wholly—probably not even chiefly—by an immediate destructive action upon the cancer cell, but by local and general stimulation of the protective mechanisms of the body. Laboratory investigations as to the effects of varying doses of *x* rays upon cancer masses *detached from their natural surroundings* cannot therefore be in any way relied on to forecast the results of similar doses applied to the living patient.

Experiments designed to raise or lower the immunity of an animal are, of course, in a different category. It has been shown at the Rockefeller Institute in America, and confirmed by Professor Sidney Russ and others in this country, that a single large dose diminishes resistance to cancerous invasion; whereas a series of carefully graduated small doses raises resistance to such an extent that it is almost impossible to get a cancer graft to "take." Whether the rays from a tube "backing up" a 16-inch gap are more effective as resistance-raising agents than those from a tube "backing up" only 10 inches remains to be proved.

The Erlangen procedure, whatever it does to the primary growth, must lower the general resistance, at any rate for a time, thus actually helping any existing metastasis to spread, and likewise any projection of the tumour which may have escaped irradiation. It also produces some skin reaction. The method must stand or fall by itself; it cannot safely be combined with operation. After such dosage he would be a bold man who would submit the skin and subjacent parts to surgical trauma; while as a post-operative procedure it is difficult to see how it can have any *raison d'être*.

Now the tendency at present, in this country, is to seek to combine *x*-ray treatment with surgery. That this can be successfully done, both before and after operation, can be testified by many, including myself. But the large single dose must be eschewed. Dr. Morton tells us that small doses stimulate a malignant growth. Perhaps a single small dose does; I have no experience. But it is certain that seven or eight comparatively small doses, from a Coolidge tube having a 10-inch spark gap, if spread over a period of about three weeks, will cause a palpable diminution in a carcinoma of the breast, and not in any way interfere with the surgeon's work. A 3-millimetre filter is used, the anti-cathode skin distance is one foot, and the patient is not shielded except as to the face. The current in the secondary is 1.5 milliampères and the time fifteen minutes.

The action I do not believe to be primarily a local one. In my opinion the treatment acts much as a course of vaccines—it raises the resistance of the patient to cancerous invasion, and places him in a favourable condition for the operation. As to post-operative treatment, the same idea is carried out. Courses of a dozen to eighteen sittings are given at intervals varying from three months to a year, the object being not to kill off any remaining cancer cells by the direct effect of the rays, but to put the body in a condition to combat their growth. That a series of small doses has this effect in animals was proved in the same set of experiments which showed the harmful effect upon the body resistance of large single doses. It will be noted that this system permits of actually shielding the growth itself from the *x* rays should this be considered desirable.

The results quoted by Dr. Reginald Morton as having been attained at the Erlangen clinic are not wholly convincing—as yet. The number of cases is small, and the observations extend for an average of about three years only. Taken at their face value, they are decidedly better than those obtainable by surgery alone, more especially as regards uterine cancer. Figures for cases treated by a combination of *x* rays and surgery, with pre- and post-operative raying, are not yet available to any great extent in this country, but there is no doubt but that they show an improvement on surgery alone.

It may be that, at no very distant date, uterine and breast cancer will be treated by *x* rays alone; but, even then, it does not follow that the Erlangen technique is the only one by which cures can be accomplished, or that there is no apparatus in Great Britain suitable for the purpose. It must be remembered that, so far, British radiologists have had practically no opportunities of finding out what can be accomplished entirely apart from operation, for the latter has been the rule.

British *x*-ray workers will, I am sure, hesitate long before adopting the heavy dosage advocated in Germany. They remember the skin disasters which were beginning to come to light just before the war as a result of big doses, although the skin had not at the time appeared to be injured; and if in the near future they are called upon to treat cancer apart from surgery, they are likely in the first place to try out less drastic procedures. To condemn the Erlangen technique would be foolish; it is a serious attempt to substitute *x*-ray irradiation for surgical operation in the treatment of cancer of the breast and uterus, and it may be successful; but, in the nature of things, the proof or disproof will occupy many years.

Meanwhile, any attempt to secure the wholesale adoption of the method in this country is to be deprecated. I have already had medical men writing to me to ask if I am sure my doses are big enough—if I am certain that I am not stimulating instead of depressing. Such is the effect of a single letter from a man of Dr. Morton's eminence.

Let us clearly understand that the Erlangen technique has not been designed for use in conjunction with surgery, whereas the attempt to "immunize" the patient by comparatively small doses is based on the results of animal experiments, makes surgical operation more easy, and is without risk of sudden catastrophe. So far it is justified by clinical results, and it is at least worth some years' systematic trial.

