after induction and vaginal delivery (30%). Equal numbers of infants were delivered by induction or caesarean section, but the incidence of neonatal jaundice was significantly higher ($\chi^2=5.06$; $P<0.025$) in the former group. The mean time of delivery for the induction and caesarean section groups was 37 weeks and 36.5 weeks respectively, while infants delivered by the vaginal route were on average 0.4 kg heavier.

Vaginal delivery is generally the preferred mode of delivery for diabetics,² but it is important to recognise that when premature delivery is induced by this route severe neonatal jaundice is a common problem. On the other hand caesarean section has been associated with other neonatal complications which also represent a serious hazard to the newborn of the diabetic.

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¹ White, P, *American Journal of Obstetrics and Gynecology*, 1978, **130**, 228.

² Drury, M I, Greene, A T, and Strojge, J M, *Obstetrics and Gynecology*, 1977, **49**, 519.

³ Soler, N G, and Malins, J M, *Diabetes*, 1977, **26**, suppl 1, p 260.

College of Anaesthetists

SIR,—Since 10 distinguished anaesthetists have chosen to use your columns as a platform to discuss this controversial issue (4 March, p 574) I would be grateful for the opportunity to comment upon their letter.

The authors suggest that a letter recently circulated among anaesthetists by the Association of Anaesthetists "appears to have been prepared and printed" before the letter calling for unity was sent by the president and faculty deans of the Royal College of Surgeons of England to all fellows. If this is indeed the case, anaesthetists have a right to know the circumstances likely to have produced such a clumsy clash. The dean of the faculty sits by right not only on the council of the association but on all its important subcommittees. To my personal knowledge, since the "college referendum" in 1971/72, when I was honorary secretary, until at least last year successive deans have been closely consulted within the association on all matters relating to "the college issue" and their views accorded, quite rightly in my opinion, a disproportionate degree of weight when important decisions have been reached. On the other hand it is the president of the Royal College of Surgeons, not of the Association of Anaesthetists, who sits by right on faculty board meetings, and one can only assume that this arrangement is the cause of any lack of liaison within our own specialty. Perhaps our new "independent" faculty will find it possible to work as closely with its parent body, the association, as it does with its partners.

Your correspondents also appear to disapprove the association's encouragement to voters to elect to the board of the faculty those known to be in favour of a separate college. I had hardly supposed that my receipt of the college's letter a week or so before the faculty elections was pure coincidence. Indeed, the letter claimed as one of its purposes "to bring the message of the college's unit clearly before those who elect its governing council and boards of faculty"; at worst the association's approach could be considered unsophisticated to the point of being downright straightforward.

The president of the Royal College of Surgeons and the faculty deans have a right to request our loyalty, provided their information is accurate and their intentions are both sincere and clear. In their letter they state that the college council delayed its final request for

the new charter until it was known that the association's referendum had not resulted in an "approach to the college for the creation of an independent college." They fail to record that the referendum analysis did result in an approach by the Association to the College to discuss two suggestions: (1) That anaesthetists should have the same opportunity as surgical fellows to elect representatives, on an equitable numerical basis, directly to their college council. The only objection to this which had ever been raised—namely, that the college council was concerned not only in the college government but also in the supervision of surgical academic affairs, could be easily overcome by the creation of a faculty of surgery. The association recognised that such a change would take time, but felt that if true equality of fellows were ever to be seen to have been achieved a commitment along these lines was required. (2) That should (1) be unacceptable to the college eventual separation seemed inevitable. If this were the case would the college support the creation of an independent College of Anaesthetists within the existing college's physical framework?

I understood that both these proposals met with unequivocal rebuff and on these grounds switched my own allegiance from "a satisfactory long-term solution within the college" to "the inevitability of independence" and the establishment of an academic foundation to test its feasibility. Am I wrong? Can I expect the 10-year experiment to move towards one or other of these aims? Even today's experiments should have some purpose other than to occupy time. It is long since the winds of change blew a bit everywhere. Fearful of too radical an outcome, it is nevertheless time to put just the faintest breeze up Lincoln's Inn Fields.

