

obliged if he had added the address of one of the accredited persons.

I will now deal with Lieutenant-Colonel Maitland and Captain Donovan exactly on the same principles as those which I applied in dealing with Dr. Saldanha, and Captain Donovan will find the process quite different from a mere acceptance of *ipse dixit*. Lieutenant-Colonel Maitland says that in Madras "cancer is very prevalent amongst Hindoos." Captain Donovan says that "Colonel Maitland speaks for the Madras General Hospital where cases from all over the Madras Presidency come for operation." Lieutenant-Colonel Maitland I myself may add, occupies the high position of senior surgeon to that hospital and professor of surgery in the university. The population of the Madras Presidency is 40,000,000 and that of the city 500,000, of whom nearly four-fifths are Hindoos. What does Lieutenant-Colonel Maitland write when he comes to figures? He says: "In the wards of the Madras General Hospital cases of cancer affecting Hindoos are *almost* always to be found. At the time that I write I have *three* Hindoos in my wards suffering from the disease." The italics are mine, "*almost* always." "*Three*." And they come from among 40,000,000 of people, 30,000,000 of whom are Hindoos!

No one has said that the Hindoos *never* suffer from cancer. What I said in my paper of August 31st was that they rarely suffered from cancer of the *stomach*. Neither Lieutenant-Colonel Maitland nor Captain Donovan mention the stomach, although they refer to cancer of "the penis, the jaws, throat, cervix uteri, and breasts, &c." Unless the reference is somewhere concealed in this "&c." I am at a loss to know what bearing Captain Donovan's letter really has on the substance of my paper, or why he should plunge into the discussion at all, especially in a manner so reckless that, even in correcting the form "Hindoo," his reproof falls as heavily on Colonel Maitland and on the dictionaries as on me. He goes on to say, "Of course, the word 'Hindoos' is employed by Mr. Keetley in a very loose sense and might have passed muster 50 years ago as of sufficient scientific accuracy, but in this more enlightened twentieth century," &c., in the same style. The term "Hindoos" is employed by me as referring to Hindoos only. It is not used by me at all except in reference to Dr. Saldanha's letter. In my original paper I asked gentlemen for facts bearing on my hypothesis, and particularly requested (1) that the distinction between carcinoma and sarcoma should be carefully observed, and (2) for exact references to published sources of information. In spite of this "enlightened twentieth century" and its "scientific accuracy," neither Captain Donovan nor any of the other gentlemen who have responded has paid the slightest attention to these reasonable requests. Each flings his general impressions at my head as if they were half-bricks. But each gentleman, whether gentle or otherwise, remains responsible for his own statements so long as I refer to him when referring to them.

Perhaps some Indian medical officer will kindly tell me whether the Mahomedans circumcise or not. I have asked a number of well-informed and travelled people this question in vain. Of the four parts to which Lieutenant-Colonel Maitland refers as "very common" seats of cancer in Madras two are the penis and the uterus. He also says that cancer is much more common among Hindoos than among Mahomedans. Now, in THE LANCET of Dec. 1st (p. 1764), Dr. H. R. Andrews, quoted by Dr. James Braithwaite, says that "the Jews are much less subject to cancer than we are and that they are especially free from cancer of the uterus." Dr. Braithwaite says that this is because the Jews take so little salt, but on that point Mr. M. Bernstein contradicts him absolutely in the same page (p. 1764). There is one, and perhaps only one, point of difference between Jews and most Christians on which we shall all agree—viz., that the Jews have less foreskin and therefore less accumulation of smegma præputialis than persons with foreskins. Now the first part of my hypothesis, as given on August 31st, is that the "specific cause" of carcinoma "flourishes in either the secretions or the cells of the skin-glands, including the mammary gland—e.g., milk, butter, cheese, *sebaceous material*, and *sweat*"; smegma præputialis I specially mention in THE LANCET of Oct. 5th (p. 939). The bearing of this on the occurrence of cancer of the uterus is obvious. Its bearing on the frequency of cancer elsewhere is also clear, though less obvious, and it is important, because if carcinoma is in any way a contagious disease any increase or decrease of cancer in a situation like the penis or the uterus, where it is attended by discharge, is an increase or decrease of

foci of infection, and therefore leads to increased or decreased general frequency of the disease.

There are other ways, I acknowledge, in which circumcision may diminish the frequency of carcinoma—viz., by lessening liability to syphilis and gonorrhœa and to other causes of chronic ulcerations which may lead the way to carcinoma.

Lastly, may I wish success to your efforts to obtain the appointment of a commission to inquire into the causes of cancer?

I am, Sirs, yours faithfully,

Grosvenor-street, W., Jan. 10th, 1902.

C. B. KEETLEY.

VOLKMANN'S CONTRACTURE.

To the Editors of THE LANCET.

