

growth brings all her inflammatory forces into line. X rays and radium act by destroying a certain number of malignant cells outright, but they act mainly as a stimulant to the healthy cells of the body to urge them on to still greater inflammatory activity.

The best way to investigate the problems is coöperation between the pathologist and radiologist. Both should be men of experience, and both should have a sceptical mind. My experience of most radiologists is that they do not study pathology enough and are too eager to attribute changes found in tumours to the action of rays alone.

I am, Sir, yours faithfully,

ERNEST H. SHAW.

Camden-road, Holloway, N., Oct. 22nd, 1922.

EPIDIDYMITIS IN MALARIA AND DYSENTERY.

To the Editor of THE LANCET.

SIR,—I should feel greatly obliged to any colleague who can refer me to recorded instances of subacute or chronic epididymitis due to malaria or dysentery, or who has met with such cases. Further, if malarial, whether this condition is favourably influenced by quinine.—I am, Sir, yours faithfully,

P. WATSON-WILLIAMS.

Clifton, Bristol, Oct. 23rd, 1922.

DEATH IN THE ALPS.

To the Editor of THE LANCET.

SIR,—Dr. W. J. Rutherford¹ shows perturbation at his discovery that the theory of the unconscious explains those phenomena attributed to demoniacal possession. Concerning the theory I once wrote²: "If the reader thinks this conception brings us back to the old one of demoniacal possession I will admit that the only difference lies in the definition of the demon;" and the critics said my views were not original. Why is Dr. Rutherford so perturbed, instead of being justifiably proud of his acumen? And why make it a reproach that the psychogenetic theory of epilepsy finds support in Holy Writ?

I am, Sir, yours faithfully,

MILLAIS CULPIN.

Queen Anne-street, W., Oct. 21st, 1922.

CHRONIC ENDOCERVICITIS AND ITS TREATMENT.

To the Editor of THE LANCET.

SIR,—I have read with considerable appreciation Dr. J. W. Burns's article on the above subject in THE LANCET of Oct. 14th, and find myself in agreement with most of his conclusions. There are one or two points, however, on which I should like to comment.

First, as to the bacteriology. I am afraid, with the technique described, the chances of discovering the true percentage of cases due to the gonococcus are remote. The gonococcus is notoriously difficult to find in smears from the endocervix, and still more difficult in cultures. The complement-fixation test I have found practically valueless in women. The difficulty, therefore, of saying definitely whether or not a case of chronic endocervicitis is gonococcal, and still contagious, or non-gonococcal, and unlikely to set up a urethritis in the male, is very great.

As a help in diagnosis it will be found that if a gonococcal proteose be injected subcutaneously, and a smear taken 48 hours afterwards, gonococci will often be discovered in smear and culture from cases of this nature which would otherwise have been pronounced due to secondary organisms only. If the

proteose is not available, swabbing out the endocervix with 2 per cent. silver nitrate, and taking a smear 24–48 hours afterwards, often gives similar results.

In addition, the composition of the media used is of extreme importance. Even when the gonococcus is known to be present, I have found, time and again, that competent bacteriologists, coming to the London Lock Hospital for material, have been unable with the media at their disposal to grow the organism. I am not surprised, therefore, to find that in Dr. Burns's cases only about 4 per cent. were found to grow the gonococcus.

With regard to treatment, whilst not condemning drugs, caustics, curettage, in the whole-hearted manner of Dr. Burns, I am entirely with him in considering indiscriminate douching actually harmful, and vaccines practically valueless, in this condition. I am particularly pleased, moreover, to find at length some recognition of the pioneer work of the late Dr. Sloan, of Glasgow, the value of whose methods is now only beginning to be appreciated. Ionisation properly carried out is exceedingly valuable in chronic endocervicitis, and anyone stimulated by Dr. Burns's article to use it should read Dr. Sloan's book¹ on the subject.

Dr. Burns's description of the technique is incomplete, probably from reasons of space, and if followed, without further guide, liable to get the practitioner into trouble; for after running the current for some 10 or 15 minutes, as described, it will be found that, on attempting to withdraw the zinc rod from the cervical canal, it is firmly held by the mucous membrane, and often cannot be dislodged without causing bleeding. If, however, the current is reversed for five minutes before withdrawing, the rod comes out quite easily.

On the subject of erosion I am afraid I do not see eye to eye with Dr. Burns; but I feel that I have trespassed too much on your space to dilate upon this question. It only remains for me, therefore, to congratulate both the author and THE LANCET on ventilating this much-neglected subject.

I am, Sir, yours faithfully,

J. JOHNSTON ABRAHAM.

Harley-street, W., Oct. 23rd, 1922.

THE BACILLUS ABORTUS AND HUMAN ABORTION.

To the Editor of THE LANCET.

SIR,—On Oct. 16th, at the Milk Conference at the Guildhall, Sir Stewart Stockman, of the Ministry of Agriculture, made the following statement:—

"I have no doubt that thousands of pregnant women have drunk milk containing the *Bacillus abortus*, but in no case has it been alleged that a woman aborted from that cause, at least not so far as I am aware."

His address was on infective abortion in cows. He ascribed the disease to a bacillus which he described, and he stated that if that bacillus were smeared on the pharynx of a pregnant cow it led to abortion. The acoustics of the hall left a good deal to be desired, and I may be mistaken, but if I heard correctly I also understood him to say that the disease could be communicated from cow to cow by ingestion of infected material.

In view of the fact that we cannot in all cases trace the cause of abortion in human beings, it may be worth while to pursue the line of investigation indicated, to study the *Bacillus abortus* from the human point of view, and attempt to determine whether or not there is an ætiological relationship between this micro-organism and abortion in the human female.

I am, Sir, yours faithfully,

S. G. MOORE,

Medical Officer of Health, Huddersfield.

Oct. 18th, 1922.

¹ THE LANCET, Oct. 21st, p. 885.
Spiritualism and the New Psychology, p. 85.

² Electrotherapy in Gynæcology, Heinemann.