ever prove it, but only because no one can prove a negative. Suppose, for instance, I stated that at three minutes past midnight on Nov. 10th, 1900, a dragon sat and curled his whiskers on the cross of St. Paul's could anyone disprove the assertion? Could Dr. Wilson? Certainly not. He would merely say that my assertion was incredible and ask *me* for evidence. I ask him for evidence. The onus of proof rests with him.—I am, Sirs, yours faithfully,

Southsea, July 1st, 1901.

G. ARCHDALL REID.

"PUERPERAL FEVER TREATED WITH ANTI-STREPTOCOCCIC SERUM."

To the Editors of THE LANCET.

SIRS,—In THE LANCET of June 29th, p. 1824, I read notes of a case which appears under the heading: "A Case of Puerperal Fever treated with Anti-streptococcic Serum; Death." I think this heading is distinctly misleading. From May 1st the patient is passing through an intensely septic process with repeated rigors, delirium, incontinence of urine (which was albuminous), and thrombosis of both legs and left arm. The patient's condition became so critical that strychnine and brandy were necessary on May 18th. Then, on

May 19th, anti-streptococcic serum was used for the first time. I think that it would have been wise to have stated that the serum was first employed on the nineteenth day after the manifestation of septicaemia. Moreover, one would like to know whether: (1) the case had been proved to be one of pure streptococcic infection by bacteriological and microscopical examination; and (2) what was the nature of the nurse's infection? My personal experience of anti-streptococcic serum is that if it be used early and in sufficient dosage in suitable cases—i.e., in cases proved to be of streptococcic origin—it is as valuable a therapeutic agent as anti-diphtheritic serum.

I am, Sirs, yours faithfully,

Bedford, June 30th, 1901.

S. J. ROSS.

ALFRED HUGHES MEMORIAL AT KING'S COLLEGE, LONDON.

To the Editors of THE LANCET.

SIRS,—A movement has been set on foot to establish a memorial to the late Professor Hughes at King's College. It has been felt that his memory should be perpetuated in London as well as in Wales, for the last years of his life were spent with characteristic energy among London students. His anatomical collection has, in accordance with his expressed wish, been presented to King's College by Mrs. Hughes and will bear his name. It is hoped that the fund now being collected will enable us to erect a memorial tablet in the anatomical museum or college chapel and also to found a scholarship or medal in anatomy to be awarded annually and to be named after our late colleague. With this object in view a large number of circulars have been posted to members of the medical profession, prominent anatomists, and others. Already a considerable number of subscriptions have been sent in. These include £5 from Lord Lister, £5 from the Hon. W. F. D. Smith, M.P. (treasurer of King's College), £2 2s. from the Archbishop of York, and a number of others, principally from members of the Council, the staff, and the students of King's College. The object of this letter is to make the fund still more widely known. There are doubtless many of Professor Hughes's friends and old students whom it is difficult to reach in other ways who would like to show their appreciation of his worth by sending donations to the fund. A full list of subscribers will be published later. Contributions should be sent to me.

I am, Sirs, yours faithfully,

W. D. HALLIBURTON.

King's College, Strand, June 30th, 1901.

THE TREATMENT OF DYSENTERY AT SPA BY CHALYBEATE WATERS, &c.

To the Editors of THE LANCET.

SIRS,—I think the following case of sufficient importance at the present juncture to warrant publication. Captain —, aged 27 years, good antecedents, consulted me at Spa on June 9th, having been invalidated home from the Cape after dysentery. He had a relapse on the way home, and when first seen was passing from eight to ten bloody stools per diem, with inability to leave his hotel without previously taking 10 drops of chlorodyne, &c. I put him on a course of our iron water and douches and an internal antiseptic treatment. The result has been most satisfactory. In 16 days he has put on eight pounds weight and is now, since eight days, passing formed stools without blood and has ceased using chlorodyne.

I am, Sirs, yours faithfully,

Dr. A. CAFFERATA.

Avenue du Marteau, Spa, June 28th, 1901.

EQUINE VARIOLA—HORSE POX.

To the Editors of THE LANCET.

SIRS,—During the last few weeks I have had five or six cases of the above disease in the human being accidentally inoculated from the horse, but as I do not remember ever seeing any note of similar cases I thought it might be of interest to record it. The subjects have all been stable lads employed at racing stables, and the disease has generally been inoculated into the hand or arm while drenching or balling a horse. In one case, however, the upper eyelid was the seat of inoculation. The phenomena have been far more acute and of shorter duration than in vaccinia.

I am informed that several years ago a number of cases