after qualification and it is this inappropriate training and unfulfilled expectations which lead to a rapid decline of morale at all levels.

I would disagree that paramedicals and auxiliaries are reluctant to work in the rural areas—unless they have been trained in the capital. It is essential that these cadres be selected and trained in the provincial areas where they will work. Auxiliaries are invariably in a service situation and realize that their opportunities lie in the under-doctored areas—not in competition with physicians but as an extension of medical care. Medical students should also be trained at a provincial level and in a provincial hospital instead of a capital-based national teaching hospital. For all cadres the vital essential is a job description with training founded on that job description.

I fully agree that an isolated worker, doctor or "lesser-trained worker," develops extraordinary habits, both professional and social. It has long been known that without regular supervision, consultation, and encouragement peripheral workers at all levels will fail. The onus lies on those who should supervise them. I take serious exception to the term "lesser-trained worker." They are appropriately trained for their job requirements after a careful analysis of the job requirements. The degree of training for one cadre cannot be judged in the context of another cadre's job description.

Curriculum adjustment may lower standards but only if the curriculum is not in accordance with the requirements of the job. For this reason many African and Eastern countries are now holding their own postgraduate examinations—the U.K. training failed them in the work they require their own nationals to undertake. For this reason this school's M.Comm.H. course includes a three-month problem-solving exercise in a developing country and the teaching is orientated towards the developing countries and minorities. We are now unable to keep up with the increasing demand for this course.

The first commandment is to formulate a full job description, the second to formulate a training course to meet that job description, and the third is to supervise and assess in the field.—I am, etc.,

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The Suicide Profile

SIR,—Might I take issue with you on two points raised in your timely leading article (7 June, p. 525)?

You state that "depressive illness accounts for at least two-thirds of cases" of suicide. I assume the source of this statement to be the recent study of Barraclough *et al.*[1] Suicide has been dealt with by many eminent writers of fiction. Invariably a meaningful connexion is drawn between an intolerable life situation and the suicide bid. Are we now to conclude that these writers have all got it wrong? How reliably can a psychiatrist retrospectively diagnose depressive illness on the evidence of distraught relatives? Since there are no objective criteria for such a diagnosis and since it merges quite arbitrarily into ordinary unhappiness it is a matter of chance where he draws the line. It is noteworthy that in Sainsbury's study,[2] which you quote, the incidence of depressive illness was only 27%. It is unlikely that many people have killed themselves in anything but a state of unhappiness, but this is not to say that the unhappiness *accounted for* the suicide. One might equally say that since a high proportion of brides were happy during the month before marriage the wedding was accounted for by the happiness.

You further state that "any depressed patient may and many do" commit suicide and that "depression can be and frequently is a fatal complaint." "May" and "can," yes, but "many do" and "frequently is," hardly. The proportion of depressed patients who kill themselves must be very small indeed. Barraclough *et al.*[1] reported that only 6% of a series of depressives had even attempted suicide, whereas almost half of their depressed suicides had previously done so. One may conclude that in every general practice there is a pool of chronically suicidal individuals. The problem, as you rightly stress, is how to identify them. Here you spoil your case by referring to some statistically derived variables, which are of little help in assessing an individual. Rather than totting up points for and against, the general practitioner could more profitably employ his valuable time creating an atmosphere of confidence in which the patient would feel able to confess his suicidal thoughts. Barraclough *et al.* reported that a staggering two-thirds of suicides had visited their general practitioners during the month before death. At this time most were probably harbouring suicidal thoughts. It is alarming that 80% had been prescribed psychotrophic drugs. Couple this with the recent finding of Morgan that 80% of suicide attempters had been prescribed the drugs they took by a doctor.

Were we to view suicide not as the possible fatal outcome of an illness but as the temporary loss of hope of a fellow mortal we might save more lives.—I am, etc.,

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1 Barraclough, B. M., *British Journal of Psychiatry*, 1974, **125**, 355.
2 Sainsbury, P. *Suicide in London*. London, Chapman and Hall, 1955.
3 Morgan, H. G. Personal communication.

SIR,—Dr. C. A. H. Watts (5 July, p. 42) says that "the coroner today bends over backwards to avoid the verdict of suicide, so that the statistics are an underestimate of the true state of affairs."

The question of what is or is not a suicide is a matter of law and not of medicine and it is interesting to recall that on 2 July *The Times* newspaper carried a news report (p. 5) under the heading "Suicide Verdict Quashed and Coroners Warned."

In the Queen's Bench Divisional Court the Lord Chief Justice said, according to the press report: "The coroner's approach seems to have ignored one of the most important rules—that suicide must not be presumed because it seems the most likely explanation. If it cannot be proved by evidence he should find an open verdict."

