How much easier it would be if platelet stickiness were expressed as the *percentage of sticky platelets* rather than as the percentage of non-sticky platelets, and how much more logical.

That is: Platelet stickiness (%) =

$$\frac{(\text{Original Count minus Second Count}) \times 100}{\text{Original Count}}$$

With this simple transformation a high value would indicate a high degree of stickiness, and the need for mathematical gymnastics from the reader would disappear.

I would have thought that this subject is still sufficiently young to allow this break with tradition in the interests of simplicity.—I am, etc.,

Victoria Infirmary,
Glasgow S.2.

T. B. BEGG.

Specifications for Eye-drops

SIR,—The executive councils' recent circular to doctors and chemists (E.C.N. 563) concerning the new B.P.C. specifications for eye-drops has accomplished two things; it has confused the prescriber and appears to have discriminated strongly against branded products. As the makers of the Alucid brand of sulphacetamide eye-drops, with wide experience in this field, we feel that certain facts should be made clear to all users of these preparations.

The circular states that: "... the brand products of sulphacetamide in their present form do not meet the B.P.C. requirements." This could imply that branded products (including Alucid) fall short of a certain standard of performance or purity. This is particularly ironic in view of the history of Alucid, which has set standards of production and purity which have only gradually been approached over the 18 years during which the B.P.C. have issued six revisions in formulation and sterility specifications in the search for a satisfactory product. How do the brand and B.P.C. preparations compare today? The B.P.C. calls for two tests on the finished product, assay, and sterility testing. Alucid requires seven further tests before it is released for sale.

The difference between the preparations are, and have been, important; for example, Alucid has always been marketed as a sterile preparation, and it is nearly twenty years since filtration was adopted by us as the only satisfactory method of sterilizing a product of this nature. On the other hand, it is only with the recent publication of the 1966 Supplement that the B.P.C. has specially stipulated that eye-drops must be sterile, and recommended filtration as the most suitable method for sterilizing those containing sulphacetamide. Under these circumstances the executive councils' statement falls rather oddly on the ear.

A further instruction to prescribers standardizes the pack size for these preparations at 10 ml. This is quite at variance with most users' preference; our experience over the years, confirmed by up-to-date prescription surveys, shows clearly that $\frac{1}{2}$ oz. (14 ml.) is the size preferred by doctors prescribing either branded or unbranded sulphacetamide. At first sight there might seem to be an economy here, but a comparison between the Drug Tariff's prices and Alucid's shows that it

is at the expense of real value. According to our calculations, 14 ml. of Alucid 30% costs the country a total of 6s. 10d. 10 ml. of 30% sulphacetamide eye-drops B.P.C. costs 6s. 4½d. The "better buy" is obvious.

Are the executive councils acting in the best interests of doctors and patients, or is their main concern to be seen to save pence? If the latter it is a strange saving, at the expense of true value and of the wealth of care and experience expended by the brand manufacturers.

Finally, the situation raises a question of vital importance to prescribers of these preparations. Any formulation changes in branded pharmaceuticals quite rightly necessitate a submission for the consideration and approval of the Dunlop Committee on Drug Safety. Can the Pharmacopoeia Commission assure us that their re-formulations of sulphacetamide eye-drops have been at least as strictly vetted by toxicity testing and clinical trial as similar changes in branded versions would certainly have been? If they cannot do this, doctors will surely continue, in the interests of their professional standards and their patients' well-being, to rely on the reassurance implicit in the known branded products by specifying them on E.C.10.—I am, etc.,

G. R. FRYERS,
Managing Director,
Nicholas Laboratories Ltd.
Slough, Bucks.

Dangerous Patients

SIR,—Your leading article (11 February, p. 317) and the article by the medico-legal correspondent (same issue, p. 373) raise a number of points.

Security hospitals which are half-way houses between the ordinary psychiatric hospital and special hospitals would seem to be necessary; as the Mountbatten report expresses (in double negatives) the real problem of the future as follows: "... there unfortunately seems no reason to think that the number of violent and dangerous prisoners will not continue to increase."¹ In these institutions the troublesome type of prisoner from the ordinary prison could be detained, as well as those dangerous patients sent there by the courts under hospital orders, with restriction orders made without limit of time. But should mentally abnormal persons and psychopaths be housed together in a single institution? The Royal Commission on Capital Punishment² felt that it was not qualified to pronounce on this question.

Your legal correspondent draws attention to the amount of power in the hands of the hospital managers, who may under certain circumstances discharge a patient against the advice of the responsible medical officer. That lay persons can interfere in medical matters was made evident in the Birmingham public inquiry.³ It seems desirable that the powers of the managers should be limited, and that a legally qualified chairman should always be appointed.

Regarding the young man of 23 who was expelled by a committee of doctors and patients from a psychiatric hospital, surely the criticism here should not be directed against the psychiatrist concerned but rather at the Court of Criminal Appeal, who made a three-year probation order with a condition

that the man entered the hospital in question as an informal patient.—I am, etc.,

Southampton.

