

After all these years there must be a mass of evidence obtainable, the careful consideration of which by an authoritative body of men should result in a report which could not fail to be of immense value. Possibly such a report would find that the isolation hospital was not so beneficial as is generally conceded; on the other hand, it might indicate improved methods of administration whereby the usefulness of such institutions could be greatly enhanced. In any case such an inquiry can do no harm and, in view of the diversity of opinion on the effect of the isolation hospital in controlling scarlet fever, appears to me eminently desirable.

I am, Sirs, yours faithfully,

A. MEARNS FRASER, M.D.,

Medical Officer of Health, Portsmouth.

Portsmouth, Dec. 22nd, 1903.

WATER-CRESS AND TYPHOID FEVER.

To the Editors of THE LANCET.

SIRS,—I have read with interest the admirable report of Dr. J. King Warry on an outbreak of typhoid fever in Hackney in June last and also your editorial comment in THE LANCET of Dec. 12th, p. 1671, and lastly the criticism of Dr. William Bruce in THE LANCET of Dec. 26th, p. 1834. May I say at once that in my opinion Dr. Warry's conclusions appear to be amply justified by the facts he has collated and that Dr. Bruce's criticism appears to me to be an apt illustration of a logical fallacy.

Dr. Bruce says: "If eating water-cress was the real reason of the outbreak surely the first evidence would require to be that *everyone* attacked partook of the article in question." (The italics are mine.) How can Dr. Bruce advance this argument when he believes (as, indeed, all persons with any considerable experience of typhoid fever *know*) that the disease is frequently "spread by infection from person to person." Surely the earlier cases in Hackney may have been infected by the polluted water-cress while some of the later cases may have been cases of personal infection and yet some of them indirectly attributable to the infected water-cress. Considering the various channels by which the typhoid bacillus may reach the alimentary canal it is preposterous to advance the view that everyone attacked should have partaken of the suspected article. Some of the cases were probably quite independent of the water-cress. I do not suppose for a moment that Dr. Warry suggests that the whole of the cases were traceable to the polluted water-cress but merely that that infected article was the origin of the excess of typhoid fever in June, 1903, as compared with the average June incidence in previous years.

Dr. Bruce does not seem able to grasp how one sample of water-cress may be infected and another innocuous, even though both may be obtained from the same polluted source. In the first place, typhoid bacilli are particulate matter in suspension (and *not in solution*) in the contaminated water; they are not, therefore, present in every drop or cubic centimetre, or even litre, of the contaminated water; hence *only portions* of the water-cress growing in the sewage-polluted stream become contaminated with typhoid bacilli. In the second place, even an infected portion of water-cress may be rendered innocuous by being very thoroughly washed under a running tap. Some persons are more careful and cleanly than others and wash their water-cress more thoroughly. Such persons may thus get rid of the particulate pathogenic bacteria by the mechanical force of a stream of water. Dr. Bruce again says: "The specific bacillus has never once been demonstrated as present in any single sample of suspected water." Though I would be extremely cautious as to the use of the word "never" it may be granted that it would be quite exceptional to find the typhoid bacillus in a suspected water. Has Dr. Bruce never reasoned as to why this is so?

I have already shown above that not every litre of a contaminated water would necessarily contain a few particulate bacilli, while most bacterioscopic examinations of water are carried out with only fractions of a cubic centimetre. Apart from this consideration it is a question to be decided whether the specific pollution of the water is a continuous pollution or was of a part and temporary nature. More often it is the latter and by the time that cases of typhoid fever have arisen in sufficient numbers to cause suspicion to be attached to a particular water-supply several weeks have probably elapsed since the date of specific pollution. It is therefore not a matter for surprise if the specific bacillus is not to be found,

but if the bacterial examinations afford ground for suspecting the presence of sewage pollution (even if the specific bacillus is not isolated from the small quantities which only can be plated out) the likelihood of the suspected water being the probable vehicle of the poison is greatly strengthened.

Again, as regards shell-fish from polluted layings it is by no means every shell-fish which becomes infected with the particulate specific bacteria but only such as happen to imbibe a particular portion of infected water as it passes. The typhoid bacillus *has been isolated* by Klein from polluted shell-fish but the majority of the shell-fish examined from the same layings revealed only some of the more common sewage bacteria. All, however, are potentially dangerous. Just so with water-cress from polluted streams: all is not actually infected with the specific germ but all must be considered as potentially dangerous. That secondary infection from previous cases is under-estimated I firmly believe and assert and on this point I am in agreement with Dr. Bruce.