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Papilloedema

SIR,—Your leading article (4 February, p 263) does well to reiterate that papilloedema from increased intracranial pressure was shown by Parsons¹ to be non-inflammatory. Along with this, and with insufficient emphasis, is the complete lack of interference with vision except in advanced cases, and this is its main differentiating feature from inflammatory papillitis as in multiple sclerosis. Because of this good vision the discussion on axonal transport is largely academic, as blockage of axonal transport is not consistent with continuance of normal function. Such blockage may be all very fashionable and play a big part in papillitis and ischaemic papillopathy. It probably plays some part in advanced papilloedema when vision does fail, but I maintain that it has no place in early papilloedema or in its causation. Clinically, visual loss is only a late effect and not an early one unless from incidental involvement of visual pathways by the tumour.

I had thought from your article that a reference² had been found which preceded my physiological theorising^{3,4} that there would be stasis anoxia of retrolaminar tissue from shunting of blood to the easier prelaminar tissue in states of raised cerebrospinal fluid pressure and that this shunt led to a breakdown of local autoregulative control which aggravated the effects. But no, the reference² related only to ischaemia from increased intraocular pressure,

which is prelaminar and practically the reverse of papilloedema. Much as I deprecate the suggested involvement of axonal transport in plerocephalic papilloedema up to the stage of loss of function, which is what usually pertains, my theory does at least offer a rational explanation for anoxic blockage of flow in advanced cases, and as far as I can see it is only when there is some degree of visual failure that one can get blockage of axonal flow.

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¹ Parsons, J H, *Pathology of the Eye*, vol IV, p 1349. Oxford University Press, 1908.

² Obstbaum, S A, and Podos, S M, *Investigative Ophthalmology*, 1974, **13**, 81.

³ Primrose, J, in *Vision and Circulation*, ed J S Cant, p 29. London, Kimpton, 1976.

⁴ Primrose, J, *Documenta Ophthalmologica*, 1976, **40**, 391.

Eruption during treatment with oxprenolol

SIR,—In the past two months we have seen two patients who have developed a distinctive eruption while receiving treatment with oxprenolol (Trasicor).

The two patients, a 77-year-old woman and a 49-year-old man, developed their rashes after receiving treatment for five months and two months respectively. The lesions consisted in both cases of well-defined red rings, some of which were scaly or eroded, on the trunk, face, and limbs. Psoriasiform features were seen on the lower limbs in the second case. Histological examination revealed lichenoid changes with basal cell liquefaction degeneration, intraepidermal necrotic cells, and colloid body formation.

Within the past two months we have also received biopsy specimens from two further patients developing rashes during oxprenolol treatment which have shown similar histological changes.

A rather similar eruption due to oxprenolol has previously been described;¹ our cases will be more fully reported in the near future. They may reflect increased usage of oxprenolol because of anxiety about the safety of other beta-blockers. A preliminary communication is being made here because we are concerned that this probable side effect should be recognised early, perhaps allowing more serious side effects to be avoided.

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¹ Holt, P J A, and Waddington, E, *British Medical Journal*, 1975, **2**, 539.

Psychological evaluation in cases of self-poisoning

SIR,—Dr R Gardner and his colleagues at Addenbrooke's Hospital, Cambridge (17 December, p 1567) suggest that patients who take a drug overdose deliberately need not be referred routinely to a psychiatrist but that physicians should decide if specialist psychiatric advice is necessary. I should like to present some information which I feel is relevant to their suggestion.

Ambulance personnel and casualty staff in 62 London casualty departments were asked in a questionnaire survey for their recommendations for the management of cases of deliberate self-poisoning. From the 1350 questionnaires distributed, 1248 (92%) were returned, of which 858 were completed by doctors and nurses. Among this group 88% believed that these patients should never be discharged without full psychiatric evaluation, 85% thought that there should be a resident psychiatrist in the hospital to advise patients, and 80% thought that patients with deliberate self-poisoning should always be admitted, at least overnight. Although 73% thought that doctors and nurses should be given additional training to deal with these patients, 57% believed that they should be taken to special emergency centres and not brought to hospital casualty departments at all.

