dilators. Recently, too, a patient with pemphigus vulgaris who had been on massive doses of corticosteroids died unexpectedly and necropsy showed this to be due to pulmonary embolism from silent phlebotrombosis. (I hope this does not provoke discussion on the relationship of steroid treatment to this.)

It is impossible to say why my patient died. The purpose of my original letter was to show that asthma is still very difficult to treat and that the addition of a further technique (hypnosis) does not provide us with a panacea.—I am, etc.,

Ransom Hospital,
Rainworth, nr. Mansfield.

D. DAVIES.

Professorial Units

SIR,—There seems to be a move to establish professorial units in medicine and surgery in teaching hospitals. Has any work been done showing that advantage accrues from combining teaching and research?

As I see it, teaching is an art, not a gift, which can be acquired but is often neglected. It is a fallacy to assume that a person who is elected to the staff of a teaching hospital because he has a first-rate knowledge of medicine, or (as sometimes happens) because he is a good chap, can necessarily teach. I think it is equally fallacious to suppose that the ability to make advances in medical knowledge is in any way correlated with the ability to impart what is already known. I know of no systematized collection of facts which has established the role of the professorial unit in undergraduate teaching. It seems to me that anecdotal evidence and opinion are too slender grounds to make the radical changes which are envisaged and in many cases already under way.—I am, etc.,

Frinton, Essex.

RUSSELL BARTON.

"Persantin" in Coronary Disease

SIR,—Drs. Tom Foulds and John Mackinnon in their paper (September 17, p. 835) state that they did not find "persantin" to be effective in patients with angina of effort. The same result was reported in a letter by Drs. Johannes Mosbech and Preben Ollendorff (November 5, p. 1385). I should like to make the comment that these two reports do not concur with the findings of many other investigators. My own findings¹ were also different. I gave 12.5–25 mg. persantin thrice daily orally to 46 patients with typical angina pectoris ambulatoria. In 30 of the patients I observed definite improvement in respect of frequency, intensity, and duration of the angular attacks.

We all realize, of course, that in studies of this kind subjective statements of the patients must be used as criteria. I do not think, however, that from the two reports referred to above the conclusion can be drawn that persantin is ineffective in angina. The report of Drs. T. Foulds and J. Mackinnon covered 30 particularly severe cases—within five months five of the patients (17%) had died. The report of Dr. J. Mosbech hardly allows of a statistical evaluation since the number of patients was very small; of the original 11 patients one died and two could not be followed up, so that only eight patients were included in the results.—I am, etc.,

Vienna, 10.

OTTO LANG.

REFERENCE

¹ Lang, O., *Wien. med. Wschr.*, 1959, 109, 1006.

Haematemesis and Myocardial Infarction

SIR,—Dr. P. Curzen (November 19, p. 1489) in his article "Management of Acute Myocardial Infarction Complicated by Haematemesis" discussed the difficulty of introducing a large-size polythene cannula. A simpler method has been described. A nylon cannula, fitted with a Luer mount, is introduced into the selected vein using a special needle designed by MacGregor.¹ Half the circumference of the distal part of the needle is absent, so the cannula may be inserted into the shaft of the needle and not through the hub. The needle, carrying the cannula, is inserted into the selected vein, the cannula is threaded up the vein and then the needle is removed. This method has been used successfully for "pitocin" (oxytocin) drips since the original article appeared. It has been possible to discard arm splints. It is hoped to publish a detailed paper later.

The nylon cannulae are made by Portland Plastics Ltd., Bassett House, Hythe, Kent, and the MacGregor needles by C. F. Thackray, Ltd., Park Street, Leeds. The cannulae vary in size from 2/0 to 6 and there are appropriate needles for each. The size of the needles vary from 18G to 11G (blood transfusion needles varying in size from 17G to 13G).

—I am, etc.,

Tyrone Co.nty Hospital,
Omagh, Co. Tyrone.

JOHN F. O'SULLIVAN.

REFERENCE

¹ MacGregor, J. D., *Lancet*, 1960, 1, 528.

SIR,—In the article by Dr. P. Curzen (November 19, p. 1489) on the management of acute myocardial infarction complicated by haematemesis, I was interested in his use of a 6-in. (15-cm.) length of polythene cannula, together with adapters, for the administration of heparin and noradrenaline. He refers to the use of a cut-down on the long saphenous vein at the ankle, and his photograph shows at least 3 in. (7.6 cm.) on the surface of the foot before penetrating the skin. I would be very wary of administering a noradrenaline drip at this site with such a short cannula. To avoid the complication of ischaemic necrosis due to the local action of the noradrenaline I would recommend a length of not less than 9 in. (22.6 cm.) to be inserted up the lumen of the vein so that there is unlikely to be any leak-back into the tissues around the ankle. I have seen a nasty gangrenous foot as a result of using only a short cannula.—I am, etc.,

Aylesbury, Bucks.

WM. L. HOOPER.

Vanillic Acid Diethylamide

SIR,—In a paper presented to the American Therapeutic Society earlier this year, Dr. M. J. Miller commented very favourably on the use of the convulsant drug vanillic acid diethylamide as both a respiratory and a vasomotor stimulant. Vanillic acid diethylamide has the generic name of "ethamivan" in the United States. This substance was first commented on in the British literature by Dr. E. K. Gardner.¹

Vanillic acid diethylamide has now been used by my unit for more than eighteen months. When injected intravenously using single doses short of producing convulsions (up to 500 mg.), a stimulant effect upon respiration is apparent within 10 seconds, usually maximal in 30 to 60 seconds, and lasting from 10 to 20 minutes. Repeated single doses at short intervals have shown no cumulative effect and no apparent falling off of activity. The respiratory rate increased from initial rates of 11–15/minute to 20–24/

minute, and the tidal volume increased from an initial 200–350 ml. to 450–900 ml. The respiratory rate tends to be maintained during most of the time of activity of the drug; the tidal volume falls rapidly after the peak effect at 30–60 seconds but remains appreciably increased for 12 to 20 minutes. In most patients we were unable to demonstrate any effect upon blood-pressure or heart rate. On no occasion did convulsions occur without a preceding period of respiratory stimulation. The rapid onset of respiratory stimulant effect and its restricted duration of action and apparent absence of cumulative effect made this drug safer, easier to administer, and therefore more acceptable clinically than the convulsant picrotoxin.

