**Malaria Strains Resistant to
4-amino-quinoline Drugs**

SIR,—In a letter referring to malaria strains showing resistance to the 4-amino-quinoline group of drugs (24 July, p. 229) it was stated that in some instances cross-resistance has been found to extend to all other available synthetic antimalarial compounds. In such circumstances it would be necessary to fall back on quinine for treatment of a malarial attack.

I would point out that this would not necessarily constitute a retrograde step. It is not generally appreciated that the development of most of the synthetic antimalarial drugs was inspired by the fact that Germany was cut off from all sources of cinchona during the first world war, and not by defective action of quinine.

In point of fact quinine, though not suitable for prophylaxis, is a most effective weapon for the treatment of a malarial attack, and in *Plasmodium falciparum* infections it is capable of effecting radical cure. This applies particularly to strains occurring in tropical Africa, which is the source of the majority of malarial infections encountered in Great Britain.—I am, etc.,

G. COVELL.

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Varicose Veins in Pregnancy

SIR,—One statement in Mr. Harold Dodd's letter (31 July, p. 304) disturbed me. He states that "stripping, which is fashionable," should be "relegated to the limbo of irrational operations."

I was less disturbed, however, when reading the rest of the letter, and the description of the study of Zulus with "tooth-paste stools" which cause Mr. Dodd to state in his letter that the eating of one to four tablespoonfuls of bran daily would lessen the incidence of varicose veins in pregnancy.

This made me realize that the statement regarding stripping need not necessarily be taken seriously, and I do hope for the benefit of the enormous number of sufferers that it is indeed disregarded.—I am, etc.,

London W.1.

R. ROWDEN FOOTE.

A New Protozoon

SIR,—In your issue of 19 June (p. 1602) Professor P. C. C. Garnham publishes a highly critical review of my book, *A New Protozoon: Its Relation to Malignant and Other Diseases*. I feel that some of his remarks are less than fair, and I should welcome the opportunity of replying to several points he raises. Firstly, I did not, as he suggests, make any claim to a curative effect of 4-aminoquinolines on reticuloses, cancer, or premalignant diseases, but merely recorded the effects of administering these drugs in such conditions and the fact that they may cause complete regression in premalignant conditions—and also in reticuloses lasting now up to seven years or more.

Secondly, Professor Garnham states that the frequency of remissions in the various diseases makes a cause-and-effect relationship

unproved. Some of the patients were *in extremis* suffering from reticuloses with complete radiological "blackout" of one-half of the chest, or carcinomatosis and unconsciousness due to cerebral metastatic tumours with pleural effusion. Their response to treatment was almost immediate and rapid, and in these circumstances could surely not be attributed to natural remissions in the illness. Furthermore, in the book I gave references to independent confirmatory results by Japanese workers of the results of such treatment. Other reviewers have not cast doubt on this aspect of the work.

Thirdly, Professor Garnham tends to dismiss the organism as "amorphous fragments." Against this are the identical appearances, pigmentation, and size of the organism in all conditions from which it was recovered. Some of the microphotographs of the delicate hollow spheres with fenestrated circumferences (Figs. 17–19) closely resemble other protozoa, such as *Noctiluca miliaris*, and the veined forms (Figs. 15–16) stages in the life history of Myxomycetes (see Wenyon's *Protozoology*, 1926). These could hardly be artificial fragments. As stated in the book, slides of the organism were shown to a number of Professor Garnham's fellow protozoologists, who agreed on its organismal nature. He might be interested to know that the finding of the organism has already been confirmed in other countries of the world.—I am, etc.,

London W.1.

R. WYBURN-MASON.

Diagnosis of Hysteria

SIR,—Dr. Robert Thompson's letter (24 July, p. 234) contains statements and premises which I for one cannot accept without clarification. He wrote, "the confusion in the diagnosis and treatment of hysteria lies . . . in a failure to differentiate two types of anxiety—the anxiety which is based on depression and the anxiety which is based purely on suggestion."

For years many psychiatrists have diagnosed anxiety with depression and depression with anxiety as separate conditions, placing the primary syndrome first. The hypothesis on which they base these diagnoses is clearly that depression and anxiety are separate entities and that these frequently co-exist. Hence the failure to bring about a sustained improvement if E.C.T. or antidepressants are mistakenly given for a primary anxiety state with secondary depression. There would appear to be no valid reason for using the term anxiety neurosis (or state) for depression with anxiety as suggested by Dr. Thompson, even if he does not believe that an anxiety state can exist as a separate entity uncomplicated by either hysteria or depression. He did not say this, but his suggestion infers it.

I believe a great deficiency in definition exists. Anxiety is frequently given as a symptom of depression, particularly in advertising circulars for antidepressant drugs. After 25 years' psychiatric experience I am now certain that whenever anxiety symptoms are present they are part of a separate syndrome and never a symptom of depression. Perhaps attacks of agitation are mistaken for anxiety symptoms by some therapists who believe they are hearing described or witnessing an acute anxiety attack.

In my view attacks of agitation and acute anxiety attacks are distinguishable. Agitation is defined as a behaviour pattern, always occurring in response to ideas of disaster, and it is psychotic. On the other hand, anxiety consists of a chain of autonomic physiological changes set in motion by fear (the connexion between the fear and the symptoms usually being unrealized by the patient), and it is neurotic. Secondary phobias arising from autonomic changes in anxiety (e.g., heart disease from tachycardia) are not ideas of disaster, phobias having a basis in fact not delusion.

But if Dr. Thompson believes I am splitting hairs and that anxiety occurs in depression as a symptom, he is in good company—Mayer-Gross, Slater, and Roth (*Text-book of Clinical Psychiatry* (1955 edition, p. 199, para. 2; page 205, para. 1), although p. 200, para. 4, does perhaps tend to bear out my theory regarding the delusional basis for agitation.

Dr. Thompson has done a service in reminding us of Pavlov's definition of hysteria. If psychiatry is to become an exact science clear definition remains the essential. The hardest illness of all to define is hysteria. Dr. Thompson suggests that the fainting by a medical student faced with the operating theatre is a good example. I would say that it was much more likely to be an autonomic response due to anxiety and that the mild hypochondriasis of his other examples are due either to depression or anxiety, certainly not hysteria. If fainting were hysterical the subject would go on fainting at intervals after being removed from the scene of his conflict. Surely hysterical symptoms always persist beyond the moments of impact with the precipitating factors?

It is of paramount importance that the depressions, anxiety, hysteria, and obsessional states be clearly defined, and that it is accepted that admixtures of any or all can occur in the same patient at the same time, and when they occur together are separate syndromes requiring additional treatment. It is because of the prevailing inexactitude of definition in psychiatry that one is bedevilled by reams of descriptive case notes written by juniors which do not conclude with a diagnosis, a situation which Mayer-Gross himself deplored and spoke about at least once at an R.M.P.A. meeting.—I am, etc.,

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W. V. A. ERSKINE.

Infection in Schools

SIR,—I am interested in the conflicting views expressed on the value of school exclusion in eliminating the spread of Sonne dysentery contained in the article by Dr. A. B. Christie (19 June, p. 1597) and the letter from Dr. G. M. O'Donnell (24 July, p. 235), both in this journal. Sonne dysentery is admittedly usually a mild disease but is always a nuisance and sometimes serious, while relapses and/or second attacks are of frequent occurrence. (Last week a child in one of my districts was admitted to hospital with convulsions and pyrexia of 106° F. (41.1° C.).)

My own view is that school exclusion has an important part to play as a method of reducing the weight of infection, particularly