officers than they had been asked to do. Details are as follows:

<table>
<thead>
<tr>
<th>Royal Commission</th>
<th>B.M.A.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pre-registration house officer,</td>
<td></td>
</tr>
<tr>
<td>1st post ... ... ...</td>
<td>£675</td>
</tr>
<tr>
<td>2nd post ... ... ...</td>
<td>£750</td>
</tr>
<tr>
<td>Fully registered house officer,</td>
<td></td>
</tr>
<tr>
<td>1st post ... ... ...</td>
<td>£825</td>
</tr>
<tr>
<td>Later posts ... ... ...</td>
<td>£825</td>
</tr>
</tbody>
</table>

Has there ever been an occasion previously when an arbitration tribunal awarded a trade union a larger pay increase for a section of its members than had been asked for?

The present position is that all hospital medical staff from the level of senior registrar downwards are grossly underpaid relative to the work they do in terms of hours and responsibility. What steps are being taken to rectify this position? Perhaps this situation contributes to the notion that B.M.A. subscriptions are too high. It was fortunate for junior hospital staff that the Royal Commission was realistic enough to accept the evidence of more enlightened medical organizations which were in a better position to present the facts. Indeed the whole profession owes a great deal to the work of these small bodies whose dedicated services to truth ensured that a wider range of evidence was available to the Royal Commission than would have been possible if the B.M.A. alone had been available. These organizations also have to be supported by subscriptions which tend to go up.—I am, etc.,

Watford, Herts.

P. W. ROE.

Sir,—I sympathize with the previous correspondents who don't want any examinations bedevilling medicine. But I think it is important and desirable that there should be a postgraduate qualification which indicates a certain degree of competence in the specialty of general practice such as the other specialties possess.

It is not of course necessary that such a qualification be associated with the College of General Practitioners, as any of the universities could offer such a postgraduate qualification. However, I see no objection to the College of General Practitioners offering a qualification as M.C.G.P., but I object most seriously to present members of this College being granted this title without examination of their qualities, whereas other practitioners will have to prove their competence.

The report of the Examination Committee states quite clearly "it will in no way affect those who are already members of the College." If there is to be a test of our qualities, then let it be by examination (no other method) for all concerned whatever our ages.—I am, etc.,

Wallsend.

S. J. GILL.

Adrenaline and the Foetal Circulation

Sir,—In his paper on adrenaline and the foetal circulation (February 17, p. 443) Mr. R. W. Beard describes observations on pregnant women to whom sufficient adrenaline or noradrenaline was given intravenously to cause "severe foetal bradycardia." If the bradycardia was due to foetal anoxaemia then such procedures are potentially hazardous. The degree of hypoxia required to cause foetal bradycardia in experimental animals induces a profound fall in foetal oxygen consumption.

Alternatively the bradycardia may have been due to transplacental passage of these amines, causing a rise in foetal blood-pressure—there is much evidence to suggest that the baroreceptor reflexes are functional in the foetus. The experiments of Dornhorst and Young and of Adams et al. were quoted to support the view that the foetal circulation is relatively insensitive to adrenaline and noradrenaline. The observations of Dornhorst and Young have been discussed elsewhere and reasons have been adduced for their failure to observe changes in blood-pressure on injection of small doses of sympathetic amines. To these may be added the fact that they made intrafoetal injections into a jugular or vitelline vein, whereas (because of the course of the foetal circulation) it is necessary to inject into a femoral vein in order to mimic the effects of injection into a superficial vein after birth. The observations of Adams et al. on only four foetal lambs are irrelevant, not only because they appear to have tied one umbilical artery but also because they injected adrenaline or noradrenaline into an umbilical vein. It is well known that these amines are taken up in large amounts on a single passage through the liver, and this explains why injection into an umbilical vein causes a smaller rise of blood-pressure than an injection into a femoral vein in a foetal lamb.

There is therefore no justification for supposing that the tissues of the foetus are inherently less sensitive to sympathetic amines. Some recent experiments have indeed suggested that the umbilical circulation may be
unusually sensitive in a premature foetus, and instances of acute vasoconstriction with a drastic fall of umbilical flow have been observed (after infusion into a foetal femoral vein). Thus even if these amines do act by crossing the placenta there is a potential hazard.

Comparatively little is known about the effects of many drugs upon the foetus, and until more experiments have been done on animals (the monkey is available if a primate placenta is thought desirable) observations which can lead to “foetal distress” in human infants may involve an avoidable risk.—I am, etc.,

The Nuffield Institute for Medical Research,
Oxford.

