a wide, ragged, and excessively serious wound instead of the smooth, narrow burrow as above described. Secondly, in the opinion of some observers the space between the combatants has tended to grow less than it was at first, and as a natural consequence the severity of the wounds received at the shortened ranges is much greater than it was. In the third place—and according to Dr. Alverne it is here that we have to look for the real explanation—the soldiers being all more or less worn out owing to climate, exposure, bad food, privation, and excessive marching are not so apt for recuperation as they were at the beginning of the war. Their constitutions have run down and their wounds consequently suffer from the lack of vitality. Hence the frequency of the suppurations which are now found to retard cicatrisation.

TRANSSAAL WAR NOTES.

Major B. W. C. Deeble, R.A.M.C., and Civil Surgeon H. J. Starling left South Africa for England in the *Dunera* on Christmas Day.

The following have been discharged from hospital to duty:—Civil Surgeons: H. E. Birmingham, W. O. Beddard, W. J. Collins, and A. Thompson.

SOME FACTS RELATING TO CHOLERA IN INDIA.

The annual report for 1898 of the Sanitary Commissioner with the Government of India published recently contains, among other things, some interesting facts in connexion with cholera. The year 1898 was a relatively healthy one both as regards the European and native armies of India and the native population, and the military shared in the immunity from cholera—an immunity which generally existed throughout the Indian Empire. Cholera occurred among British troops in nine stations only, and there were in all 30 cases and 21 deaths, a case-mortality of 70 per cent. In only one instance was the disease in epidemic form—viz., at Umballa, where there were 20 seizures, with 10 deaths out of the 11 Europeans attacked. Among native troops cholera occurred in six stations among 10 bodies of men. There were in all 57 cases, of which 44 ended in death, a case-mortality of 77·2 per cent. Only 11 jails out of the total number of Indian jails suffered, and there were only 18 cases in all. From the cases in which details were available it appears that altogether 259 individuals were in attendance on 102 cholera cases, and none of them were attacked. No seizures followed subsequently to the movement of troops from infected localities in the case of British troops, but the effect of such movement, although generally satisfactory, was not so completely successful in the case of native troops. The outbreak among European and native troops and followers at Umballa was of special interest, because the Punjab was apparently free from cholera at the time of its occurrence. It is unfortunate, however, that the accounts of the outbreak at Umballa were of a fragmentary character. The Umballa cantonment water-supply is conveyed by pipes which were leaky, and the medical officers consequently ascribed the outbreak to a polluted water-supply, but there was no satisfactory evidence pointing to the water as the cause of cholera. It is curious “that the vibrio of cholera was found in the well used by the Government model dairy, but no case occurred at the dairy, and neither the milk nor the butter from the dairy, appear to have been used by the natives who suffered.”

THE STORY OF THE THORN.

The story of the thorn is published in the *Times* of Dec. 26th, 1900, as illustrative, we suppose, of the surgical incompetence of the medical staff and the shortcomings of army hospitals in South Africa. It may or may not be an accurate record of the experience of the patient in question, about which we have our own opinion. But it seems to us grossly unfair and wrong for a writer to seek to cast a slur upon a body of men belonging to any service under the shelter of anonymity. “F.R.C.S.” may have a keen appreciation of what constitutes a good story, but we cannot congratulate him on any fastidious sense of honour or justice. If he considered it his duty on public grounds that the allegations should be brought to notice he could easily have done this through the proper channel. At any rate, the story should not have been told anonymously but should have been signed and accompanied by such particulars as would have enabled the authorities on the spot to have instituted, in the interest of all concerned, some inquiries into the matter. As it is, “F.R.C.S.” has cast an unworthy aspersion indiscriminately upon a large number of

members of his own profession in the army and, in effect and by implication, has maligned them.

ARMY REFORM.

It is understood that Lord Wolseley will take an early opportunity of speaking on this subject in the House of Lords, and there is naturally a good deal of interest and curiosity felt as to what the late Commander-in-Chief will have to say and as to the nature of the reply which the Government will make and the course which it will adopt. The matter is one of national interest and concerns the medical as well as all other branches of the army.

On Jan. 1st Her Majesty the Queen paid a visit to the Convalescent Home at East Cowes. Several men from South Africa were there, and on the evening of the same day members of the Royal family from Osborne entertained them with music, both vocal and instrumental.

Among the names in the list of New Year honours is that of Major John Crimmin, I.M.S., V.C., who has been made a Companion of the Order of the Indian Empire.

Correspondence.

“Audi alteram partem.”

LENGTHENING TENDONS FOR THE SO-CALLED ISCHÆMIC PARALYSIS (VOLKMANN) FOLLOWING ON INJURIES ABOUT THE ELBOW-JOINT.

To the Editors of THE LANCET.