The use of *x* rays as immunizing agents in cancer was fully discussed by me in a paper in the BRITISH MEDICAL JOURNAL for June 12th, 1920. The immunity is not specific; it is effective in tubercle, Graves's disease, and other conditions. The conception of *x* rays as resistance-raising agents, rather than substitutes for local surgery, is likely to help much towards that close combination between surgeons and radiologists which is so greatly to be desired.—I am, etc.,

London, W., Feb. 14th. F. HERNAMAN-JOHNSON.

CONFERENCE OF STAFFS OF VOLUNTARY HOSPITALS.

SIR,—I observe that the Leicester motion, which I vainly attempted to oppose at the London Conference, and against which I protested strongly in a letter to the JOURNAL immediately afterwards (January 1st, 1921, p. 31), has been rejected by the Conference in Scotland in favour of one considerably more modest even than that which I put forward myself as an alternative. I trust that the Leicester motion may now be dropped altogether. Rarely indeed has a resolution so detrimental to the voluntary system been propounded, even by its worst enemies. Had the Representatives in London first discussed it with their own lay committees, and had the Chairman refrained from giving that very strong lead which he did, I am persuaded that the ill-considered motion of Leicester would have received in London the same reception that it has now deservedly encountered in Scotland.—I am, etc.,

Chichester, Feb. 27th. G. C. GARRATT.

THE POSITION OF ARMS IN BREECH WITH EXTENDED LEGS.

SIR,—The gain to obstetrics from Dr. Victor Bonney's letter of February 16th, in your issue of February 28th, will be specially recognized: it signifies careful clinical obstetrics; it marks the value of museum evidence which, to some of us, brings its daily lesson.

The paper by Dr. W. S. A. Griffith and the late Dr. Arnold W. W. Lea on breech presentation with extended legs, in the Obstetrical Society's *Transactions*, 1897, brought the best of clinical information.

Since that time I have collected relative museum material. Some of the material, the undisturbed foetus with extended legs at four and a half months within the intact amnion and chorion, and at seven months within the uterus, both confirm Dr. Victor Bonney's description of the extended arms, only partial however, and confined to the lower hemisphere of the head, amounting to this—that if the vertex presented the foot and the hand would be palpable at the periphery of the head.

Eight years ago, after discussing this fact with Sir Francis Champneys, I was supplied by him with the notes of one of his forceps deliveries in August, 1882, embodying these anatomical facts, which so often escape published description and, probably nearly as often, clinical observation. At that time it was my wish to

have the opportunity of displaying what is now well, if not widely, understood in teaching schools. Of late years the cost of illustration has been prohibitive.—I am, etc.,

HENRY BRIGGS.

Department of Obstetrics and Gynaecology, University of Liverpool, March 1st.

SIR,—As the medical man unsuccessfully sued for negligence whose experience Mr. Victor Bonney refers to in his letter (February 26th, p. 320) may I mention the following facts?

The case I had to conduct was one of delayed descent with the os dilated. Under chloroform I introduced my hand, and found a breech with fully extended legs (sacro-posterior breech not engaged). Passing my hand above the knee to flex and coax down the leg, I felt a displaced arm passing upward, and at once thought "if I leave that I shall get extended arm when the legs are born," so I followed the arm and found that hand lying flat on the parietal bone. Then I sought the other arm, and found it also displaced upward, elbow forward, forearm above ear, and hand under occiput; I first replaced both arms, then pulled down both legs, the child being large, turning axially at the same time, and delivered without difficulty, the arms coming out in the natural position as replaced.

I have always doubted the truth of the textbook teaching that extension of arms in a breech case can be caused in any way if the arms are in the natural folded position while *in utero*. I believe that in all cases of extended arms there must be some precedent unusual position of the arms; this belief makes it more, not less, important to verify the position of the arms as soon as the umbilicus appears in a breech case. It may mean we ought to verify their position still earlier in labour.

Having in this case lighted on some evidence in support of my belief, I wrote to seven eminent gynaecologists suggesting faulty position of arms *in utero* as the true and main cause of arm extension, and asking if they had ever had occasion to discover the position of the arms *in utero* in a case which subsequently showed extended arms.

One replied that he had sometimes found arms displaced much as I describe; none of them agreed with my theory, but none of them had a case in which, after examination had shown the arms in normal position, the arms subsequently became extended. I suppose they would say because they never pulled unduly, but I suggest that undue pulling will only be followed by extension of arms if the arms are so abnormally placed that extension would occur without undue pulling, or possibly if the pulling is so vigorous that it sets up a reflex causing the child to displace its arms *before* they reach the brim.

