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