This is the latest of a series of similar pronouncements by the Divisional Court and the rule which it states must be followed by coroners as judicial officers. It is not a question of bending over backwards, sideways, or forwards but of giving a decision in accordance with the law of the land.—I am, etc.,

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Cost-benefit Analysis of Long-term Haemodialysis for Chronic Renal Failure

SIR,—To rely on the average wage as an indicator of economic benefit to society of the use of dialysis (Mr. M. J. Buxton and Dr. R. R. West, 17 May, p. 376) is highly misleading. For instance, it excludes the benefit of life to patients who do not work and also evaluates the lives of those women who work as half that of men. Policy conclusions which might be derived from this are therefore absurd.

Cost-benefit analysis is about measurements of social costs and benefits of activities. The measurement of benefits of health care presents great problems because the society and individuals stand to gain far more from health care than their ability to contribute to the productive capacity of the nation, and indeed wages are a poor measure even of the latter.—I am, etc.,

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A Question of Diagnosis

SIR,—I was interested in Dr. D. C. Anderson's presentation of "A Unique Case of Iatrogenic Cushing's Syndrome" (5 July, p. 37). Is the diagnosis correct? Could this not be just a case of Wilson's disease?—I am, etc.,

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Slow-K Ulceration of Oesophagus with Aneurysmal Left Atrium

SIR,—Drs. A. D. Howie and R. W. Strachan (26 April, p. 176) draw attention to the danger of oesophageal ulceration when Slow-K tablets are administered to patients with left atrial enlargement. In their own case the dysphagia and ulceration followed insertion of a mitral prosthesis, as in the examples they quote from the literature. The following case shows that this complication may occur in the absence of surgical intervention.

A woman was found at the age of 33 to have aneurysmal dilatation of the left atrium with mitral stenosis and regurgitation and aortic regurgitation. She was in atrial fibrillation but was well compensated on digoxin. At the age of 44 she was admitted in heart failure and frusemide 80 mg per day together with Slow-K two tablets twice daily was prescribed. After 51 days of this regimen she died from a massive haematemesis. No dysphagia was recorded, but an episode of retrosternal pain occurred early during the treatment and this lasted for some days.

Necropsy showed moderate mitral stenosis and aortic stenosis with incompetence. The left atrium measured about 15 cm across, with a characteristically normal-sized atrial appendage. There was ulceration of the oesophagus below the carina. The ulcerated area involved about 5 cm of the oesophagus leaving 4 cm of apparently normal oesophagus

between the ulcer and the cardiac orifice. The ulcer was irregular in shape with a 1-cm proximal lesion on the anterior wall and a distal 4-cm lesion immediately below it which encircled most of the oesophagus and which communicated with the aorta through a channel 0·3 cm in diameter. It was thought that the entrance to the aorta was at the site of an intercostal vessel which had been involved in the ulcer base. There were adhesions between the aorta and the oesophagus. Altered blood was present which was adherent to the ulcer base and the stomach contained a cast of clot. No radiographs of the oesophagus were available. At necropsy the ulcerated area appeared to occupy the midline.

The necropsy was performed in 1967, and, though it was realized that distortion of the oesophagus by the enormous left atrium must have determined the site of the ulceration, it was assumed that this was peptic ulceration and the significance of Slow-K administration was not realized until Drs. Howie and Strachan's article led to re-examination of the case-sheet. No doubt pulsation of the aorta augments the damage to the oesophageal wall that results from any firm object arrested between the aorta and an enlarged left atrium.—I am, etc.,

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Haemoglobin Binding Curve and Oxygen Transport

SIR,—Dr. D. C. Flenley and his colleagues refer (15 March, p. 602) to the potentially adverse effects on oxygen delivery to the cells of a shift to the left in the oxyhaemoglobin dissociation curve. They envisage that advantage may accrue, in patients with fixed low cardiac output, from the use of a drug (should one be developed) which will return the displaced curve to the right.

Such drugs exist; they act by producing acidosis. No doubt Dr. Flenley and his colleagues dismiss this as a practical way of treating patients with shock, and very properly so. Nevertheless, acidosis has been deliberately induced for a precisely similar purpose, to counteract the leftward shift of the dissociation curve in hypothermic total body perfusion. Edmark[1] and Carson *et al.*[2] advocated the addition of hydrochloric acid to the perfusate, or perfusion at raised P_{CO_2}, to maintain a pH of 7·3 in arterial blood at 30°C.

This practice was rather transient, and there is now ample evidence that perfusion at pH 7·5-7·6 at 30°C is not harmful, though temperature and pH combine to shift the dissociation curve to the left.[3] Indeed, an extracellular pH of 7·6 may be the appropriate one to secure cellular neutrality at 300°C.[5] We have shown[1] that during bypass, whether normothermic or hypothermic, mixed venous P_{O_2} and saturation are inversely related to the availability of oxygen to the cells—the lower the P_{O_2} for a given rate of perfusion, the better, in general, is the delivery of oxygen. The position of the dissociation curve is apparently of far less importance than the state of the microcirculation. If blood can be brought within reach of the cells, so to speak, they will take up oxygen at a very small driving pressure. During the postoperative period, Kirklin and Theye[6] found that arteriovenous oxygen content difference was unreliable as a guide to the adequacy of cardiac output; this means that mixed venous P_{O_2} is very poorly related to oxygen delivery in these circumstances.