B. L. M. TURNER.

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Pulmonary Artery Thrombosis and the Nephrotic Syndrome

SIR,—In discussing the aetiology of the pulmonary artery thrombosis in their patient with nephrotic syndrome, Dr. S. E. Levin and colleagues (21 January, p. 153) point out that alpha-2 globulins, which are usually increased in the serum of patients with the nephrotic syndrome, have been shown to possess antifibrinolytic activity,¹ and also Dr. S. G. Cotton (18 February, p. 426) has shown in four patients with nephrotic syndrome a great increase in inhibitory effect on fibrinolysis by their plasma compared with age- and sex-matched controls. Macleod *et al.* have shown that there is a defect in the fibrinolytic enzyme system of lung tissue of patients who died with renal failure and postulate that this defect may be important in the formation of the excessive fibrinous exudate commonly seen in renal failure.

In a study² in which we are now engaged, consisting of 17 subjects without renal disease, we have observed that renal vein blood has increased fibrinolytic activity compared with arterial blood when estimated by the euglobulin lysis method as described by von Kaulla³ with slight modification.⁴ These findings suggest that kidney parenchyma produces fibrinolytic activators which are passed on to the circulation. It is only reasonable to assume that when kidney is diseased the activator production by the parenchyma is impaired, which may be a contributory factor in the causation of deranged fibrinolytic activity in the nephrotic syndrome.—I am, etc.,

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Royal Victoria Infirmary,
Newcastle upon Tyne.

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- ⁴ Von Kaulla, K. N., *Chemistry of Thrombolysis: Human Fibrinolytic Enzymes*, 1963, p. 79. Springfield, Illinois.
- ⁵ Menon, I. S., *Lancet*, 1967, 1, 116.

Enteroviruses and Congenital Abnormalities

SIR,—The Medical Officer of Health for Blackburn has suggested that an apparent increase in the number of congenital abnormalities in his area may be linked with the use of Sabin polio vaccine in the town during August, September, and October 1965. The inference is that the foetus may have been affected by the live virus during the first trimester of pregnancy. We carried out a small investigation which is of some relevance and is therefore reported.

In 1965 routine virus studies in the Blackburn group of hospitals showed that not only polio virus, both wild and vaccine strains, were isolated but also the following enteroviruses: Coxsackie types A5, A9, B3, B4, and B5. E.C.H.O. types 3, 6, 7, 8, 15, and 22.

Most of these cases occurred during July, August, and September.

Infants infected with rubella in utero harbour the virus after they are born, and it can be isolated and even infect other susceptible individuals. We felt it worth while, therefore, to see if we could culture any enteroviruses from foetuses, stillbirths, and neonatal deaths where the first trimester of pregnancy coincided in whole or in part with the enterovirus outbreak. This was done in 23 cases and selection was random. In one case faeces only of a child who survived with an abnormality were examined. Investigation was not confined to infants who were congenitally abnormal.

Brain, lungs, faeces, and placenta were examined by conventional methods, being inoculated into monkey-kidney, human-amnion, and Hela-cell cultures. The foetal tissues and placentas were examined for histological changes.

All results were negative except for one case. This was a full-term stillborn infant who had shown foetal distress. E.C.H.O. virus type 3 was isolated but the histology was negative. The mother's blood was examined in November 1966 and no E.C.H.O. antibodies were demonstrated. The significance of this isolation is therefore in doubt.—We are, etc.,

P. D. MOSS.

C. K. HEFFERNAN.

J. G. THURSTON.

Blackburn Group of Hospitals,
Blackburn.

L. ROBERTSON.

Public Health Laboratories,
Preston.

Prevention of Pressure Sores

SIR,—Everyone who is interested in the adequate care of both geriatric and younger long-term patients must have read with interest the excellent article on the large-cell ripple mattress (18 February, p. 394). I am sure the findings will be endorsed by all who use this type of mattress.

The writers mention in passing the Siere air-filled mattress.¹ This is very much cheaper and has not the inconvenience of needing a motor. All readers may like to know about it, because hospitals are often prevented by cost from having as many ripple beds as they would like to have, excellent as these undoubtedly are. In my small hospital unit for the young chronic sick we use these for all bedridden patients, and find it of equal value in both preventing and healing pressure sores, though because it does not reach the heels normal precautions have to be taken to avoid pressure on these areas. The use of this mattress eliminates the frequent turning of patients, which is trying for any patient who wants to carry on the normal activities of daily living from his bed, and is exhausting and time-consuming for the staff.

The principle on which this bed works is that it has two long air cells connected by a tubular collar which lies under the pillow, and a long cell on each side. These cells

are inflated, and when the patient is lying on the mattress the sacral area is almost completely relieved of pressure, and on movement of the patient there is a flow of air from one part of the bed to the other, which results in an equalizing of pressure over the whole body surface.—I am, etc.,

Guy's Hospital,
Dunoran,
Bickley, Kent.