The opinions of these doctors and nurses cannot be lightly dismissed as their experience of all drug-related problems, including that of deliberate self-poisoning, is considerable.¹ In some London casualty departments, for example, there is a daily average of three drug overdose patients compared with 1.3 at Addenbrooke's. It is against this background that the recommendation of Dr Gardner and his colleagues is set; before it is generally adopted the opinions of those who would have to assume greater responsibility for this very large group of patients should be sought most carefully. If, as has been reported,^{2,3} their attitude to deliberate self-poisoning patients is an unsympathetic one the care of these patients might well be affected adversely—particularly if staff in other hospitals are less fortunate and lack the support and instruction of an interested and involved psychiatrist.

In any case, the aim of any amendment to the recommendations of the Hill Report should surely be that the treatment of the patients is improved, not merely shifted from one group of doctors to another.

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¹ Ghodse, A H, *British Journal of Preventive and Social Medicine*, 1976, **30**, 251.

² Patel, A R, *British Medical Journal*, 1975, **2**, 426.

³ Ghodse, A H, *Social Science and Medicine*. In press.

SIR,—Dr M D Enoch and Professor J R M Copeland (4 February, p 300) have touched on the issue of clinical responsibility. When we stated (17 December, p 1567) that "physicians should decide for each of their patients if specialist psychiatric advice is necessary" we were, of course, referring to consultant physicians.

The Department of Health's recommendation¹ does have implications which extend far beyond the management of self-poisoned patients in the general hospital. What if other expert committees were to recommend, say, that in all cases of head injury patients must be clinically evaluated by neurosurgeons, or that all men with chest pain should be referred to cardiologists, or that every patient with abdominal pain must be examined by a general surgeon? Such clinical decisions are usually left to the discretion of the medical practitioner. We decided at Addenbrooke's that physicians must have ultimate clinical responsibility for the self-poisoned patients admitted under their care. It followed that they could overrule psychiatrists' assessments,

and in the clinical trial they did so in five out of 133 instances. Since they requested psychiatric opinions for only 16% of their patients it is unlikely that in other hospitals physicians would require psychiatrists to see every self-poisoned patient.

Sir Denis Hill (11 February, p 362) has done much to improve the whole standard of undergraduate education in psychiatry. But most student psychiatric attachments are for only 8-12 weeks. Is this sufficient training for the future doctor? In general medicine and surgery further training is considered essential during the preregistration year. It is this aspect of medical education which has been neglected by psychiatrists and to which they might contribute with their colleagues in other specialties.

I share Sir Denis Hill's concern lest psychiatrists should now leave physicians to undertake the initial psychiatric assessment of self-poisoned patients without first ensuring that junior doctors and nurses are adequately taught and that both psychiatric treatment and help from social workers are available once such patients have been discharged. What should be taught and how consultation liaison can be achieved might best be discussed at a joint meeting between the Royal Colleges of Physicians and Psychiatrists.

A decade has now elapsed since the Hill report.¹ Is it not time that the standing medical advisory committees reviewed the arrangements for the treatment and after-care of self-poisoned patients and initiated a detailed study of the prevention of poisoning?

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¹ Central and Scottish Health Services Councils, *Hospital Treatment of Acute Poisoning*. London, HMSO, 1968.

Drug treatment of chronic stable angina pectoris

SIR,—Your leading article on the above subject (25 February, p 462) states that studies on prenylamine have generally shown it to be no more effective than placebo. This statement is contrary to the facts and indeed misquotes one of the references.¹ The reference concerned actually states: "Prenylamine (Synadrin—Hoechst) depletes catecholamines in a similar way to reserpine, and acts in angina probably by reducing the sympathetic stimulation to the heart. In several adequately controlled double-blind trials, prenylamine in a dose of 180-300 mg daily was significantly better than placebo in preventing anginal attacks." However, both this paper and another cited² are review articles.

