

three years does not reflect credit on those concerned, especially the civil and military heads of the War Office."

The public attention has been drawn to the three aspects of the question:—

1. *The medical aspect.*—I have shown them that in every case where large camps, military or civil, have placed themselves under a scheme of sanitation only distantly approaching the completeness of that which I have proposed these camps have been practically exempt from these evils. This statement the public accepts on my authority as a Doctor of Medicine. If this statement should be incorrect scientifically I shall be glad if you will correct it.

2. *The military aspect.*—I have pointed out to the public that the consideration of this scheme in its military aspects—transport, efficiency, discipline, &c.—has resulted in the unanimous support of a large number of military experts representing all branches of the army two years ago at the Royal United Service Institution and that the scheme has the continuous strong support of leading military journals and many prominent soldiers.

3. *The disciplinary aspect.*—The possibility of educating every man in the army up to the point of keen interest in every detail of the scheme. Practically every journal in the country has expressed confidence in the power of the scheme to attain this end. This aspect of the scheme is essentially one to be decided by the public at large.

Having considered these three statements, or aspects, the public are asked to express strongly their determination that they insist upon the principles of prevention which guide civil authorities being applied immediately to the army without further delay.

The particular question before us is the propriety of the *Times* publishing my letter and commenting upon it, which naturally follows on the impropriety of my writing to the *Times*. Seeing that the subject has already been fully discussed in its medical aspects before two important medical bodies and in numerous articles, in its military aspects before the chief military institution for technical discussion of military subjects, and in its disciplinary aspects by the entire press of the country it seems amazing that the *Times* should be incompetent to review the position after these discussions. Such an assumption—naturally excluding from the columns of that journal discussions on every branch of technical science, medical, legal, military, naval, agricultural, clerical, or fiscal—however desirable in an ideal State, would be a manifest disaster to this country, governed, moved, and animated as it is by public opinion. I am of opinion that in these circumstances to have assumed, as official experts once did, that there was no remedy for these evils and to have palliated by silence the delay of the War Office in applying suitable remedies would have been a public wrong on my part and on the part of the press, military, medical, or lay, if it had been tempted to this course.

Leaving the ethical for the technical considerations suggested by your article, your article assumes that I have "attributed a too exclusive influence to water-borne contagion, whilst overlooking the risks which attend direct contact with the infected excreta." In the letter to the *Times* referred to it is expressly stated that I "insisted upon the necessity of a 'pioneer section' specially trained in methods of camp sanitation" and at the Medical Society of London last year I stated in reply to the discussion on my paper that "I regarded no system of camp sanitation which dealt only with protected water as satisfactory unless every other avenue of typhoid fever—flies, dust, contagion, &c.—was also closed." A system which in addition to protected water required also a special service¹ organised and trained in the prevention of infection through polluted soil, latrines, &c., insisted on minute attention to details of cleanliness by the men, required also the rapid isolation of all cases of intestinal disease or suspected cases of typhoid fever with the sterilisation of their excreta, clothing, tents, &c., can hardly be regarded as a limited view of the etiology of typhoid fever or as "overlooking the risks which attend direct contact with the infected excreta."

I am therefore in full accord with you when you state that "it would be ill-advised to place any confidence in a plan of campaign against typhoid fever which concentrates the attention upon the prevention of indirect contagion by polluted water and which takes little account of the

formidable dangers of direct faecal contamination." Such a plan of campaign I am totally unacquainted with and bears no resemblance to the scheme I have proposed. It is true that the part of my scheme dealing with protected water has been given special prominence as practically all the difficulties of the scheme centre round the rapid continuous service of sterilised water to vast bodies of men, and without this difficulty being overcome no scheme of sanitation is imaginable. I am not aware that the work of Professor Koch in the infected villages of the western frontier has revealed any new avenue of typhoid fever to armies that has not been fully considered and provided for in the scheme I have proposed. Three years' study of this subject with actual practice and entire success in the prevention of the diseases referred to in large bodies of men and in communities previously badly infected has shown me that the knowledge already exists for effective preventive technique, without any "consensus of expert authority" (which could be produced in abundance if required) or without any further delay in favour of "continuous research."

I am, Sirs, yours faithfully,

Sept. 21st, 1903.

LEIGH CANNEY, M.D. Lond.

A MANUAL OF MEDICINE.

To the Editors of THE LANCET.

SIRS,—While heartily thanking you for your careful examination and very kindly criticism of my "Manual of Medicine," I may perhaps be allowed to comment upon the two points with regard to which my accuracy or my judgment is called in question. The sentence in which reference is made to enteric fever and tuberculosis does possibly err in the direction of dogmatism, but so far as the prolonged febricula of childhood is concerned I range myself with those who regard it as being actually enteric. As to fixing a definite period of quarantine for all cases which are convalescent from a given infectious fever the general practitioner who tries it, say, in scarlet fever, is likely to find before long that the task is beyond his strength. The difficulty, first of ascertaining the minimum safe period, and secondly of enforcing it in a mild case in an adult, goes far to explain the fact you note—namely, that the question is not generally dealt with in the text-books. With medical officers of schools the matter is very different, but they have the recommendations of their own association to guide them. The opinion of THE LANCET, however, will be borne in mind.

I am, Sirs, yours faithfully,

Glasgow, Sept. 21st, 1903.

T. K. MONRO.

A CASE OF MALIGNANT DISEASE FOLLOWING SPLENIC ANÆMIA.

To the Editors of THE LANCET.

SIRS,—The case recorded in THE LANCET of Sept. 19th, p. 813, under the above heading is of interest from a clinical and diagnostic point of view, but until Dr. C. H. Sedgwick gives further evidence to uphold his diagnosis he surely cannot expect any great acquiescence in his conclusion that the malignant disease in this case supervened on splenic anæmia. One is at a loss to discover what peculiar symptoms in the course of his patient's illness and what results from blood examinations or from post-mortem examination lend any support to this expression of diagnostic acumen. The case, I take it, from the excellent notes provided, was probably one of primary malignant disease of the liver. The enlargement of the spleen with diminution in its size after a bleeding, the hæmatemesis, and the melæna are but the common evidence of a somewhat acute portal obstruction situated in the liver and caused by rapidly progressive malignant disease of that organ. The reports of the blood examinations support only the diagnosis of severe hæmorrhage and the venous congestion of the spleen explains its size and post-mortem appearance. The morbid physical signs and symptoms displayed by Dr. Sedgwick's patient can most simply and amply be explained by such a diagnosis as I offer, and certainly there is nothing in the case as reported to support the very haphazard one of splenic anæmia.

I am, Sirs, yours faithfully,

Edinburgh, Sept. 19th, 1903.

GEO. S. CARMICHAEL.

¹ Royal United Service Institution, November, 1901. Medical Society of London, October, 1902. *Times*, Sept. 1st, 1903.