By using a continuous drip it was possible to overcome the short duration of action of single injections. Solution strengths of 0.5% to 1.0% in 5% glucose or normal saline appear to be most useful. We have given continuous drips of these solutions at rates of 0.5 to 1.5 ml. (3 mg. to 10 mg.) per minute for periods up to 72 hours with continuous respiratory stimulation. Prolonged administration did not appear to be followed by any late psychiatric disturbance as has been described for other respiratory stimulants, although the drug appears to have some euphoriant activity in conscious patients.

The E.E.G. of patients in coma who received this substance was reported as showing an awakening pattern. Though the drug not infrequently induced coughing, sneezing, and limb movements in patients in coma, it had no apparent awakening effect on them beyond this, and did not appear to appreciably shorten the duration of coma. In a few stuporous uncooperative patients, however, it produced a marked, though short-lived, increase in co-operation, making feeding and toilet much less arduous.

Our main clinical experience relates primarily to the drug's use in respiratory failure due to various poisons, including barbiturates and other hypnotics and sedatives. Our experience of its use in pulmonary disease is very restricted. If a chemical respiratory-stimulant is considered necessary, vanillic acid diethylamide, which was the most potent chemical respiratory-stimulant with this pattern of action of several that we have used, would appear to be an effective and easily controllable agent.—I am, etc.,

S. LOCKET.

Oldchurch Hospital,
Romford, Essex.

REFERENCE

- ¹ Gardner, E. K., *Brit. J. Anaesth.*, 1958, **30**, 155.

Trichophyton Sulphureum Infection

SIR,—Drs. James C. P. Logan and P. Derblay in their letter on *T. sulphureum* (November 19, p. 1523) suggest that segregation of infected individuals is all important in eradication of an outbreak. Whilst acknowledging that segregation is epidemiologically desirable, we think it will be of value only if all infected individuals can be segregated simultaneously. With non-inflammatory *T. sulphureum* of the scalp this is virtually impossible, since detection requires an extended period of both clinical and mycological observations. By the time cases deserving segregation on clinical grounds are found, infection is probably widespread and several other children may have subclinical infections so difficult to detect on physical examination alone that a diagnosis is more readily made following mycological sampling of their possessions, especially hairbrushes and combs. Segregation is a measure we have avoided whenever possible, partly because it is so inconvenient to school and parents alike, but principally because it is educationally, psychologically, and socially bad for the child. A point of considerable epidemiological significance is the degree of infectivity of the pathogen. All the

evidence we have accumulated in our studies has indicated that *T. sulphureum* is not highly infectious, and this factor was taken into account in deciding our course of action.

Before "eradication" can be claimed at an institution we would advocate that mycological sampling along the lines described in our paper is essential. In the epidemic we described infection was not entirely suppressed despite the rigorous, protracted, and varied methods of eradication that were adopted. With less strict criteria and a shorter follow-up period, an erroneous claim of complete eradication might have been made. It would be interesting to know if mycological samplings at the institution mentioned by Drs. Logan and Derblay would yield positive results although two years of apparent clinical freedom from infection have been reported. In view of the inefficacy of clinical detection it does not appear to us that a claim for complete eradication by clinical examination alone can be valid.

The value of griseofulvin in epidemics of this nature is to provide an acceptable substitute for x-ray epilation, which in minimal infections caused by this fungus is unjustified. We did not make excessive claims for the effectiveness of griseofulvin, nor did we suggest that griseofulvin alone could eradicate an epidemic. Other control measures are essential. We believe that "hair-brush diagnosis" fulfils the role played by a Wood's lamp in fluorescent ringworm of the scalp—viz., detection of infected individuals, estimation of the duration of infection, and the examination of contacts. Under controlled conditions it is a sensitive technique and can be used as an absolute indication that eradication of infection is complete.—We are, etc.,

Department of Microbiology,
Queen's University of Belfast.
Belfast County Borough
Health Department.

D. W. R. MACKENZIE.

A. L. WALBY.

Congenital Deformity

SIR,—I am grateful to Mr. Norman Capener (November 26, p. 1596) for his kind remarks about my work in a field which, he reminds us, was described as of great promise by Sir Robert Jones. That distinguished surgeon and generous man encouraged me when I was a frustrated registrar by coming to see my work on talipes; he remains the only British orthopaedic consultant to have done so. But then Mr. Capener goes on to suggest that this field is worked out and that "the future lies elsewhere." Referring me to a half-sentence of his own¹ as evidence, he reproves me for assuming I have been working alone. Well, who are the others? I can find no analysis of possible effects of intrauterine mechanics on the foetus in any textbook of embryology, obstetrics, pathology, paediatrics, neurology, or orthopaedics, with the exception of the 40-year-old Dutch work he refers to. It is significant that though there have been two international conferences on congenital deformities in London this year, in neither was any reference made to ideas of this kind, nor was I permitted to make one.

The obstructions to recognition are three: the present medical system of closed specialties, the natural reluctance of professors to encourage work that may upset their own teaching, and my use of a method of research other than the experimental one which is alone recognized in medicine. I have employed that used by astronomers—observation followed by reasoning; if my work had a foundation