In my annual report on the health of the borough of Southend-on-Sea and again in a paper which I read before the Royal Institute of Public Health at Liverpool in July, 1903, I have reiterated the following precautions which I think if followed would very greatly reduce what I term the "residual" typhoid fever in this country: 1. By avoiding the *touching* as well as the eating of all forms of shell-fish except such as are beyond any suspicion of pollution. 2. By avoiding the eating of any uncooked vegetables (such as water-cress) which have been subjected to manurial or sewage pollution. 3. By the early removal to hospital of any case of typhoid fever which may occur (a large proportion of cases are secondary consequent upon the nursing of patients by untrained relatives). 4. By careful and constant attention to effective sewage schemes and to frequent and effective disposal of trade and house refuse, &c. 5. By all forms of cleanliness, which must apply particularly to our water-, milk-, and other food-supplies and more particularly to shell-fish and water-cress than hitherto. To these I may add another which I mentioned in my annual report for 1901. 6. The routine administration to typhoid fever patients of urotropin before they pass from professional observation. Yet a seventh preventive measure is—Warning convalescent patients of the possibility of their excreta being infective for a considerable time after recovery and instructing them in simple methods of disinfection and other precautions to be observed.

I am, Sirs, yours faithfully,

J. T. C. NASH, M.D. Edin., D.P.H. Camb.,

Medical Officer of Health, Borough of Southend-on-Sea.

Dec. 29th, 1903.

THE TREATMENT OF CERTAIN FORMS OF OBSTINATE CONSTIPATION BY ENTERO-COLOSTOMY.

To the Editors of THE LANCET.

SIRS,—In THE LANCET of Dec. 12th, 1903, p. 1673, I notice that you mention a new method of dealing effectually with cases of intractable constipation which has been devised and successfully carried out by Mr. W. Arbuthnot Lane. At a meeting of the Medical Society of London on April 22nd, 1901, I read a paper on a precisely similar case in which I had successfully performed the same operation in October, 1900. I saw the patient a year later and am glad to say that the operation (it was the fourth occasion on which laparotomy had been performed upon her) had completely relieved her of her symptoms.

I am, Sirs, yours faithfully,

Wimpole-street, W., Dec. 24th, 1903. C. MANSELL MOULLIN.

THE SIMULTANEOUS OCCURRENCE OF TWO ACUTELY DEVELOPED CUTANEOUS LESIONS.

To the Editors of THE LANCET.

SIRS,—The interesting case of concurrent psoriasis and erythema nodosum described by Dr. H. W. Syers in THE LANCET of Dec. 19th, p. 1718, furnishes yet another illustration of the principle of co-existence in diseases of the skin. The possibility of the development of two, or even more, acute cutaneous disorders side by side is one which is very apt to be overlooked clinically, but it is a fact that this association occurs more frequently than is generally

believed. I have been accustomed to divide the cases into those of: (1) natural—i.e., causally related; and (2) accidental association. As an instance of the former may be mentioned the acute development of alopecia with a seborrhoeic dermatitis upon the trunk, while as an illustration of the latter I may refer to a case which I have described in detail elsewhere¹ of a man, aged 40 years, who came to the skin department of the Tottenham Hospital suffering from an acute attack of psoriasis. Within a short time he developed an eruption of secondary syphilis which was perfectly distinct and separate from his psoriasis, his skin presenting a most unusual spectacle. Curiously enough, it is psoriasis, the "disease of the healthy," which most often co-exists with other independent cutaneous affections and it is generally the one first in the field, the second disease appearing shortly after its efflorescence. Syphilis, purpura, and leprosy have all been observed to co-exist with psoriasis, but, with the exception of the first, they are rare. In Dr. Syers's case the erythema nodosum appears to have developed quite simultaneously with the psoriasis. Although there was no history of rheumatism in the case, yet it will be admitted that the concurrence of two such cutaneous disorders, both of which are so frequently associated, directly or indirectly, with that disease would form a strong reason for treating such a case with salicin or the salicylates. As was originally pointed out by Dr. Radcliffe Crocker, these remedies often exert a favourable influence upon the course of psoriasis alone. Regarded from the diathetic standpoint Dr. Syers's case would fall into the category of my first group—namely, those concurrently developed acute cutaneous disorders which are "naturally" associated through a more or less causal relationship.

I am, Sirs, yours faithfully,

G. NORMAN MEACHEN, M.D., M.R.C.P. Lond.
New Cavendish-street, W., Dec. 18th, 1903.

MALIGNANT GROWTHS AND NORMAL REPRODUCTIVE TISSUES.

To the Editors of THE LANCET.