SIRS,—A point of great importance has been omitted from my paper on Volkmann's Contracture owing to a mistake of mine during the correction of the proof-sheets. It is to the effect that Professor Oppenheim of Berlin refers briefly to ischæmic paralysis in his "Text-book of Nervous Diseases," 1900. Here he emphasises the importance of *normal* electrical reactions as the most characteristic feature in the diagnosis of this deformity.

I am, Sirs, yours faithfully,

Jan. 11th, 1902.

LEONARD S. DUDGEON.

To the Editors of THE LANCET.

SIRS,—I have read Mr. Leonard S. Dudgeon's paper on "Volkmann's Contracture" in THE LANCET of Jan. 11th, p. 78, with great interest. "Volkmann's contracture" appears to me to be so much better a way of naming the deformity than "ischæmic paralysis"; it commits us to no theory as to causation. There is one slight error in the paper which I hope I may be pardoned for correcting. Mr. Dudgeon says, "Tendon lengthening first advised by Mr. Page has since been carried out by Mr. Littlewood, Mr. Barnard, and others, &c." If Mr. Dudgeon will again refer to the cases he has quoted he will see that I performed the operation on Dec. 5th, 1898, and that Mr. Page performed his operation on Dec. 20th, 1898, 15 days later. Since performing the two operations referred to I have operated on a third case, with a very satisfactory result, the inclosed short notes of which I shall be glad if you will insert. A female child, aged three years, was admitted under my care into the Leeds Infirmary on August 12th, 1900. She was suffering from the typical deformity—Volkmann's contracture. This had appeared after a fracture of the lower end of the right humerus in May, 1900. The fracture was treated with an anterior rectangular splint, which was kept on for five days; on being removed a large sore was found on the anterior surface of the forearm, near the elbow-joint. At the time of the operation there was a large scar in this position and the contracture was extreme. The operation was performed on August 28th, 1900. All the tendons were lengthened on the front of the forearm. It was a difficult operation, as the tendons were such slender structures. In this case I lengthened them so that I could put up the fingers in the slightly hyper-extended position. The patient made a good recovery. I saw her yesterday (Jan. 12th, 1902). She has now quite an excellent hand, with full use in it.

I am, Sirs, yours faithfully,

Leeds, Jan. 13th, 1902.

H. LITTLEWOOD.

THE LIVERPOOL PATHOLOGICAL DIAGNOSIS SOCIETY.

To the Editors of THE LANCET.

SIRS,—As clinical pathology is in its infancy and will claim an increasing number of workers the question as to the fees of the above society and of pathologists generally should be now considered by a representative committee. I trust Mr. C. Powell White will initiate such action. I think no one will seriously deny that the work is worthy of consulting fees. Such is the opinion of most London hospital medical staffs. There can only remain the broader question whether medical services are adequately rewarded when compared, say, with the legal profession or trade. The comparison is unfavourable, more especially when we consider the value of our work to the community. It would be to the interests of both the profession and the public to consider fully whether this branch of medicine alone should be underpaid. Public bodies and charitable institutions

should afford such facilities to the poorer classes, but to allow the well-to-do to escape a just toll must react eventually injuriously on one and all. Precisely as cheap medical and surgical institutions are unsound in principle and harmful to the majority, so also are cheap pathological institutes. I think gratitude is owed to Mr. White for bringing forward this important question once more.

I am, Sirs, yours faithfully,
Plymouth, Jan. 11th, 1902. F. BUSHNELL.

FIRST AID FOR PRIMARY HÆMORRHAGE: THE WRONG TEACHING GIVEN TO AMBULANCE CLASSES.

To the Editors of THE LANCET.

SIRS,—“The notions stated to have been acquired at ambulance classes” are certainly startling if the holders of first-aid certificates “have got the idea fixed in their minds that they must always apply some sort of an improvised tourniquet to the limb above the bleeding part,” and this treatment to include “cases of ruptured varicose veins, superficial lacerated wounds, and compound fractures.” It is still more startling to be told that these “notions” are in accordance with what “they had been taught to do.” I do not suppose that I have either more or less common sense than any of the other honorary surgeons of the St. John Ambulance Association, but for my own part I strongly repudiate such absurd teaching as Mr. Walter G. Spencer lays to the charge of ambulance teachers. I quite agree with him as to the great danger of the tourniquet in the hands of ambulance pupils. Will Mr. Spencer kindly state what the comrades ought to have done in the following case which I extract from the *Daily Mail* of Jan. 7th? “Wm. Hickman had both his legs cut off in a chaff-cutting machine which he was working yesterday at Messrs. Whittome and Co.’s, Islington. He died before he could be taken to hospital.”

I am, Sirs, yours faithfully,
Ongar, Essex, Jan. 8th, 1902. J. B. G. GIDLEY-MOORE.

To the Editors of THE LANCET.

