

cooking or long exposure to sunlight. In India the cook is trained to overcook and stew everything, from porridge, soup, and vegetables to the meat. Peas and other things are put in cold water and left for hours to simmer—meat the same; the milk is boiled and reboiled many times. All fresh fruit and vegetables are excluded for fear of cholera.

Drink.—Pure water is the best, and plenty of it; four or five big glasses a day is not too much. It should be boiled and cooled, if not obtainable from a reliable spring. Alcohol is most inadvisable.

Constipation is due to improper food, nothing raw in the diet, and to insufficient water to drink. It may be traced also to the use of animal fat for cooking instead of oil, and perhaps chiefly to the wrong posture used during defaecation. The Government Public Works Department in India has standardized a high commode, which would induce constipation in the healthiest. The closet seat should be as low as possible. Observe the native and be willing to learn.

Clothes.—Clothing must admit of a free supply of light and air on the skin. Shorts and a shirt are the proper clothing for hot weather in the tropics. The clergy are the worst offenders, and to see Indian candidates for holy orders in black coats and white collars, perspiration streaming down their faces, is calculated to make both men and angels weep. Prickly heat is due to overclothing, and is never seen on naked skins.

Light.—Light does no one any harm; on the contrary, no one is healthy without light on his skin. As with the air, people can shut themselves up until they become sensitive to every draught and every ray of light, or they can harden themselves to bear sunlight and be the better for it. The harmful rays are the heat rays, and it is better for people to avoid the sun from midday until 3 o'clock; and to lie under a thick tree is better than to be shut up in semi-darkness and stagnant air, as is too often the European custom.

Exercise is essential in the morning and evening. As recent research has shown, the ultra-violet rays are then at the maximum, whereas at midday the heat rays predominate and lead to exhaustion. Contrast two classes in the East. The subaltern, who leads an active life and attends parades, route marches, polo, tennis, dances, and can wear shirt and shorts, keeps hard and fit. The clergy and office workers wear special clothes, designed to keep off all light and air from their bodies, with hard tight collars; exercise is at a minimum, indoor life at a maximum. The sedentary life in the East as led by Europeans sooner or later ends in disaster, because Nature never intended us to live in that way. Note the Indian holy man who gives himself to meditation. Not for nothing is he naked and sits in the sun. The more thinking done the more light required.

I would sum up by saying that to keep well in the tropics, light and air on the skin, exercise, plenty of water to drink, fresh food, fruit, and nuts, and avoidance of constipation are essential.—I am, etc.,

April 15th.

K. VAUGHAN, M.B.Lond.

DR. YOUNG'S CANCER PARASITE.

SIR,—In my letter (BRITISH MEDICAL JOURNAL, April 10th, p. 675) I deprecated much of the looseness and inaccuracy of statement which in discussions on cancer often masquerade as sincere scientific criticism. Dr. Leitch's reply (April 17th, p. 721) is a very complete vindication of this plea. Two of his main statements are demonstrably inaccurate. He affirms that at the end of the test experiment carried out at his laboratory he "learned, for the first time, that it was 'leukaemia' rather than cancer" which I undertook to produce. He states further that "we insisted on controls, which Dr. Young regarded as quite unnecessary." It is obvious that were these statements true Dr. Leitch sweeps the ground from under my feet. So far, however, are they from being true that on May 10th, 1924—that is, before the experiment was commenced—I sent a letter to Dr. Leitch outlining the proposed scope and nature of the experiment, in which I undertook to produce "a progressive lymphomatous lesion of the nature of pseudo-leukaemia and, in an advanced case, of lymphosarcoma." Further, I asked Dr. Leitch to arrange for an adequate number of mice, "say 50 with 50 controls," which he did! These quotations from my letter prove that on both counts Dr. Leitch's statements are misleading, and his inaccuracy in regard to a scientific experiment is as remarkable as it is inexcusable. I have no doubt that when Dr. Leitch consults his correspondence he will see the need for apology.

