his students to psychometric tests, personality tests, such as the Rorschach, and questionnaires designed to show abnormal traits, or did he just rely on his psychiatric intuition?—I am, etc.,

Sheffield.

F. J. KELLEHER.

The Problem of Chilblains

SIR,—I am grateful to Dr. William P. McKeever (March 6, p. 475) for drawing attention to a terminological error in my paper (Feb. 21, p. 336). As a result of experiments on the effect of cold on cellular metabolism it was found that cyanosis did not develop in a hand that was immersed in cold water for as long as six minutes when the venous return was stopped by a tourniquet, but rapidly developed on transferring the hand to hot water. The failure to develop cyanosis in the first place was no doubt due to impaired cellular activity, and the development of cyanosis in the second place was due, most probably, to the accumulation of the waste products of cellular metabolism and the consequent increase of reduced haemoglobin.

When the hand was returned to the cold water, with the tourniquet still in position, the cyanosis slowly disappeared and at the end of six minutes had been replaced by a bright red colour. I suggested that the reason for this must have been a dissociation of the carbon-dioxide-haemoglobin relationship (I used the term carboxyhaemoglobin in error).

I am aware that it is stated that cyanosis only appears when 40% of a normal haemoglobin content exists as reduced haemoglobin. (*Price's Textbook of the Practice of Medicine*, 1942 p. 339), but on this basis I found it difficult to explain the disappearance of the cyanosis on exposure to cold. It is hard to imagine why cold should decrease the percentage of reduced haemoglobin present, assuming the venous circulation to be stagnant in the area under consideration. That cyanosis may develop from other causes is evident from the fact that sulphonamide medication may cause it; in this case the cyanosis may be due to either methaemoglobin or sulphaemoglobin (*ibid.*, p. 16).

In my paper I tried to avoid being dogmatic regarding the nature of the products of cellular metabolism which cause the inflammatory reaction found in chilblains. I prefer to leave the elucidation of this point to colleagues who have a greater knowledge of biochemistry than I possess.—I am, etc.,

Ilkley, Yorks.

R. JOHN GOURLAY.

The Design of Bed-pans

SIR,—Although the practice of getting patients out of bed soon after operation will to some extent solve the problem of that nerve-shattering act the use of the bed-pan, nevertheless there will still be many patients who are condemned to use it. The writer did at one time design a bedstead in which the bed-pan was placed below the bed and moved into position by a lever rather like the breech-block of a gun. This, however, required a special mattress and draw sheet and was considered by the nursing profession to be too elaborate. It is essential therefore to redesign the bed-pan itself, which should be deeper and more stable. This is an unpleasant subject to discuss in print, but the comfort of our patients must always be our first thought and justifies calling attention to the matter.—I am etc.,

London, W.1.

MALCOLM DONALDSON.

Medical Photography

SIR,—In the concluding section of "Where are we Going?" Dr. Ffrangcon Roberts (March 13, p. 485) says that economics is a dismal science. But surely he is being a little too dismal. He is particularly unfortunate in his choice of clinical [*sic*] photography as a scapegoat. The present growth of medical photographic departments (the term "clinical" is too restricted to cover their work) has been brought about mainly by a widening realization of the value of visual methods in education.

We in this country, with a few noted exceptions, had been lagging behind the United States in our use of visual methods of instruction, but during the late war we were compelled to increase the efficiency of our technique for the simple reason that the need for more rapid training was paramount. It was

shown time and again that the great majority learn better and faster by using their eyes than by using their ears. There is now a widespread desire among teachers to apply this wealth of experience to medical teaching in general, and I feel sure that Dr. Roberts would be the first to support anything which can lighten the almost intolerable burden of the present curriculum. Agreed that the facilities must be intelligently employed, but it is quite certain that the demands of teachers for the products of the department are wellnigh overwhelming, in spite of expansion.

Dr. Roberts does not give the sources of his quotations, but I can recognize them with the exception of the reference to radiology. The first article was widely felt at the time to be "castles in the air" and likely to frighten people away, and several writers said so. I can safely assure him that it does not represent the opinion of the majority of those connected with departments of medical photography. The statement that the staff will need a long and complete training needs no support. If the new qualification set up by the Institute of British Photographers, the examining board for, which includes qualified medical men, is to mean anything, its standards must be high. And I have yet to find evidence that an expert in any branch of photography, or for that matter in anything else, can be made overnight. The quotation concerning cramped quarters is torn from its context, and does not suffice to convey the author's meaning. Would Pasteur, Thomson, and Hopkins have been any the worse for good accommodation? Surely this is merely a passion for what the Services used to call "doing it the hard way"?

Finally, I would suggest that the present increase in medical photographic departments in our teaching hospitals does not indicate an urge for expansion; the teaching staff are not all suffering from delusions of grandeur. It indicates only a very reasonable desire for high quality in recorded and instructional material, and a determination that those who provide it shall work under reasonable conditions. There is also every chance that it will lead incidentally to a much-needed improvement in the standard of pictorial and line work submitted to the journals for publication.—I am, etc.,

Manchester, 13.

ROBERT G. W. OLIVERENSHAW.

Bone Conduction in Otosclerosis

SIR,—What is most urgently needed to-day in regard to the over-publicized fenestration operation for otosclerosis is a careful, sober, and dispassionate estimation of end-results. After Simson Hall had performed 200 operations he reviewed 118 of them only as suitable for analysis (*Journal*, 1946, 2, 647); as long ago as January, 1945, Lempert, of New York, published a series of 1,000 fenestration cases, and Shambaugh, of Chicago, recorded 930 cases in 1946. And here is Mr. Magauran reporting a case in the *Journal* (March 20, p. 569) six weeks after he had left hospital and only 12 weeks after his first (of two) fenestration operations, and apparently the patient's hearing was never examined with an audiometer before or after operation.

Otologists in general agree that two years (some optimists one year) is the period after which the hearing result from fenestration may be regarded as permanent. Suggit found in 1938 that there was an average fluctuation of 10 decibels in a group of stationary cases of otosclerosis when examined methodically with the audiometer. Psychological deafness is well recognized nowadays, though not many otologists have found as many cases as Canfield (1947) did in his war veteran patients, and there is undoubtedly a marked psychological element in many otosclerotics. As for bone conduction in otosclerosis, most otologists would, I think, agree with Tumarkin (1947) that the myth of increased bone conduction has long been exploded.—I am, etc.,

London, W.1.

R. SCOTT STEVENSON.

Brodie's Abscess

SIR,—In the interests of accuracy it seems desirable to add to the article by Messrs. Stanley Scott and Frank S. Preston (Feb. 14, p. 296) and to Mr. St. J. D. Buxton's letter on this subject (March 6, p. 472). The latter rightly points out that although Brodie saw the first of his cases for the first time in 1824 he did not actually publish anything on the subject in