SIRS,—In THE LANCET of Dec. 26th, p. 1830, you give a very full account of the interesting paper recently communicated to the Royal Society by Professor J. B. Farmer, Mr. J. E. S. Moore, and Mr. C. E. Walker in which they dwell on the resemblances between certain of the cells of malignant growths in man and those of normal reproductive tissues, and from which resemblances they draw the conclusion that the malignancy of these tumours is intimately connected with the reduction of the normal somatic cells into reproductive or germinal tissue. To me this communication is of special interest as I have already put forward a similar view in my paper on Inoperable Mammary Carcinoma read before the Edinburgh Medico Chirurgical Society in May, 1896, and published *in extenso* in THE LANCET of July 11th and 18th of that year. I there say, when dealing with the etiology of cancer, that "in whatever way brought about there seems to me a reasonable ground for thinking that the active processes seen in a cancerous tumour are best explained by regarding the epithelium of the part as having taken on the properties and powers of the germinal epithelium." I further expressed the belief that the special cells seen in sections or scrapings of cancer and known as "cancer bodies" may eventually be shown to be special germinal cells corresponding to the ovum cells elaborated by the ovary in the female and by the testis in the male.

While Professor Farmer and his coadjutors seem to have come to the same conclusion as the result of their microscopical inquiry I did so clinically and chiefly in consequence of the results obtained by oöphorectomy in inoperable mammary cancer. Under that procedure cancerous activity has been arrested and proved cancerous tissue has disappeared with, in addition, diminution of pain and improvement in the general health, a state of matters that has never before been observed in this disease under any other medication and that, too, in advanced cases of the malady. In writing thus I am not giving only my own individual experience. Other surgeons have observed the same results. Unfortunately, I think, the profession has regarded this method of treatment from a therapeutic aspect only and judged it solely by the standard of its value as a *cure* agent. This is, of course, quite right, but whatever be the final decision on this point

I would fain hope that the effects following oöphorectomy will not be lost sight of, especially as we are dealing with a disease the true nature of which has, so far, baffled inquiry. To cause cancerous tissue and cancerous glands to disappear is a very remarkable fact and one that should have more importance attached to it than it has yet had, for I look upon it as furnishing a clue to the real nature of cancer and as strongly confirmatory of the opinion I expressed in May, 1896, "that the disease consists in the epithelium of the part affected taking on the active proliferation which is the marked characteristic of the germinal epithelium." The observations of Professor Farmer and his coadjutors seem to support this view of mine, which, however, was arrived at by a different line of inquiry.

I am not sure that Professor Farmer and Mr. Moore and Mr. Walker are altogether correct in the statement they make as to the exact mode of development of the special cells to which they give the term "gametogenic," but a letter such as this is not the place to go into such a question. The mere establishment of the presence of such cells in malignant tissue is a point of the greatest importance and strongly corroborative of what I may call the *germinal-cell theory* of cancer.

I am, Sirs, yours faithfully,

Glasgow, Dec. 27th, 1903.

GEORGE THOS. BEATSON.

THE LATE FLEET-SURGEON KIRKER, R.N.

To the Editors of THE LANCET.

SIRS,—In the sympathetic notices of the late Fleet-Surgeon Gilbert Kirker no mention seems to have been made of that officer's work on the effects of the modern rifle bullet of small bore. Dr. Kirker's observations were communicated by the late Professor T. Longmore to the Section of Military Medicine and Surgery of the International Medical Congress of 1881 and have since lain buried in the ponderous volumes of the Transactions of that meeting.¹ A careful study of his paper, entitled "A Comparison of the Cylindro-conoidal and Round Bullet Wounds," would have rendered many of us less free in the expression of surprise at the results of the modern bullet wound as observed in the late war. The late Dr. Kirker, I know, felt that his work had scarcely obtained the recognition it deserved and it seems only right that, although late, attention should be drawn to his pioneer work in this direction.

I am, Sirs, yours faithfully,

Dec. 30th, 1903.

G. H. MAKINS.

THE FUNCTIONS OF THE GENERAL MEDICAL COUNCIL.

To the Editors of THE LANCET.

SIRS,—In reference to your frequent allusions to rampant quackery, is it not possible for the General Medical Council to take some practical steps for the protection of the medical profession and the public? No doubt the Council's work is good as far as it goes but I express the feeling of many other medical men when I say that we think the professional needs of our class do not receive adequate attention. You say in your section of the Annus Medicus dealing with the Council that the preliminary education of the student and the consideration of penal cases have absorbed all the time that the Council can afford to give to its deliberations. In that case Parliament should grant a subsidy to the Council, for it is clear that other subjects remain for discussion and no less clear that there are abuses to be reformed.

I am, Sirs, yours faithfully,

Dec. 28th, 1903.

M. D. CANTAB.

* * Our correspondent will find his question dealt with in a leading article on p. 37.—ED. L.

THE UNIVERSITY OF LONDON AND MEDICAL EDUCATION.

To the Editors of THE LANCET.

SIRS,—Your leading article, which appeared under this title on Dec. 19th, 1903, sufficiently indicates the great educational work which lies before the reconstituted University of London as well as the extreme importance of that work to the medical profession.

While congratulating the University of London upon the

¹ Medical Press and Circular, vol. i., 1902, p. 773.

¹ Transactions of the International Medical Congress, 1881, vol. ii., p. 578.