SIRS,—In your review of the surgical work of the year you refer to tendon lengthening as one of the surgical procedures worthy of note. The case you refer to is “a little boy suffering from ischæmic paralysis the result of application of splints for a fracture of the lower end of the humerus or separation of the lower epiphysis of the humerus.” In THE LANCET of Feb. 3rd, 1900, you kindly published a clinical lecture of mine on this subject, and if you will allow me I should like to again emphasise one or two points I then raised.

I think it is extremely unfortunate that such an inappropriate term as “ischæmic paralysis” should be applied to a condition which is really primarily a contraction of muscles and not due in any way to the involvement of the nerves, nor can it be shown to be the result of deprivation of blood to the parts—ischæmia. In my lecture I stated:—

The deformity appears always to occur in children after some severe injury about the elbow-joint and generally after a fracture separation of the lower humeral epiphysis. I have once seen it after a dislocation of both bones of the forearm backwards. The cause of the deformity is, I believe, primarily in the muscular portions of the flexor muscles of the forearm. If the histories of these cases are carefully gone into it will be found that the injuries have been severe and that the flexor muscles have been torn, possibly by the displacement of the forearm backwards with the lower end of the humerus. The torn muscle is replaced by fibrous tissue, and this contracting is, I believe, responsible for the deformity. A lump varying in size can be felt in the flexor muscles in the upper third of the forearm and it has been present in all the cases which I have seen. It is a well-recognised condition in the sterno-mastoid muscle. A swelling is found in about the middle of the muscle soon after birth, which is thought to be due to a tearing of its fibres during parturition; this sometimes produces wry-neck.

I do not believe that splint-pressure has anything to do with the production of the deformity, although I am well aware that in most cases a sore—pressure sore—is present; but this is not always so, for in Case 1 no abrasion of any kind was present and in Case 2 a sore formed but was on the extensor surface of the forearm. I am especially laying stress on this point as it has given rise to one theory—that the contraction is due to a secondary inflammation extending to the muscles from the splint sore. That explanation clearly will not account for Case 1 in which there was no sore of any kind, and in Case 2 the sore was on the back of the forearm. One would imagine that any inflammation extending from this sore would have involved the extensor muscles and produced a hyper-extension of the wrist instead of involving the flexor muscles. For these reasons I am inclined to think that the pressure-sores are not causally related to the deformity; they may, however, be taken as some index of the amount of laceration of the soft parts at the time of the injury, producing a great amount of effusion which increases in amount after the application of splints. Probably the effusion of blood into the skin and subcutaneous tissues renders them anæsthetic, acting in the same way as a hypodermic injection of water; and this may explain why a large sore may form and yet the patient will not complain of any pain.

The explanation here given is, I venture to think, worthy of a fuller consideration. As to the treatment by tendon

lengthening the results justify the treatment. Mr. Page and myself quite independently arrived at the same conclusion that this was the most reasonable way of treating this particular deformity. By referring to the records of our cases it will be seen that I performed the operation about a month before Mr. Page—another example of arriving at the same conclusion independently when similar conditions are under consideration. In my first case the result is now excellent; in the second case the result is fair, the fingers are straight, but the movements are weak. I have since operated on a third case, on August 28th, 1900—a girl aged three years. Her fingers are now quite straight and the power is daily improving; it is too early yet to speak of what the ultimate result will be.—I am, Sirs, yours faithfully,

Leeds, Dec. 31st, 1900.

H. LITTLEWOOD.

"ARSENIC IN ARTIFICIAL MANURES."

To the Editors of THE LANCET.

SIRS,—THE LANCET of Dec. 22nd contains a short annotation under the above heading which, if copied into numerous country papers, is calculated to produce a most unwarrantable prejudice against the application of artificial manures. It is not sufficient to make general statements, the actual facts should be given. If, as the writer of the annotation states, "the results are not yet ripe for publication," why should a premature prejudice be created in the public mind? In one paragraph it is stated that the Laboratory Commissioners had found certain artificial manures to be rich in arsenic in a soluble form—namely, soluble in water; but further on in another paragraph it is suggested that arsenic accumulates in the soil. Now, if arsenic be present in a form readily soluble in water it must be obvious that it is not likely to accumulate in the soil. On the contrary, if it exists in a soluble form it is more likely to be washed out of the soil than to accumulate. The mere presence of arsenic in a sample of artificial manure is no evidence whatever that crops manured with such a fertiliser would be in any way likely to produce injurious results to the health either of man or beast. In all these matters it is a question of quantity. Some years ago the present writer had occasion to examine some meadow grass which was rendered unfit for grazing purposes in consequence of sediment deposited from the washings of a copper mine, and in order to make a comparison of the amount of copper salts present pieces of turf were obtained from portions of the field not damaged by mineral washings, but, curiously to relate, these pieces of turf contained appreciable quantities of copper, and the grass had been evidently appreciated by the cattle who had refused to eat the portion upon which the detrita of the mine washings had been deposited. The quantity of arsenic likely to be supplied in any artificial manure must be exceedingly small when distributed over the whole field, probably not more than $\frac{1}{10}$ th of a grain per square foot, and being in a soluble form is quite as liable to be washed out of the soil as to be absorbed by turnips or cabbages. I am, Sirs, yours faithfully,

Mark-lane, E.C., Dec. 31st, 1900.

