

become the renegades. Their colonies are despatched through the blood stream.

Is it possible to cultivate the parasites outside the human body? The answer to this question is: Probably the only possibility of cultivating a cancer or sarcoma outside the living body would be by placing an aseptic living section of a tumour in a constantly changing nutritive medium containing oxyhæmoglobin maintained at the body temperature. The difficulty of this undertaking, if not insuperable, would be extremely great. Where the parasites exist outside the human body is hard to surmise, but judging by analogy some insects, possibly flies or water animalcula, have something to do with their transmission.

It may seem premature to theorise about the parasites of malignant disease before they are definitely proved to exist but it is probable that their existence will never be mathematically proved according to Koch's data, yet most of us are morally certain that they are there. Action, it is said, must be based on theory. Sir Thomas Watson, long before the bacteriological age, held that tetanus was caused by the gradual manufacture of a strychnine-like poison within the body. Sir Patrick Manson, long before it was proved by Ross, was convinced that the malarial parasites were carried by mosquitoes. A year before the malarial flagellated bodies were proved to be spermatozoa I suggested that they were such from their peculiar movements.

I am, Sirs, yours faithfully,

J. H. WHELAN, M.D. R.U.I.,
Fleet Surgeon, R.N.

Oct. 1st, 1903.

THE CARDIAC REFLEX OF ABRAMS.

To the Editors of THE LANCET.

SIRS.—Anent a recent annotation in THE LANCET¹ on the Cardiac Reflex of Abrams, one is constrained to conclude that the reflex in question can be elicited without the aid of the Roentgen rays. The latter observation, however, is only partially true, for the very evident reason that the cutaneous irritation necessary to produce the heart reflex will contemporaneously induce the lung reflex of dilatation which of itself will reduce the area of superficial cardiac dulness. The lung reflex being of shorter duration than the cardiac reflex, persistent reduction in the area of superficial cardiac dulness after a space of two minutes (a time exceeding that necessary for the lung borders to recede) would indicate that the diminished cardiac dulness was due to reduction in the heart volume and not to lung dilatation. In soliciting the aid of the cardiac reflex in diagnosis, prognosis, and treatment I almost invariably place sole reliance on the Roentgen rays with the fluorescent screen approximating the anterior chest wall. Unless one is specially skilled in mapping out the deep or relative cardiac dulness the Roentgen rays and not percussion should be our guide. You mention in your annotation that Merklen and Heitz of Paris have extended my observations and I beg to direct your attention to further facts concerning this reflex which I have elaborated since the publication of my original contributions on the subject. I have studied the reflex as an index to the condition of the myocardium and find in brief that when the cardiac muscle is beyond restitution in myocarditis and valvular diseases the cardiac reflex cannot be elicited. Heretofore this reflex was only observed in the transverse cardiac diameter, but it may also be noted in the sagittal diameter. The recognition of the heart reflex will often aid us in excluding the murmurs of a relative insufficiency. Here vigorous rubbing of the præcordium will temporarily dispel the latter murmurs. The principle involved in the latter manœuvre is essentially this: in relative insufficiency of the valves they have undergone no anatomic change but are no longer able to close the enlarged orifices of the heart, therefore vigorous friction of the præcordium by inducing the heart reflex will contract the myocardium and the orifices and will allow closure of the latter by the valves, at least temporarily. One may elicit the heart reflex by irritation of more remote regions. Thus one may easily observe by aid of the rays that when ammonia is inhaled there is decided recession of the cardiac ventricles (heart reflex), especially the left, and that this reflex may be more pronounced than when discharged through the skin of the præcordium. Ether and chloroform produce a similar though less pronounced effect. With the

nose closed a similar though less pronounced effect of the vapours may be obtained, presumably by their action on the pharyngeal and laryngeal mucosa. In a few instances the vapours produced a veritable heart inhibition. I could observe no diminution in the intensity of the heart tones during the inhalation of the vapours, although sphygmograms showed a decided difference in the output of blood into the general circulation before and after the inhalation of ammonia. These observations suggest the wise expedient of cocainising the nasal mucosa before using an anæsthetic and further suggest the cogent necessity of anæsthetising the pharyngeal and laryngeal mucosa.

I am, Sirs, yours faithfully,

San Francisco, Sept. 18th, 1903.

ALBERT ABRAMS.

TAKA-DIASTASE IN GOUTY DYSPEPSIA.

To the Editors of THE LANCET.

SIRS.—Every practitioner has under his care at various times very troublesome cases of dyspepsia associated with the gouty diathesis and the difficulty of keeping patients to a suitable dietary is well known. The stomach seems to digest well up to a certain point and appears to assimilate proteid matter far more readily than fats and carbohydrates; in fact, it is the faulty digestion of the latter, together with the sustained irritation caused by the decomposition of their by-products, that prevents the organ returning to a healthy tone. On the other hand, it is unquestionable that a diet almost exclusively composed of milk and farinaceous products is the one most suitable to produce permanent amelioration of the effects of uric acid saturation, and it is essential to put the stomach in a condition fit to receive and to use such foods. I have used salicylate of bismuth before meals for this purpose with good results as to relief of symptoms so long as its use was continued, but its curative action is practically *nil* and the mucous membrane reverts to the same irritable and atonic state when the drug is discontinued. Some samples of taka-diastase (Parke, Davis and Co.) were sent to me with notes on its value as a diastatic ferment with powerful action on starch. I tried it in two and a half grain tablets with a patient who had long suffered from this form of dyspepsia, with very gratifying results, and have since ordered it to be taken regularly after meals in this class of case until a satisfactory condition has been re-established. I should be glad to hear if my opinion on the value of this preparation is borne out by other practitioners and can heartily recommend a trial from my own experience of it.

I am, Sirs, yours faithfully,

L. WHEELER, M.B., B.Ch., B.A.O. R.U.I.

London, Sept. 30th, 1903.

FEVER AND CONVULSIONS DUE TO ASCARIDES.

To the Editors of THE LANCET.

SIRS.—Round worms in children of the poorer class are very common in the district round St. Day, near Redruth, and I have seen several cases of fever and convulsions, a few cases of incontinence of urine, and one case of chorea which could be traced to this condition. The children frequently vomit the worms and they are passed per rectum often in clusters. I used to put the cause down to the celebrated Cornish pasty which is the main food of the lower class. The outer covering of the pasty is often very lightly baked, while the contents are of various materials from underdone meat to cabbages and rotten potatoes. I imagine most medical men in practice in Cornwall have seen many peculiar symptoms which they have credited to the ascaris lumbricoides and it would be interesting and instructive to hear what, in their opinion, is the cause of this endemic disorder in the extreme south-west of England.

I am, Sirs, yours faithfully,

Streatham, S.W., Oct. 4th, 1903.

E. H. WORTH.

To the Editors of THE LANCET.

SIRS.—Surely "worm fever" is not a complaint of extreme rarity, even though, as you note in THE LANCET of Sept. 26th (p. 898), "it is not recognised by British writers." Two cases, both of much interest at the time, have occurred in my short experience. Of these, one was that of a child six or seven years of age whom I admitted to the wards of a children's hospital as a case, probably, of typhoid fever, a

¹ THE LANCET, August 29th, 1903, p. 619.