BARBARA MORTON.

REFERENCE

- ¹ *Mth. Bull. Minist. Hlth Lab. Serv.*, 1966, 25, 239.

Proflavine and Acriflavine

SIR,—Professor E. Boyland¹ in an interesting recent lecture on "A Chemist's View of Cancer Prevention," speaking of the carcinogen acridine orange, mentions that it is a derivative of proflavine, and that the latter "does not seem to have been adequately tested for carcinogenic activity." In the case of a compound which has been so long and extensively used as a surface antiseptic for wounds such a statement might well cause serious misgivings. However, I was able to say in 1937,² "I am indebted to Professor Kennaway, of the Research Institute of the Royal Cancer Hospital (Free), London, for unpublished observations which indicate that neither acriflavine nor proflavine, when painted on the skin of mice over a long period, has carcinogenic action." I have not met a suggestion that Kennaway's work was ever inadequate, and so conclude that this reference was overlooked. But if Professor Boyland meant that additional laboratory procedures should have been employed, it must be added that over 50 years of almost world-wide clinical use without a sign of carcinogenicity, in addition to Kennaway's negative results, constitute a stupendous record. This affords practical proof that neither proflavine nor acriflavine, as applied therapeutically to wounds, is carcinogenic in man. Anticarcinogenic properties have even been ascribed to some acridine compounds.³ Finally, it must be emphasized that so far generalizations on pathogenic properties based purely on chemical genealogies require to be made with the utmost reserve.—I am, etc.,

Glasgow.

C. H. BROWNING.

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- ² Browning, C. H., *Edin. med. J.*, 1937, 44, 497.
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Percutaneous Cholangiography

SIR,—Percutaneous cholangiography has become an accepted method of investigating obstructive jaundice. I have been using this method intermittently during the past three years, and after initial enthusiasm following a few very successful attempts I became somewhat discouraged. I had many failures, especially in jaundice of fairly recent origin. The fault may well have lain in my technique, but I should be interested to know whether other workers have also found that this method can only be relied upon in cases of long-standing jaundice where the diagnosis is probably known already.—I am, etc.,

Ashford Hospital,
Ashford, Middx.

ROBIN BURKITT.

Simultaneous Tubal and Uterine Pregnancy

SIR,—I should like to report a case of simultaneous tubal and uterine pregnancy with continuation of the pregnancy to term.

The patient was a 36-year-old African, who was admitted to hospital on 9 February 1966 complaining of intermittent pain in the right side of the abdomen. She had a history of two months' amenorrhoea, and a previous tragic obstetrical history—she had had nine children all of whom died in infancy, except the youngest, who was aged 10 years.

On examination she was thin and ill-nourished (haemoglobin 60%). There was colostrum in her breasts. Abdominal palpation revealed a mass in the right iliac fossa which appeared to be arising out of the pelvis. Pelvic examination revealed a soft uterus and cervix, with a slightly tender mass in the right fornix, but no uterine bleeding. The diagnosis suggested ectopic pregnancy or uterine pregnancy with an ovarian cyst.

At laparotomy there was about 1 litre of fresh blood in the peritoneal cavity and a ruptured tubal pregnancy at the fimbriated end of the tube. Right salpingectomy was performed. The uterus was the size of a 10-week pregnancy.

She made satisfactory postoperative progress and was discharged home on the 16th day with a haemoglobin of 68%. At the follow-up clinic four weeks later her uterus had increased in size, and on 18 August 1966 she had a spontaneous vertex delivery of a male infant weighing 6 lb. 14 oz. (3 kg.) and the puerperium was uneventful.

It was not noted at laparotomy which ovary contained corpus luteum, but since neither ovary was removed the progestational hormone was adequate to maintain the uterine pregnancy.

Novak¹ reported 276 cases of combined intrauterine and extrauterine pregnancies, yet Jeffcoate² says, in discussing ectopic pregnancy, that simultaneous intrauterine and extrauterine pregnancies are rare. Reid³ estimates they occur once in the course of several thousand pregnancies. With our patient, who had such a bad obstetrical history, the satisfactory outcome of her uterine pregnancy was very gratifying.—I am, etc.,

C. BRIGID CORRIGAN.

Makiungu Hospital,
Singida, Tanzania.

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Imported Tropical Diseases

SIR,—Your leading article (25 February, p. 450) gives the names of three special centres dealing with tropical diseases in Great Britain, but omits to mention the Dreadnought Seamen's Hospital at Greenwich.

May I venture to remind you that it was from the seamen's hospitals that Sir Patrick Manson and Joseph Chamberlain developed the first hospital for tropical diseases and the first school of clinical tropical medicine in London? The great names of Manson, Low, and Manson-Bahr may not be as closely associated with the seamen's hospitals as they were 20 years ago, but cases of tropical disease continue to appear in our wards, and a unique feature of the Dreadnought Hospital is its traditional familiarity with the inhabi-