Mr. Bonney now suggests that in every case of extended legs the arms are abnormally placed. I recently had a case of a very small child born alive with ease and with extended legs unreduced (I was called late), in which the arms were normally placed. This does not affect the wisdom of Mr. Bonney's suggestion that in all cases of extended legs the position of the arms should be ascertained, and, if misplaced, replaced. The question in my mind is whether we ought not to go further, and ascertain arm position in every breech case which we see early enough to make so doing easy. I have never yet seen any trouble follow the introduction of my hand into the uterus, and children are lost from arm extension. I think it is debatable. Can any reader record a case of arm extension in a breech case in which he had previously proved the position of the arms to be normal, or subsequent extension in a case where he had replaced misplaced arms? I admit that negative evidence cannot prove my theory, but I maintain it until positive evidence disproves it. I suppose every experienced practitioner has seen cases of arm extension in which no unwise pulling was indulged in, and it is really time that the school theory that such pulling is the main cause of arm extension was given up.—I am, etc.,

T. H. GODFREY.

Finchley, Feb. 25th.

THE DROOPING SHOULDER SIGN OF PHTHISIS.

SIR,—In my book, *Tuberculin in the Diagnosis and Treatment of Tuberculosis*, one reads on p. 72, line 16, these words: "This 'habitus phthisicus' manifests itself in the thin, weakly, weedy, long, lanky individual with stooping gait and *drooping shoulders*, a narrow flat chest, wide oblique intercostal spaces, moving but little (paralytic chest), and wing-like shoulder blades projecting outwards." Certainly Dr. Rivers has not read my book. Comment is unnecessary.—I am, etc.,

W. CAMAC WILKINSON, M.D.Lond., F.R.C.P.

London, ., Feb. 15th.

SIMPLE GOITRE IN SCHOOL CHILDREN.

SIR,—When examining school children in Hampshire I have been struck by the large number of cases of slight enlargement of the thyroid occurring in both boys and girls below the age of puberty. This condition would appear to be much more common in this country than is generally supposed. The thyroid as a rule is not conspicuous, and is seen only if specially looked for. The children are perfectly healthy and robust, but upon comparing their size and weight with those of other children of a similar age, it will be found that they are almost invariably below the average. The condition consequently is not easy to detect unless a number of children of the same age are being examined at once.

Such children are never brought to a doctor for treatment, as they are not ill. The parents frequently do not realize that the child is small for its age, or if so, it is put down as a family peculiarity. I have had no opportunity of treating such children, but should they come under medical treatment for some other condition, it would be well worth while putting them on a course of thyroid or iodide, as was done in the South of France, in the hope of improving their growth.—I am, etc.,

WILLIAM A. LETHEM, M.C., M.D., D.P.H., Assistant M.O.H. County of Hampshire.

Brockenhurst, Feb. 27th.

Obituary.

ERNEST COURTNEY LOMAS, C.B., D.S.O., Surgeon Captain R.N.

SURGEON CAPTAIN ERNEST COURTNEY LOMAS, R.N.(ret.), died at Pencaitland, East Lothian, on February 24th. He was born on December 24th, 1864, and educated at Owens College, Manchester; he graduated M.B. and Ch.B. of Victoria University in 1888, and took the M.R.C.S. in that year and the F.R.C.S. in 1907. After filling the posts of house-surgeon to the Manchester Royal Infirmary, of senior house-surgeon to the Royal Albert Edward Infirmary, Wigan, and of resident medical officer of the Barnes Convalescent Hospital, Cheadle, he entered the navy as surgeon in 1891. He was specially promoted to staff surgeon in 1900 for service in the South African war, became fleet surgeon in 1904, surgeon captain on September 11th, 1918, and retired in 1919. He served with the Naval Brigade in the South African war, taking part in the relief of Ladysmith, was mentioned in dispatches, and gained the Queen's medal with two clasps, a special promotion, and the D.S.O. During the recent war he was medical officer in charge of three hospital ships in succession. He contributed a description of the equipment and working of hospital ships to the special series of articles published in the BRITISH MEDICAL JOURNAL and afterwards collected in the volume *British Medicine in the War*.

Sir HUMPHRY ROLLESTON writes: Surgeon Captain E. C. Lomas's long and painful illness, by necessitating his retirement, was a severe blow to the Naval Medical Service, not only on account of his surgical and administrative abilities, but also for the loss of a specially attractive and lovable personality. He won the D.S.O. for the relief of Ladysmith in the South African war in 1900, and had a distinguished record in the service, his war work being recognized by the C.B. in 1916. He was senior medical officer of three hospital ships in succession; the *Maine* was wrecked off the coast of Scotland before the war, but he was not on board at the time on account of his wife's illness; the *Rohilla* was wrecked off the East Coast late in 1914, and Lomas suffered considerably from exposure before he would leave; the *Garth Castle* was a model and happy ship under his genial and tactful guidance. He subsequently organized and, in October, 1916, opened the Royal Naval Hospital at Granton, near Edinburgh, where I often saw him. Not only was he a most efficient organizer and, until administrative duties