general neuritis. It was several months before he recovered. In another case the symptoms were less severe, but most marked as connected with nerve disturbance from the very commencement. He was in attendance on several bad cases of influenza in the winter of 1892, when he began to have numbness and weakness of his legs; in two days he could not stand. He was obliged to take to his bed, was carefully watched, and never had a rise of temperature or any other symptom, except a tendency to syncope. After several weeks he recovered, having for a long time all the symptoms of ataxia. The tendency to syncope at the beginning of the disorder is not at all uncommon, and several fatal cases have been recorded. It seems indeed as if some poison was acting on the upper part of the spinal cord and so affected the vagus nerve. One patient was recommended after three months' illness to go into the country to complete his convalescence from the effects of influenza. He answered that when after three days' illness he could scarcely walk from weakness and pains in his legs he was ordered to bed, and now after three months he was ordered to go out when his symptoms were exactly the same in every particular as at the beginning. Another patient, who was rather suddenly seized with weakness of the legs and was told six weeks afterwards that it was the result of influenza, declared that he had had no other influenza but that, and was somewhat angry with a medical man who told him he must have had febrile symptoms; they might have existed without his knowledge or been overlooked; but assuredly he must have had something prior to the nerve trouble. To some minds it seems difficult to make clear that these nerve symptoms can occur without a previous febrile state.

All the facts of influenza seem to me to point to its nervous origin. In very many cases there is an associated fever or pulmonary inflammation, but in others the symptoms seem purely of a nervous kind. I think therefore the word "sequelæ," as applied to the essential nerve symptoms, is misleading. Dr. Gowers may disagree with me as to my opinion of the nature of the complaint, but more probably may not differ much from my views, and explain that the word "sequelæ" is used by him in a more general sense than my definition allows. It refers, he may say, to the fact of the long continuance of the symptoms and even to their development as the case progresses—for example, that a neuritis of three months' standing may be called a "sequel," although it commenced the illness. A very large number of cases will be required to afford a good report on influenza, and the subject must be further discussed from the point of view I have been taking.

I am, Sirs, yours truly,

Grosvenor-street, W., July 18th, 1893.

SAMUEL WILKS.

"EXAMINERS AT THE COLLEGE OF SURGEONS."

To the Editors of THE LANCET.

SIRS,—In an annotation on "Examiners at the College of Surgeons" in THE LANCET of July 15th a covert insinuation of political dishonesty is made against myself and Mr. Mayo Robson for voting against a motion proposed by Mr. Willett and seconded by Mr. Tweedy: "That in the opinion of this council it is desirable that no one who has been elected a member of the Court of Examiners should at the expiration of his period of office be eligible for immediate re-election, but that this resolution shall not apply to the existing members of the Court of Examiners." This resolution, which deals with a matter of detail, is magnified into a cardinal point of reform, and the writer, after mentioning the names of those who voted for and against the resolution, confesses "to some surprise at finding the vice-president of the Association of Fellows" (myself) "and one of the specially selected candidates of the Association at the last election" (Mr. Mayo Robson) "giving an adverse vote." A recommendation of the Association in 1884 of a similar kind to Mr. Willett's motion, but passed as an amendment to the Charter, is quoted at the end of the article.

Permit me to say that in 1884 I did not belong to the Association or take any part in its proceedings, but during the last five years I have been an active member of this body and identified with its policy. During that period the committee of the Association has carefully abstained from formulating any suggestion in regard to the vexed question of examinerships, directing its energies to the primary

point of all reform at the College so far as the Fellows are concerned—viz., to secure for the Fellows a consultative voice in the affairs of the College and a position akin to that occupied by the Fellows of the Royal College of Physicians. To this policy all adhere, but considerable difference of opinion would probably be shown in regard to the relation of examiners to the council. On this subject everyone is free to think, speak and vote as his judgment dictates, so that any suggestion that I have departed by a single hair's breadth from any professed principles falls at once to the ground. And now a few words in regard to the substance of Mr. Willett's motion. It will probably be conceded that the first duty of councillors at the College is to administer faithfully the affairs of the institution in accordance with the provisions of the charters and by-laws as they stand. The Charter provides that every Fellow of the College who shall be elected to the office of examiner shall go out of office at the end of five years from the day of his election, but that it shall be lawful for the council of the said College immediately to re-elect such person to the said office. The retiring examiner is declared by the Charter to be eligible for immediate re-election. Mr. Willett's motion would have declared him to be ineligible and to my mind is directly at variance with the Charter. The motion, if carried, would have been inoperative; and I declined, and always shall decline, to vote for motions contrary to existing charters and by-laws, and motions which would be futile and inoperative. If the resolution had affirmed the expediency of amending the Charter in this particular it would have stood upon a different footing, but even then the question of continuity or non-continuity of examinerships is by no means so simple as it appears. There is much to be said on both sides of the question.