G. S. DAWES.

REFERENCES
1 Dornhorst, A. C., and Young, I. M., J. Physiol., 1952, 118, 282.
4 ——— Mott, J. C., and Rennick, B. R., ibid., 1956, 134, 139.

Twins and Heredity

Sir,—In their extensive twin study Dr. A. G. Marshall and his colleagues (January 6, p. 1) have indicated many of the difficulties encountered in reaching valid conclusions regarding genetic versus environmental factors in disease when twin data are analysed. It appears that decisions regarding zygosity based on personal inspection and family history are not accurate enough. Checking this diagnosis by blood-typing in 44 of 2,537 pairs, they found an accurate determination of zygosity in only 36 pairs. It is impossible to judge whether this degree of deviation is representative for the entire study, but it seems necessary to employ extensive blood-typing as well as other similarity tests for the diagnosis of zygosity, to draw firm conclusions from twin studies. It is appreciated that these are not only cumbersome but also costly. Nevertheless, such effort would be well worth while to put specific genetic or environmental influences on disease on a more secure footing.

It is commonly assumed that one-egg twins are genetically identical and deviation of known genetic traits in a set of twins generally rules out the diagnosis of monozygosity. These conclusions are based on a large number of characteristics and firmly supported by identity of sex and blood-groups as well as acceptance of switch-homografts. If exceptions occur to these rules it would be difficult to defend monozygosity for such a set of twins by current thinking. Nevertheless, recently Turpin et al.1 2 described a pair of apparently single-ovum twins, one a normal XY male, the other an XO Turner’s syndrome. Blood-grouping and switch-grafting supported monozygosity. In a pair of twins3 with identical blood-groups but phenotypic differences a kidney transplant functioned for a few months and then showed modified homograft rejection; the skin grafts were also rejected and the twins were stated to be “non-identical” but zygosity was not firmly established.

While prolonged survival of skin grafts is widely used to diagnose zygosity it may not necessarily distinguish if segregation of histocompatibility determinants occurs asymmetrically at cleavage. Moreover, cross-circulation in placentas as seen in some identical twins and rarely in fraternal twins (chimerae) can lead to acquired tolerance.

There is irrefutable evidence that the quadruplets of pregnancies in the nine-banded armadillo, Dasypus novemcinctus, are derived from a single fertilized ovum. Despite this fact of established monozygosity, five of eight grafts between litter-mates underwent complete homograft destruction between the 17th and 34th days after transplantation. One further graft showed a chronic process of rejection from the 42nd to 80th day and two had not been rejected on the 20th day when the recipients died. From these findings it is concluded that either the genetic determinants for individual specificity in this species are not symmetrically distributed among the four embryos or that unusual environmental circumstances influence the development of these characters. Possibly both interact.

It is uncertain whether this direct evidence of tissue incompatibility of monoyzgous quadruplets has a counterpart in man. If so, they certainly would be modified by the different type of placenta. The apparent lack of circulatory anastomoses in the armadillo placenta contrasts with 70%4,5 of human monozygous twins who share their circulatory beds and would be subject to tolerance. The spectrum of twins—fused monster to dichorial twins—indicates wide varieties of times of the onset of twinning in man, in contrast to the regular phenomenon in armadillos. This then may also be paralleled by a spectrum of difference in antigenic or genetic constitution. Only long-term transplant results in human twins combined with an accurate knowledge of the placental status4 will resolve these fundamental questions.—I am, etc.,

Department of Pathology, Dartmouth Medical School, Hanover, New Hampshire, U.S.A.

REFERENCES
5 ——— ibid., 1961, 18, 334.

Schizophrenia in Old Age

Sir,—The “disagreement” with our views to which Dr. Frank Fish refers in his letter (February 10, p. 407) was based on some strange reasoning. The starting point of the argument was the incidence (6%) of schizophrenia diagnosed from the case records of patients aged 60 and over admitted to hospital in Edinburgh in 1957.1 2 The proportion of patients with late paraphrenia admitted during six years to the one hospital in West Sussex where our work was done was 9.9%.3 Discovering that the addition of “paranoid depressions” and “psychogenic reactions” to the schizophrenic cases in his material brought his figure up to 9.4%, Dr. Fish seems to have concluded that the only possible explanation for the discrepancy is that late paraphrenia is a heterogeneous group. But the obvious differences between the populations from which the two groups of cases have been drawn is virtually ignored. Thus all our cases had been admitted to one mental hospital serving a rural catchment area, whereas Dr. Fish’s cases had been drawn from two public mental