JOHN HUGHES, F.I.C.

* * Our words "the results are not yet ripe for publication" did not refer to the examination of artificial manures but to an investigation into other substances. A list of these will be found in another column of this issue (see p. 54). The fate of soluble arsenic, according to our correspondent's view, would be precisely the same as that of the equally soluble nitrates and phosphates used for fertilising purposes. Are they also "washed out"?—ED. L.

"ISOLATION HOSPITALS."

To the Editors of THE LANCET.

SIRS,—The following cases occurring during the past week are surely a proof of Mr. E. D. Marriott's contention that the "return" cases from isolation hospitals are probably the greatest source of further infection. Two children, William Neville and Annie Foster of Henham, Essex, living about two miles apart, returned from the Hospital for Infectious Diseases at Bishop's Stortford on Dec. 16th last. William Neville was sent there on Oct. 22nd and Annie Foster on Nov. 2nd, both suffering from scarlet fever. The child Neville was kept longer than the usual six weeks so that they

"might return home together." Their respective homes were absolutely free from any disease during the time that the children were in hospital, but on Dec. 23rd, just a week after the return of each, I discovered a fresh outbreak of scarlet fever in both of the homes, two children being attacked in Neville's house and one in Foster's. The child Neville had a nasal discharge when he returned home.

As far as I can ascertain these were the only cases of scarlet fever in the near neighbourhood. So there is certainly no doubt whatever that these "return" cases were the cause of the second outbreak. My idea is that isolation hospitals should be situated on eminences, if possible, as far away from the town as practicable and an intermediary home or hospital (at a distance) should be supplied where convalescents could remain for a further fortnight after discharge from the isolation hospital and where they should be daily bathed, sent out in the open air, and examined and returned to their homes either in new clothes or properly boiled old ones. I am, Sirs, your faithfully,

Stansted, Essex, Jan. 2nd, 1901.

EDGAR PITTMAY.

"THE TEACHING OF ANÆSTHETICS."

To the Editors of THE LANCET.

SIRS,—THE LANCET of Nov. 17th, 1900, contains a leading article (p. 1440) and a letter (p. 1460) by a well-known surgeon—Mr. J. Lionel Stretton—on anæsthetics. Mr. Stretton complains that "such recently qualified men as seek for hospital appointments are lamentably ignorant in the administration of anæsthetics," and this being the case with the pick of the men entering the profession "the rank-and-file must be deplorably incompetent." Mr. Stretton further states that the "examining bodies do not demand any special training in this important subject. .... I hope the General Medical Council will give their early attention to the matter." The leading article adds point to Mr. Stretton's remarks, and makes it tolerably clear that the principal reason for his complaint is to be found in the fear of chloroform which is impressed upon students by the teaching of anæsthetics in London. We are told by the writer of the article that the safety of an anæsthetic depends, not on the method of administration, but on "whether or not its power of destroying nerve protoplasm is great or small. That of chloroform is great, that of ether is slight, that of nitrous oxide is least of all." This antiquated doctrine is destitute of the smallest shred of truth, as chloroform and ether when inhaled have no more effect on nerve protoplasm than they have on the protoplasm of the stomach or the biceps. But the moral of such teaching, of course, is that the use of chloroform, the most beneficent of all anæsthetics, should be restricted to the narrowest limits, and in dental operations, at all events, only gas or gas and ether employed. As long as fustian of this kind constitutes the standard teaching in England it is useless to expect any improvement whatever in the confidence and ability of the rank and file of the profession to administer anæsthetics. Nor would examinations improve the matter, since they would only test the proficiency of the student in the teaching which is at the root of the evil. The effects of this teaching, which has been tolerated a great deal too long, are threefold. It has rendered the anæsthetist inefficient—chiefly through fright; it has been the direct cause of an immense number of preventable deaths; and it has tended to debase surgery.

In THE LANCET of the previous week (p. 1383) Dr. Dudley Buxton, who is an authority on the London methods of teaching anæsthetics, states that the anæsthetist is not to be looked upon "as a giver of an anæsthetic. His familiarity with surgery should enable him to know what the operator requires, and what will be the effect of the operator's manipulations upon the patient under anæsthesia. His knowledge of anæsthetics should be such that he can adapt the anæsthetic to the necessities of the patient and the requirements of the surgeon. The tremendous responsibility of the anæsthetist whose want of knowledge, of skill, or of courage may jeopardise the patient's life or frustrate the surgeon's most skilful endeavours should be most fully recognised." The first part of this statement appears to be tantamount to advising the man who is giving chloroform to devote part of his attention, the whole of which ought to be concentrated on the inhalation, to assisting or furthering in some way or other, or "keeping