of time. I have no idea of the patient's condition beforehand, and the clinic's time has to be allocated no matter whether I can do something for the patient in the end or not. I get people to certify who have glasses on order with which their vision is fair, and people who are about to enter hospital for operation in the very near future. Could they not wait till treatment is finished, so that I have not to decertify them afterwards again, while children's eyes go blind through neglect?

Finally a point of a different kind. Why was "place of examination" omitted in the new B.D.8s? Is it not most important for the validity of the certificate whether the examination was made in a decent clinic or an improvised clinic without proper light and space, or in the patient's home, as was the rule in the good old days?—I am, etc.,

SCHOOL OCULIST.

Popliteal Ligation for Post-phlebitic Leg

SIR,—In my paper (December 9, 1950, p. 1307) I endeavoured to investigate popliteal vein ligation in as dispassionate a way as possible. Mr. S. M. Rivlin in his letter (December 30, p. 1495) lists me as its protagonist; he is certainly an antagonist, presumably because his results have been unfortunate.

I like to think that if he selects his cases along the lines I suggested, even if he does not stoop to measure the erect venous pressure during exercise, he will get better results. In a condition like post-phlebitic leg it would be asking too much to expect any one measure to cure, as we cannot remake valves. If however a measure in properly selected cases can cause relief in a considerable proportion, he would be a poor physician who would withhold such a remedy because eventually its good effects might cease. I agree of course with Mr. Rivlin that there is no guarantee that these cases will not subsequently relapse, but surely even a passing relief is worth while.

Mr. Rivlin has missed my point in the aetiology of the condition. Essentially the normal venous pumping mechanism has failed, but there is no evidence that the flow in the incompetent vein is retrograde in exercise. The normal person when standing is always moving slightly from foot to foot, and this exercise is sufficient to keep the venous pressure low. Depending upon the extent of valvular failure, the pressure rises, and it is this raised pressure in the erect position which causes the trouble. It is well known that if all these patients are kept in bed irrespective of what is done to the veins good results will ensue, and it is readily demonstrable that when lying in bed the pressure is very low. By tying the popliteal vein the strain is, of course, put on the collateral veins, but it must not be tacitly assumed that they must become incompetent.

As to Mr. Rivlin's query regarding the eleven satisfied clients, they included the earliest and latest of the series and the remainder are scattered in between.—I am, etc.,

Leeds. A. J. WALKER.

Treatment of Tinea with Oral Sulphone

SIR,—An Indian labourer, about 40 years of age, had suffered from tinea corporis for 15 years, resisting a great variety of treatments. These included iodine, mercury, chrysarobin, salicylic acid, benzoic acid, and gentian violet. The whole of the trunk, upper parts of the arms, buttocks, and legs showed contiguous circular patches which healed at the centre and spread at the periphery. There was no anaesthesia, nerve thickening, or lepromatous tissue to suggest Hansen's disease (leprosy). Because of the marked resistance to other treatment and the fairly high concentration of sulphones obtained in the skin, diaminodiphenylsulphone (D.A.D.P.S.) was given in dose of 0.1 g. daily for the first week and 0.2 g. daily thereafter. By the end of the second week no trace of tinea existed. The success of sulphone in this case may be of interest to others, as this type of resistant fungus infection is not uncommon in the Tropics.—I am, etc.,

Doom Dooma, Upper Assam. P. H. BIRKS

Oedema of the Glottis

SIR,—The rare clinical phenomenon known as acute oedema of the glottis (see December 30, 1950, p. 1476) or, more correctly stated, of the introitus laryngis (arytenoids, ary-epiglottic folds, and ventricular bands) may result not only from a sting or other physical trauma but from any localized or proximal infective condition, and, as pointed out by Dundas Grant, the presence of acute dysphagia and laryngeal stridor in the absence of obvious inflammation in the oro-pharynx must incriminate the hypo-pharynx and (or) the introitus laryngis, the early recognition of which is of such supreme prognostic importance.

As regards expectant treatment, if such be ever justifiable, the frequent local application of a weak solution of cocaine and liquor adrenalin. in equal parts by spray or otherwise combined with the intravenous injection of calcium gluconate supplemented by parenteral antibiotic treatment would be most likely to effect the requisite rapid improvement. In view, however, of the vital urgency of these cases, the rapid relief which it effects, and the ease with which it can be performed, scarification or multiple puncture of the oedematous supra-laryngeal structures is the treatment of choice. It can be carried out after the topical application of cocaine adrenaline solution by means of a laryngeal knife or, alternatively, a long-handled curved bistoury and a tongue-depressor. A frontal mirror is desirable but not a *sine qua non*. From personal experience I can vouch for the relative effectiveness of this procedure in comparison with the technically much more difficult and risky tracheotomy.—I am, etc.,

Cork. J. B. HORGAN.