SIRS,—Permit me to endorse the opinions of my co-lecturers and examiners of the St. John Ambulance Association whose letters on first aid in primary hæmorrhage appeared in THE LANCET of Jan. 11th, p. 117. The first-aid treatment of hæmorrhage is most clearly and soundly laid down in the text-book of the association, and I submit that no one can take exception to that instruction, which I find, after a considerable experience as an examiner, is most strongly emphasised by the lecturers. It is not always easy to instil these principles into the minds of some of our pupils, but such ones seldom succeed in gaining the certificate. Moreover, many of the pupils fail to see the necessity of attending more than one course of lectures and hence forget the knowledge they have gained. But these facts do not detract from the correctness of the instruction which is given to the classes.

Your correspondent Mr. Fenoulhet is right in suggesting that a somewhat more prolonged course of instruction would be beneficial, but in a great number of classes this plan is carried out by lecturers, much to the advantage of the pupils. I am quite sure that when intelligently carried out first aid has been often the means of bringing to the doors of medical men “living” patients instead of “dead” people.

I am, Sirs, yours faithfully,
Streatham, S.W., Jan. 13th, 1902. M. COATES.

To the Editors of THE LANCET.

SIRS,—Permit me through your columns to express my thanks to Mr. Walter G. Spencer for his admirable letter upon this subject. I am sorry to say that I have taught the errors alluded to until this winter, but having seen many cases on a par with those so fully stated by him I had no difficulty in convincing myself that the present teaching was as erroneous as it was injurious and for the first time decided not to teach a class except in my own way. Here arose difficulties which I could not brush aside. If I taught my own views my pupils would not pass the necessary examination, and their chances of gaining the valuable cups, &c., given for competition would be *nil*, and to teach as formerly

was out of the question. In this dilemma I wrote to the secretary of the Fife Colliery Surgeons’ Association asking permission to express my views on the subject at its next meeting on Nov. 23rd, 1901. He replied that he was pleased to hear of my intentions, but unfortunately this letter was followed by another on Dec. 3rd in which it is stated that my “remarks could be received only informally, the main object of the association hitherto being for the ethical side of our profession.”

After such a snub I need scarcely add that my paper [was not read, that my class is still unopened, and my pupils as anxious as ever. My views and cases run parallel with those mentioned by Mr. Spencer, and with very slight modifications I agree with his treatment. Now, Sirs, What am I to do? I trust that Mr. Spencer’s letter will be more successful in arousing the profession generally to take an interest in this matter which I failed to obtain locally—and that someone will reply to my query—What am I to do?

I am, Sirs, yours faithfully,
JOHN MACDONALD, L.R.C.P. Edin.
Lochgelly, Fife, N.B., Jan. 15th, 1902.

THE SANATORIUM TREATMENT OF CONSUMPTION.

To the Editors of THE LANCET.

SIRS,—It would be wise to pause and to consider whether the money now being provided for sanatoriums to *cure* consumption could not be more advantageously spent to *prevent* the same by producing over large areas the hygienic conditions under which tuberculous diseases slowly and surely die out. Dr. Lubbock of Norwich, who taught an out-door active life, open windows in dormitories, beef-steaks and porter, has done much good for those who have condescended to learn of a provincial Englishman.

I am, Sirs, yours faithfully,
Framlingham. GEO. E. JEAFFRESON.

REVACCINATION.

To the Editors of THE LANCET.

SIRS,—In view of the present epidemic of small-pox in London it would be highly instructive as well as interesting to know what percentage of persons attacked by the disease had been *successfully revaccinated*, and whether cases have occurred of persons who had recently (say, within six months) been revaccinated contracting the disease. Perhaps some of your correspondents who are on the medical staff of the small-pox hospitals would kindly afford me this information.

I am, Sirs, yours faithfully,
Jan. 8th, 1902. J. BRANDER, L.R.C.P. & L.R.C.S. Edin.

SUSCEPTIBILITY TO REVACCINATION.

To the Editors of THE LANCET.

SIRS,—While thoroughly agreeing with Dr. F. W. Andrewes that the publication of statistics by various practitioners would be of use to the cause of vaccination, I fear that there will be great discrepancies owing to the various methods of vaccination employed, and this will be well instanced by a comparison of the results obtained by Dr. Andrewes and by myself. Curiously enough he has vaccinated at St. Bartholomew’s Hospital 171 persons, and in the midst of a large number of vaccinations recently performed by me I have a batch of 193 revaccinations performed at one place—namely, the office of the Ocean Accident Corporation in Moorgate-street, E.C.; of these 162 were males and 31 were female clerks or officials on the staff. Out of the whole number of revaccinations there was not one single failure, all the arms developing well-marked vesicles in the three places scarified. Among these cases six had been recently vaccinated with negative result, but in my hands they showed no insusceptibility. In 140 cases the vesicles were large and well formed, being 72.53 per cent.; in 31 cases the vesicles were moderate in size, being 16.06 per cent.; and in 22 cases the vesicles were small but not under a quarter of an inch in diameter, being just under 11.4 per cent.

With regard to these results I think it right to say that the only lymph used was that of the Jenner Institute, obtained through Messrs. Allen and Hanbury, and the method of vaccinating was as follows. The arms were first washed with