and in some cases treatment will have to be instituted before these people are dispersed throughout the country. This is a formidable task, but one which should be undertaken if the N.H.S. is not to be overloaded with work as the result of imported and transmitted ill-health.—I am, etc.,

Bournemouth,
Hants

C. R. SALKELD

Aetiology of Varicosity

SIR,—Any proposition from Mr. Denis Burkitt (3 June, p. 556) must be treated with respect, but the thesis that haemorrhoids and varicose veins have a similar causation from bowel dysfunction seems too facile. I submit a list of questions which are provoked by it.

(1) Has it been established that haemorrhoids and varicose veins occur together more often than the chance coincidence of these two common conditions would provide for? There has been no notable concomitance in my own experience.

(2) Can this theory explain why varicose veins are so much commoner in women? Does it explain the well-known aggravation of varicose veins during pregnancy (indeed, very early in pregnancy) or the temporary aggravation which many women experience (at least of symptoms) with menstruation?

(3) Can this account for the often quite irregular (and sometimes bizarre) distribution of varicose veins? It is not at all uncommon for them to be quite marked on one lower limb and not (or scarcely) in evidence at all on the other: while odd patterns of tortuosity and dilatation of the superficial veins of the lower limbs are common-place.

My own belief is that the hereditary influence is the most important of perhaps several factors responsible for varicose veins—in keeping with the well-known familial incidence. They are second cousins to angiomas, of which frank examples of segmental distribution are the extreme and indisputably inborn venous and arteriovenous abnormalities. Others, I submit, represent chiefly an inherited vascular "weakness" or anomaly of development of a lesser degree. This may well be an over-simplification itself, and a begging of the question. But to accept varicose veins as due to constipation seems almost too credulous.—I am, etc.,

ROBERT S. LAWSON

Melbourne,
Victoria,
Australia

Effects of Dietary Fibre

SIR,—With regard to the letter by Sir Charles Dodds and others (19 August, p. 472) I showed in my own letter (13 May, p. 408) how little importance I attached to the change in flour milling in 1880, compared with the vastly increasing sugar consumption at that period; and with respect to the incidence of varicose veins in Africa, Mr. D. P. Burkitt's paper (3 June, p. 556) deals comprehensively with this very subject. But as regards Sir Charles's pejorative remarks about the taking of natural, unprocessed bran I have in the above letter written of my 30-year experience with this material. He speaks of the occasional case

who complains after this natural restitution, but he does not speak of the scores who bless the name of whoever introduces them to it.—I am, etc.,

Fareham, Hants

T. L. CLEAVE

Chloramphenicol Resistance in Typhoid

SIR,—Your leading article (5 August, p. 306) on chloramphenicol resistance in typhoid prompts me to report my experiences in treating 36 cases of typhoid in Malawi between 1968 and 1970.

The prerequisites for chloramphenicol resistance mentioned by Dr. E. S. Anderson and Mr. H. R. Smith (5 August, p. 329) apply in that typhoid was endemic in the district and chloramphenicol was extremely widely used. Thirty-three cases were in local Africans and three were in British expatriates. All of the expatriates had a good response to chloramphenicol. It is interesting to note that only one of the three had received a recent T.A.B. vaccination.

Of the 33 cases in Africans three showed no clinical response to chloramphenicol, though two of them showed in vitro sensitivity to it on blood culture. The third case showed only partial in vitro sensitivity to chloramphenicol. As ampicillin was not obtainable readily all three resistant cases were treated with tetracycline, to which the organism showed laboratory sensitivity, and they showed an excellent clinical response. Of the remaining cases seven were treated with tetracycline before a definite diagnosis of typhoid had been made and two had such a good response that chloramphenicol did not have to be substituted. The other five responded to chloramphenicol after failure of tetracycline therapy. A single case, originally diagnosed as a urinary infection, responded to sulphadimidine and did not require chloramphenicol.

Ampicillin was used only once—on a child who had been treated originally with chloramphenicol and who continued to pass *Salmonella typhi* in the urine after cessation of treatment. Urine cultures after ampicillin therapy were negative.

In summary, out of 36 cases of typhoid three responded to tetracycline after failure of chloramphenicol therapy and two responded to tetracycline as the drug of first choice. Five cases failed to respond to tetracycline and required chloramphenicol.—I am, etc.,

Royal Infirmary,
Glasgow

JOHN F. CALDER

Transmission of Malaria

SIR,—“Madras Club. December 3rd 1899. I am about to take up mosquito collecting. Since Major Donald Ross's investigations into the ‘malarial mosquito’ the whole Indian medical faculty is agog with this subject. But medical men, although many are keen enough to be specialists, are very uneven, and also they are very stationary. So I have button-holed the keen ones, told them to place materials at my disposal and promised to collect for them. As I visit all climates, including feverish and healthy ones, I am in far the best position to collect. Perhaps too my line of training and work render me

more fitted for the necessary discrimination and delicate work.”

This is an extract from the journals which my father kept from the time he went out to India as a government botanist in 1898 until he retired from his post as agricultural adviser to the Government in 1919. He used to spend more than half of each year on tour in remote villages studying the crops and diseases and methods of agriculture, and this involved the collection of numerous botanical specimens. He was also an expert entomologist, and collected numerous specimens of mosquito, which he dispatched to the headquarters at Secunderabad; some of them he had captured in the act of biting himself. Like any other travelling English official in India, he suffered frequently from malaria: but he brushed it off as “an attack of fever, I increased my usual dose of quinine.”—I am, etc.,

GEOFFREY BARBER

Dunmow, Essex

E.N.T. Advances

SIR,—Dr. R. Hinchcliffe (12 August, p. 419) and Dr. L. Fisch (19 August, p. 471) have both taken issue with my criticism in a book review (22 July, p. 243) of their failure to refer to electrocochleography. In fact, I was not criticizing but rather excusing them for it.

To take the latter first, there are many points in Dr. Fisch's letter with which I am in total agreement and I am pleased that your columns have given him an opportunity to make them. In particular, I would be the last to “hope that a technical procedure will take the place of proper provision for clinical audiology.” But he concedes that, in his own words, “electrocochleography was in a very experimental stage when I wrote the chapter on the deaf child,” and this was precisely the point (indeed, the whole and sole point) of my criticism—namely, that any book of this size takes time to produce and that this raises the question as to whether or not the needs of the specialist might not be better served by a series of monographs which could be more rapidly produced. I hope he does not really mean it when he says “were I to write it today I still might have omitted cochleography.”

Dr. Hinchcliffe, in effect, defends the omission of electrocochleography on the ground that it is one of the “not yet established clinical procedures.” Of course it is not, but an established procedure is not the same thing as a trend, and whatever else it may prove ultimately to be there is no doubt that today electrocochleography is one of the more interesting modern trends in otolaryngology. And that, after all, is the title of the book.—I am, etc.,

JOHN BALLANTYNE

Royal Free Hospital,
London W.C.1

Hands off Postgraduate Centres

SIR,—It seems now to have become established history that the Christ Church conference “started what was soon to become an explosion of activity,” as your leading article states (3 June, p. 547). Hardly ever is any reference made to the fact that at that