Certification

SIR,—Now that we have an epidemic upon us it seems to me that the absurdity of the method of certification is demonstrated. It cheats both patient and doctor alike, the first of his money and the second of his time and proper function. "I wouldn't have troubled you, Doctor, to call, but I must have certificates before such and such a day." The malady of certification has now spread to industry: "I'll lose my job or holiday money, etc." My visiting list, already too long, could have been halved this week but for the need of certificates—a sheer waste of time and talent.

Might I suggest that the sick-visitor could be employed to visit the sick and witness the facts that the patient is in bed and to the lay observation sick, and report them to the necessary authority? The doctor could then if needed call when more pressing professional needs have been attended to.—I am, etc.,

Wrexham. E. A. R. EVANS.

Shorter House Jobs

SIR,—As a student may I submit that a possible remedy for some of the deficiencies in our training as future G.P.s would be the shortening of house appointments in some specialist hospitals? If, for instance, it were possible to work for only two to three months in, say, a fever hospital, it would give a greater number of budding housemen a fair chance to see some of the cases they have up till now only read of, and prevent some of the crass errors of diagnosis so frequently made by beginners.—I am, etc.,

London, S.W.10. H. KELSON FORD.

Oral Penicillin

SIR,—Dr. S. A. Doxiadis and his colleagues (January 6, p. 16) state, from observations carried out on *healthy* subjects, that if penicillin is given by mouth very large doses must be administered in order to obtain an adequate level in the blood, because the hydrochloric acid of the stomach destroys penicillin. But in virtually all the conditions for which penicillin is used there is no hydrochloric acid in the stomach, its secretion being temporarily inhibited by the toxic condition of the body (Faber, K., *Gastritis and Its Consequences*. Oxford, 1935). The oral administration of penicillin, both for children and adults, has many advantages

not only to the doctor but also to the patient. Therefore before assuming this form of therapy is unsatisfactory, or too costly, or to require in addition large amounts of antacids, it would seem important that blood penicillin levels after oral doses should be investigated during illness and not during the irrelevant condition of health.—I am, etc.,

London, W.1. FRANKLIN BICKNELL.

Poliomyelitis in London in 1949

SIR,—We are glad to assure Dr. E. H. R. Smithard (January 13, p. 92) that for all statistical purposes in our paper (December 30, 1950, p. 1473) the number of confirmed notifications of poliomyelitis in Lewisham in 1949 was taken as 35, the figure he quotes as correct. A table showing the basic distribution by boroughs was prepared for publication, but could not be included for lack of space. We used for all calculations the figures given, as confirmed, to the Registrar-General by the borough medical officers of health in their quarterly returns; we hope there is no danger in this indirect aggregation, otherwise the Registrar-General's publications are presumably also implicated. The map was plotted day by day (for purposes other than the paper), and where the daily returns from the borough medical officers of health showed earlier notifications to be unconfirmed the cases were erased. In this sense the map was bona fide corrected. Only eight such corrections were received from Lewisham. We doubt whether the pictorial impression is substantially in error, but we apologize for the fact that no note was added to the diagram pointing out limitations in the meaning of "corrected."

With regard to the confirmed cases for 1950, quoted by Dr. Smithard as 32, we would add that for the first three quarters alone, according to copies of forms received by the L.C.C., he had returned 32 cases to the Registrar-General—i.e., equal to his present total for the whole year.—We are, etc.,

G. E. BREEN.
London, S.E.1. B. BENJAMIN.

Management of Thyrotoxicosis

SIR,—Dr. Raymond Greene (December 16, 1950, p. 1378) attributes to me an opinion far stronger than anything I should care to hold. He writes that I asserted at a meeting of the Section of Endocrinology of the Royal Society of Medicine that all patients who had had remissions of thyrotoxicosis following medical treatment would eventually relapse.