We agree with your statement that "new drug preparations for the treatment of angina invariably have a strong placebo effect, and double-blind trials are essential for their evaluation." Since 1965 there have been a number of double-blind controlled studies published³⁻¹¹ in the world literature definitely reporting the superiority of prenylamine over placebo in reducing the number of attacks and the consumption of short-acting nitrates in patients with angina pectoris. One study¹⁵ reported no difference between prenylamine and placebo in reducing either anginal episodes or nitroglycerin consumption, but even this

investigation, while never reaching statistical significance, shows a trend in favour of prenylamine over placebo.

While there are doubtless criticisms which can be levelled at some of the above studies either on the basis of design or statistical interpretation, the overall weight of evidence demonstrates, in contrast to your leading article, that prenylamine is more effective than placebo as an antianginal agent.

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¹ *Drug and Therapeutics Bulletin*, 1974, **12**, 1.

² Aronow, W S, *American Heart Journal*, 1973, **85**, 132.

³ Kappert, A, *Zeitschrift für Therapie*, 1965, **2**, 82.

⁴ Donat, K, and Schlosser, G A, *Medizinische Klinik*, 1966, **61**, 352.

⁵ Cloarec, M, Proceedings of symposium on "Prenylamine in therapy," Monaco, 1 March 1968, p 52.

⁶ Cardoe, N, *British Journal of Clinical Practice*, 1968, **22**, 299.

⁷ Stoker, J B, *British Journal of Clinical Practice*, 1968, **22**, 384.

⁸ Cardoe, N, *Postgraduate Medical Journal*, 1970, **46**, 708.

⁹ Mikkelsen, E, *Ugeskrift for Laeger*, 1971, **133**, 873.

¹⁰ Sepatia, G C, Jain, S R, and Prakash, J, *Clinical Trials Journal*, 1971, **8**, 43.

¹¹ Winsor, T, et al, *American Heart Journal*, 1971, **82**, 43.

¹² Arora, R, *Indian Heart Journal*, 1973, **25**, 62.

¹³ Tucker, H, et al, *British Heart Journal*, 1974, **36**, 1001.

¹⁴ Kotia, K C, et al, *Medikon*, 1977, **6**, 33.

¹⁵ Bjørum, N, Christensen, M, and Rathach, P, *Ugeskrift for Laeger*, 1967, **129**, 47.

Ectopic pregnancy rates in IUD users

SIR,—In a recent review of ectopic pregnancy and contraceptive use Tatum and Schmidt¹ suggested that "there may be a causal relationship between the use of an intrauterine device (IUD) and the occurrence of an extrauterine pregnancy." This review, in addition, suggested that the Progestasert IUD may carry a higher risk of ectopic pregnancy than other IUDs. In a recent letter (17 December, p 1600) Dr Robert Snowden has also proposed that the Progestasert IUD may carry a selectively greater risk of extrauterine pregnancy.

At a meeting in Geneva from 27 February to 1 March 1978 of the steering committee of the Task Force on Intrauterine Devices for Fertility Regulation of the World Health Organisation's Special Programme of Research, Development and Research Training in Human Reproduction the available published and unpublished data on the occurrence of ectopic pregnancy in IUD users was reviewed, and the committee had the following reservations about the interpretation of these data by the above authors:

(1) It has long been argued on the basis of the mechanism of action that an IUD protects more against intra- than against extrauterine pregnancy. It would therefore be anticipated that there should be a higher ratio of extra- to intrauterine pregnancies in IUD users, a difference in incidence which has in fact been found in many studies. The steering committee believes that the available data indicate a real increase in the risk of ectopic pregnancy among IUD users when compared with non-users. However, the magnitude of this risk is difficult to quantitate with the existing data as there are no studies in which adequate comparisons of risk have been made.

(2) The methods used to present ectopic pregnancies in published studies may give misleading information, especially when ectopic pregnancy rates are presented as a percentage of total pregnancies; such percentages may be affected by the contraception-dependent reduction in intrauterine pregnancy rate. Accordingly ectopic pregnancy rates should be expressed as a