I am, Sirs, yours truly,

Wimpole-street, July 18th, 1893.

WALTER RIVINGTON.

* * We need hardly disclaim any intention of making an "insinuation of political dishonesty" against Mr. Rivington or any of those gentlemen who voted with him. The names were obtained by us through the ordinary channel of official information. We approved the purport of Mr. Willett's resolution as being in accordance with the opinions advocated by THE LANCET for more than half a century. The precise subject under discussion has frequently been considered by THE LANCET during the last twenty-five years, and more particularly in the years 1870-73 and again in the years 1883-84. We take this opportunity of stating that THE LANCET is in no sense the special advocate of the Association of Fellows of the Royal College of Surgeons, or of any other body. Since the Association was formed nine years ago THE LANCET has given it a steady general support, because the Association had adopted and pursued a policy of reform which had for so many years previously been persistently advocated by THE LANCET. But in this as in all other matters THE LANCET will continue to maintain its right to free and independent action.—ED. L.

SURGEON-MAJOR KING AND THE USE OF VARIOLOUS LYMPH.

To the Editors of THE LANCET.

SIRS,—Mr. Russell's answer in the House on July 3rd to Sir William Wedderburn's question (for Mr. Hopwood) as to whether Surgeon-Major King had been punished for vaccinating with lymph (variola or small-pox), or small-pox, in fact, inoculated into the cow and thence taken to vaccinate human beings? opens up a question in vaccination, as carried on in India, of the greatest importance to the future of this great prophylactic in that vast country, and I can only hope that some member of our profession in the House will call for full particulars regarding this case. You may remember that about thirteen years ago you kindly published in THE LANCET particulars of an experiment I carried on at Meerut, N.W.P., India, to test the possibility of Dr. Badcock's experiments, by which he seemed to have produced artificial vaccinia in the cow by means of variolous inoculation from the human subject. My experiments at first seemed to hold out hopes of success, but as I continued them and with the greatest care avoided all possibility of vaccine lymph being used (which cannot be said for Dr. Badcock's experiments), I

could see at once that nothing truly constitutional occurred, and that if I had used the contents of the pustule—for it was never a vesicle—I should simply have inoculated the child with small-pox and thus have repeated the experiments which alarmed my uncle, the late Dr. G. G. Macpherson, some fifty years before in Bengal, when, as he told me in 1873, the most alarming results followed what he hoped would be simple vaccination. There is much that needs clearing up in Surgeon-Major King's case, and if Mr. Russell said, "Surgeon-Major King was censured and transferred from civil to military duty for having used, without authority from the Government of India, a form of lymph not sanctioned by the Government for use amongst the native population," all I can say is that something has occurred in the Indian vaccine department since I retired in 1884 which to me is absolutely inexplicable, and I wonder what it must be to many others. During my thirty years' service in India from 1854 I think I have seen as much vaccination and studied inoculation as closely as any medical officer in the Indian army, and yet I repeat that I cannot understand why Surgeon-Major King has been punished.—I am, Sirs, yours truly,

ROBT. PRINGLE, M.D. Edin., Brigade-Surgeon.
Blackheath, Kent, July 10th, 1893.

"NEW REMEDIES": "HOS EGO VERSICULOS FECI; TULIT ALTER HONORES."

To the Editors of THE LANCET.

SIRS,—Under "New Remedies" in Wright's *Medical Annual* I find it stated that Dr. E. S. Stuver, having remarked on the great value as a germicidal agent and deodoriser of peroxide of hydrogen, considers that its most characteristic property is the extreme avidity with which it combines with and decomposes pus. In my *brochure* on "Therapeutics and Disease," published in 1870, I advocated the application of oxidising agents to all suppurating surfaces, and for the purpose of preventing putrefaction. I pointed out that in such cases, as for example in diphtheria, malignant scarlet fever, cancrum oris, hospital gangrene &c., these surfaces react on the system by auto-inoculation. I believed then, as I do now, that the specificity of many constitutional diseases and of their local manifestations is due to the formation of intermediate compounds or leucomaines, and that these are rendered innocuous by speedy oxidation, whereby they are reduced to ultimate binary compounds (*vide* my work on "Kochspiel"). The persistence of suppuration of a destructive character often is thus not due to spores, which are concomitant, accidental or consequential, but to intermediate alkaloidal bodies; and wounds are thus deodorised and rendered healthy by the application of oxidising agents, oxygen being nature's deodorant *par excellence*. This practice rests on a scientific basis and not on a visionary and delusive theory such as the germ theory, which has done so much to bring ridicule and contempt on medical science. Oxygenated water is now largely used in France as an application to wounds, and it is for the reasons above stated that permanganate of potash and like agents are so efficacious in the treatment of putrid wounds or fetid suppurating surfaces. I find also in the *Medical Annual* that Guyon recommends a solution of perchloride of mercury in cases of cystitis and that his patients were greatly benefited thereby. I used perchloride of mercury solution in the treatment of cystitis over twenty years ago, and in my work on "The Functional Diseases of the Urinary and Reproductive Organs" I have described how salivation was thus accidentally caused. From the same work I gather that Dr. Stuver uses bichloride of mercury in gonorrhoea. In a paper on the treatment of this affection¹ I recommended bichloride of mercury solution (1 gr. to 8 oz.). Dr. Stuver uses 1 gr. to 6 oz. I have no hesitation in saying that this strength is excessive; the solution employed should be much weaker.—I am, Sirs, yours faithfully,