What I said in this respect was apropos the remark of a previous speaker who mentioned the "cures" obtained by physicians in the days before thiouracil came into vogue. I stated that, in my experience, there was a proportion of patients in whom the disease was episodic, and that I had operated upon a number who had undoubtedly had the disease in a severe degree many years previously, and who had been apparently temporarily cured by rest and sedatives. I added that, to my knowledge, no physician had published a long-term follow-up of such "cures." I should be surprised, however, if some patients did not remain permanently well in a disease so varied in its course and so differing in intensity.—I am, etc.,

London, W.1. CHARLES DONALD.

Acetonuria Rare

SIR,—In their article (January 6, p. 14) "Raised Blood Pyruvic Acid Level in Diabetic Acidosis" Drs. I. C. Gilliland and M. M. Martin make the curious statement that the urine was loaded with sugar and acetone ($FeCl_3$ test). I think the authors mean that a large amount of aceto-acetic acid was present in the urine and that the $FeCl_3$ test gave a deep port-wine colour. But their statement is meaningless for the following reasons:

(1) Acetone is not present in freshly passed urine, but only appears if the urine is allowed to stand for some time, when some of the aceto-acetic acid decomposes to acetone.

(2) It is impossible to produce air hunger with acetone in a cat, whereas aceto-acetic acid is the substance which causes air hunger.

(3) Acetone does not give any colour with $FeCl_3$ solution, whereas aceto-acetic acid gives the port-wine colour.

Rothera's modification of Legal's test (*J. Physiol.*, 1908, **37**, 491) is a test for acetone and aceto-acetic acid, although Rothera thought it was a test for acetone only. It is, however, a much better test for aceto-acetic acid, and Hurtley (*Lancet*, 1913, **1**, 1160) showed that acetone could be detected in a concentration of 1:20,000, whereas aceto-acetic acid could be detected in a concentration of 1:400,000.

Many people, who do not know that acetone, though expired by the lungs, is not excreted in the urine, speak of acetonuria instead of ketonuria. But it is a pity that the authors should make such an inaccurate statement in an otherwise interesting and excellent article.—I am, etc.,

London, W.1. GEORGE GRAHAM.

Refresher Course

SIR,—Reading with the greatest interest your two refresher course lectures on disk lesions (December 23, 1950, p. 1434) and acute bronchitis (January 13, p. 82), I find one must review the meaning of the word "refresher." Here we see our well-tried remedies for backache tossed in the bucket, and tracheotomy threatened for croup in lieu of ipecacuanha wine. There is no refreshment of our knowledge of medicine in all this. Must we divorce urban therapeutics from rural therapeutics? The healing urge of nature, so delightfully obvious in country practice, appears to be entirely absent in the out-patient departments and wards of the great hospitals. In such hospital experience, where the specialist meets his human material at the syringe point, little co-operation with nature can be expected. It is presumed the natural healing powers will not respond to trauma, bacterial or otherwise. Chemotherapy usurps the field.

We country workers, who still appreciate the help of providence, cannot expect specialists to understand how we treat and cure the myriads of patients they are never privileged to see. If they understood, we might escape some of their sweeping condemnations.—I am, etc.,

Hexham, Northumberland. RICHARD BELL.

What is Homoeopathy?

SIR,—What is homoeopathy? Is it scientific or mumbo-jumbo? We read little about this subject in the *Journal*, and I for one am confused about its relation to orthodox medicine. If it has something real to offer, let us hear about it, but if not should it be relegated to the status of naturopathy?—I am, etc.,

London, W.13. R. E. W. OLIVER.

POINTS FROM LETTERS

Long Umbilical Cord

Mr. A. E. CHISHOLM (Dundee) writes: Several years ago I had a case in which the umbilical cord measured 5 ft. with two true knots, both quite loose. The cord had not twisted round the baby and there was no complication.

Red-cell Stippling in Poisoning

Dr. A. PINEY (London, W.1) writes: Dr. W. Gross (December 30, 1950, p. 1496) says that in patients treated with bismuth or mercury "supravital staining with brilliant cresyl blue revealed basophil stippling of the red blood cells." It is essential to realize that punctate basophilia (stippling) of red corpuscles can be seen without any staining, whereas the basophilic substance seen after vital staining is something quite different, being indicative of immaturity of the corpuscle, not of degeneration. Dr. Gross need thus not be surprised that he was unable to find any reference to basophil stippling of red blood corpuscles in bismuth and mercury poisoning. There are, however, many references to the presence of reticulocytes—i.e., corpuscles which with vital staining can be seen to contain a basophilic substance, sometimes in the form of dots, but more usually in a skein.