I recognize that cardiac and other forms of "medical" shock may present different problems. Yet I think that here, too, it is easy to overstate the importance of the position of the dissociation curve at the expense of more vital considerations. I am sure that Dr. Flenley and his colleagues are well aware of the pitfalls in imposing what we suppose to be "normal" conditions in patients with complex disturbances of function. While nature does not always make optimal compensations, she does so often enough to make us look before we leap.—I am, etc.,

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1 Edmark, K. W., *Surgery, Gynecology and Obstetrics*, 1959, **109**, 743.
2 Carson, S. A. A., *et al.*, *Circulation*, 1964, **29**, 456.
3 Harris, E. A., Seelye, E. R., and Barratt-Boyes, B. G., *British Journal of Anaesthesia*, 1970, **42**, 912.
4 Harris, E. A., Seelye, E. R., and Barratt-Boyes, B. G., *British Journal of Anaesthesia*, 1974, **46**, 425.
5 Rahn, H., in *Symposium on Development of the Lung*. London, 1965, ed. A. V. S. de Reuck, and R. Porter, p. 3. London, Churchill. 1967.
6 Kirklin, J. W., and Theye, R. A., *Circulation*, 1963, **28**, 1061.

Breast Cancer in Young Women

SIR,—I read with interest your excellent leading article (21 June, p. 649). I agree that it would seem unwise for women to have further children once the diagnosis is made, especially where there are grave prognostic signs such as extensive axillary node deposits. Because supraphysiological doses of oestrogen on occult disseminated foci of established cancer may promote growth, tubal ligation is advised rather than oral contraception. It has the added advantage of being permanent.

I feel brief mention should be made regarding the small place of oophorectomy. Where there is axillary node involvement, distant metastases, particularly with bone pain, or suspected ovarian tumours removal of the ovaries should be considered as a possible alternative. The principal disadvantage is the likely premature menopausal symptoms which add to an already distressing situation for both wife and husband. Information about the effects of synthetic and natural oestrogens in various concentrations on such parameters as DNA synthesis in vitro in breast cancer could be helpful clinically. Certainly in some patients intractable bone pain may be gratefully alleviated by oophorectomy.—I am, etc.,

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Digoxin and Confusion in the Elderly

SIR,—While discussing the drugs which cause confusion in elderly people Dr. H. M. Hodkinson (5 April, p. 23) has omitted an important drug which is commonly used in medical practice both inside and outside the hospital. I refer to digoxin, which is known to cause confusion in elderly people. The neuropsychotic effects of digoxin include disorientation, confusion, aphasia, and even delirium and hallucinations. They are especially likely to develop in elderly arteriosclerotic patients.[1] Confusion may be the presenting feature; it occurs in about 25% of cases of digitalis intoxication.[2 3]—I am, etc.,

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1 Goodman, L. S., and Gillman, A., *The Pharmacological Basis of Therapeutics*, 4th edn., p. 694. New York, Macmillan, 1970.
2 King, J. T., *Annals of Internal Medicine*, 1950, **33**, 1360.
3 Dall, J. L. C., *Lancet*, 1965, **1**, 194.

"Orthopaedic Beds"

SIR,—There is a great vogue at the moment for unyielding beds. They are advertised to the public with the suggestion, backed up by diagrams, that a hard bed keeps the backbone beneficially straight. It is the contrary: the softer the bed, the straighter the spine, so long as the resiliency of the mattress is uniform. Since the body is so much wider at the shoulders and pelvis than at the waist the thoracolumbar spine can be kept straight only if these two prominences are accommodated by yielding springs. On a hard bed the sleeper's spine is held in side-flexion away from the side he lies on. The same applies to the supine sleeper, whose thoracic and gluteal curves cannot sink into a hard mattress; he thus has his lumbar spine maintained in slight flexion all night. What is of course harmful is localized sagging at the centre of a mattress, whereby the sleeper's lumbar spine droops into kyphosis, as it would in a hammock.

The only mattress that can usefully be called "orthopaedic" is one with a small hump right across it, level with the sleeper's waist. On that, if he lies on his back, his normal lordosis is maintained; if he lies on his side his spine is supported horizontally. —I am, etc.,

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Risks of Total Hip Replacement

SIR,—Professor J. Charnley's recent letter (31 May, p. 498) on the risks of total hip replacement may lead the unwary to believe that the operation—excellent though it is—carries no risk of intraoperative circulatory collapse. An unbiased reading of the final report to the Department of Health and Social Security of the Working Party on Acrylic Cement in Orthopaedic Surgery[1] leads inescapably to the conclusion that such a risk is real and has, in the past, led to fatalities.

It is important, as Professor Charnley suggests, to benefit from hindsight and to set the record straight from the historical point of view. In the four years that have elapsed since fatalities were first reported nothing much has changed so far as the characteristics of the patients, the application of the cement, or the operative techniques are concerned. What has developed, however, is an increasing awareness of the risks involved and an understanding of their