D. CAMPBELL BLACK, M.D.,
Professor of Physiology in Anderson's College Medical School.
Glasgow, July 15th, 1893.

"THE RAMSGATE MYSTERY."

To the Editors of THE LANCET.

SIRS,—In THE LANCET of the 15th inst. I think you are hardly fair in your comment on the Ramsgate murder case to the medical men first called in. In the first place,

you say we (Dr. Fox and Mr. Cotton) formed our opinions as to the time of death by the coldness of the deceased's hands, neglecting to mention the face, neck, ears and nose. As a matter of fact, we laid no weight at all on the temperature of the hands. In the next place, all the experiments (thermometric) that have been made on dead bodies with a view of ascertaining the rate of cooling, have been made on subjects who have died natural deaths in hospitals and been removed from there to a cold dead-house, covered only by a sheet. Now I ask in all fairness could you compare thermometric experiments made in such cases with those made on a person killed suddenly, fully clothed, in a very small room (ten feet by six), on an excessively hot day, door and windows being closed? The deceased could only have been dead two hours and twenty minutes when I saw her, and under the above circumstances I do not see where the difficulty of approximately fixing the time of death lies. The defence sought to prove that death took place at 2.45. In that case I was only fifteen minutes out. Hoping you will be kind enough to publish this, I remain, Sirs, yours truly,

Vale-road, Ramsgate, July 16th, 1893.

C. N. FOX.

* * * Our annotation was written upon the facts as published in daily contemporaries. We agree with our correspondents that too much reliance should not be placed on thermometric registers; at the same time when such are taken as bases on which to found hypotheses the best obtainable evidence should be secured. What we wrote respecting the hands would apply with equal force to the face, neck, ears and nose. We did not question the *bona fides* of the medical witnesses on either side, nor did we attempt to compare the relative value of their opinions. Our contention was that in a criminal case the same latitude should be extended to witnesses for the defence as to those engaged by the prosecution.—ED. L.

FIFTEEN TIMES TWINS.

To the Editors of THE LANCET.

SIRS,—In the course of insurance work I have lately met with some details of family history which, I think, may interest and perhaps surprise some of your readers. The proposer stated that her father died at the age of eighty; that her mother was alive at the age of seventy-nine; that she had four brothers and four sisters alive and in good health; that her mother had "had thirty-three children, fifteen times twins," and that "twenty-four died under six months old." From personal knowledge I cannot vouch for the truth of the above statements, but they came to my notice on a serious business form, which was signed by the proposer and contained a solemn declaration that she believed in the truth of her statements and agreed that they should form part of the basis of contract. I may also add that none of the accompanying documents threw any doubt upon the genuine nature of the proposal, although my first impression was that the story of "fifteen times twins" might be a poor attempt at a joke.

I am, Sirs, yours truly,

NESTOR TIBARD.

Weymouth-street, Portland-place, July 17th, 1893.

SO-CALLED "VILLOUS CANCER OF THE BREAST."

To the Editors of THE LANCET.

SIRS,—My attention has lately been called to a paper on this subject by Mr. Bowlby in THE LANCET of June 10th which appears to require brief notice from me. It is a repetition of an essay published in 1888, with some trifling additions. Mr. Bowlby says that at that time only six isolated examples of this disease had been recorded in English literature. Herein he is certainly in error, for Mr. Bryant's book on Diseases of the Breast, published in 1887, contains this number, and I can cite over a dozen different cases from other sources, while from French and German literature I have gleaned more than another dozen, all published prior to 1888. My attention was first specially directed to the subject in 1883, when I met with two instances of the disease—one in a male, the other in a female—of which I published an account in the Middlesex Hospital Reports for 1883, and subsequently I met with a third case, an account

¹ Brit. Med. Jour., April 23rd, 1870.