Dr. Leitch states, moreover, that I was not interested in nor did I wish to see the control animals. The real facts are that Dr. Leitch informed me that only two control animals were alive, the remaining forty-eight, like the dead injected mice, having been thrown away without

examination of the organs concerned. He attempts to minimize the value of my experimental results by the statement that so-called leukaemic patches in the liver "are exceedingly common in laboratory mice. They consist of multiple small foci of lymphocytes and bear no relation to tumours—or even to leukaemia." The futility of this is seen by the description of my experimental lesion which I published in 1922.

"Microscopically the histological picture is that of lymphatic leukaemia, the lymphoid growth being especially abundant in the portal areas and extending in the form of plugs of tumour cells into the surrounding parenchyma. Even in an early case the hepatic capillaries may be filled with lymphocytes. In an advanced case the liver tissue may be ploughed up and destroyed by the invading cells. . . . A close study of the appearances has practically convinced me that the neoplastic cells may, and usually do, originate *in situ* in the liver from the lymphoid nodules which, even in the adult, are constantly present in the normal mouse liver. . . . In many cases the proliferating cells may be found in vessels, the lumina of which may be completely plugged by the tumour cells. Mitotic figures may be present in these intravascular elements."

In the test experiment referred to this obvious leukaemic picture reappeared exactly as in my own previous experiments. In Maud Slye's laboratory, where the condition has been closely studied, it is considered as a malignant tumour, and it has been found to occur spontaneously in only 1.5 per cent. of the animals. It is abundantly clear that Dr. Leitch's "foci of lymphocytes" correspond to the appearance which I italicize in the above quotation! The "certain amount of glandular hyperplasia in the region of the thymus gland" found in two out of the eleven animals ultimately rescued from a mishandled experiment (in one it formed a tumour half filling the thorax!) obviously corresponds to the lesion described by me in 1922, where I state that "in an infected animal this scattered lymphoid tissue may exhibit evidence of hyperplasia in the shape of numerous mitotic figures, and in an invasion of the surrounding structures, large vessels, heart wall, etc." This tumour, which has a well recognized association with malignant types of leukaemia, reappears in Dr. Leitch's test experiment. Had Dr. Leitch taken the trouble to read the paper I sent him before the experiment began, in which I show that these lesions have been produced by the "cancer parasite" he affects to despise, or had he studied the observations of others who have described these very significant lesions, he would have been better able to judge how far the arguments for an essential linking of leukaemia, pseudo-leukaemia, lymphosarcoma, and cancer are justified. He would have been able, as was expected, to adjudicate between my claims and the "several" observers and the "numerous distinguished pathologists" to whom he refers, and behind whose opinions he now seems to find comfort in sheltering himself.

It is unnecessary further to multiply the evidence of prejudice and inaccuracy with which Dr. Leitch crowds his letter. The purpose of this correspondence is amply served with the few instances I have selected. It is interesting to note that, whilst Dr. Leitch has been hoping "to have heard the last of it," increasing evidence has been arriving from other laboratories in support of my work. My reason, however, for embarking on this correspondence was not to argue the validity of my views; it was rather to urge a general plea for fairness and accuracy in scientific criticism.—I am, etc.,

Edinburgh, April 17th.

JAMES YOUNG.

A CORRECTION.

SIR,—On page 611 of the issue of the BRITISH MEDICAL JOURNAL for April 3rd Dr. Alcock quotes my remarks at the Annual Meeting at Bath, and says that I gave as indications for operative treatment in cases of retroversion "sterility, dyspareunia, and prolapse." What I really said was that I seldom operated for uncomplicated retroversion, and considered that the indications for operation were dyspareunia, usually in acquired retroversion, and sterility or repeated early miscarriages without other obvious cause in cases of so-called congenital retroversion.—I am, etc.,

London, W.1, April 20th.

HENRY RUSSELL ANDREWS.