

roduction of modern, malleable fibre-optic endoscopes has been accompanied by a fall in the incidence of oesophageal injuries. However, even the most skilled endoscopist using the latest apparatus must still be ever awake to the risk of oesophageal damage."

Smith and Tanner's<sup>1</sup> figure of one perforation in every 300 gastroscopies and one perforation in every 100 oesophagoscopies refers to the use of rigid instruments; Ariga<sup>2</sup> collected information on 123,900 gastroscopies. The perforation rate with rigid instruments was 0.065% and with early fibre-scopes 0.03%. Its apparent safety is contributing to the rapid increase in the number of fibre-endoscopies being performed in patients of all ages by endoscopists of varied experience. Instruments are becoming more complex and investigators more ambitious. It is therefore important to obtain accurate and up-to-date knowledge of the incidence of all the complications of modern fibre-endoscopy. We hope that this information will shortly be available from a survey being undertaken among members of the British Society for Digestive Endoscopy.—We are, etc.,

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- <sup>1</sup> Smith, C. C. K., and Tanner, N. C., *British Journal of Surgery*, 1956, 43, 396.  
<sup>2</sup> Ariga, K., *Gastroenterological Endoscopy (Japan)*, 1966, 8, 7.

#### Haemophilus Epiglottitis

SIR,—I read with interest the experience of Dr. Patricia E. Mortimer and others (26 February, p. 567) in their management of patients with haemophilus epiglottitis. However, their statement suggesting "endotracheal intubation as a method of maintaining the airway is undesirable if it does not completely relieve the obstruction in a patient likely to develop cardiac arrest" and concluding that tracheostomy is easier to manage, infers that tracheostomy will relieve the obstruction if intubation does not do so adequately. If intubation cannot relieve obstruction of the upper airway, neither will tracheostomy. As has been pointed out, in haemophilus epiglottitis the obstruction is supraglottic, and Dr. Mortimer describes in one of her cases how intubation through the cords was easily possible at postmortem. If airway obstruction to some degree is still present after the use of an artificial airway, it suggests that there is co-existing peripheral obstruction. Moreover, bilateral pneumothorax complicated tracheostomy in one of the described cases, and it should be realized that this can arise in one of two ways.<sup>1</sup> Either violent inspiratory efforts suck air through the cervical incision into the mediastinum with rupture through the mediastinal pleura into one or both pleural cavities; or expiratory obstruction leads to alveolar rupture with tracking of air along the bronchovascular planes into the mediastinum and subcutaneous tissues, and possibly again into the pleural cavities after rupture of the mediastinal pleura.

I would reinforce the urgent treatment of the septicaemia by intravenous chloramphenicol, and emphasize that the condition is eminently suitable for endotracheal intubation as has already been written.<sup>2,3</sup> Tracheostomy is *not* the easy operation in

infancy that some appear to believe; it has a morbidity and mortality rate, and is fraught with much danger if carried out as an "emergency" procedure.—I am, etc.,

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- <sup>1</sup> Reading, P., *Journal of Laryngology and Otology*, 1958, 72, 785.  
<sup>2</sup> Newlands, W. J., *British Medical Journal*, 1972, 1, 374.  
<sup>3</sup> Edwards, J. M., and Roberts, K. D., *British Medical Journal*, 1972, 1, 246.

#### Red Cell Size and Age

SIR,—We read with interest Dr. T. Okuno's report (26 February, p. 569) of an increase in mean red cell volume with age in normal subjects, and his suggestion that the normal range of values for the M.C.V. should be revised from the currently accepted 76-96 $\mu^3$  or 82-92 $\mu^3$ .

In 1970, using a Coulter A cell counter and a microhaematocrit centrifuge, we reported similar findings in mentally retarded patients.<sup>1</sup> In addition, we demonstrated the increase in the mean red cell volume with increasing age in both mentally retarded epileptic patients treated with anticonvulsants and in patients suffering from Down's syndrome, these latter patients having been previously shown to have macrocytosis.<sup>2</sup> All these patients had haemoglobin levels within the normal range for their age and sex. In the mentally retarded epileptic patients treated with anticonvulsants we were also able to show that the degree of macrocytosis was related to the dose of phenobarbitone,<sup>3</sup> and that correction of folate deficiency, as measured by red cell folate concentration,<sup>4</sup> did not reduce this macrocytosis significantly.<sup>5</sup>

With the advent of instruments such as the Coulter S, research into red cell size could now progress rapidly.—We are, etc.,

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- <sup>1</sup> Eastham, R. D., and Jancar, J., *Lancet*, 1970, 1, 896.  
<sup>2</sup> Eastham, R. D., and Jancar, J., *Journal of Clinical Pathology*, 1970, 23, 296.  
<sup>3</sup> Eastham, R. D., and Jancar, J., *Epilepsia*, 1970, 11, 275.  
<sup>4</sup> Eastham, R. D., and Jancar, J., *British Medical Journal*, 1971, 4, 361.  
<sup>5</sup> Eastham, R. D., and Jancar, J., *Journal of Clinical Pathology*, 1971, 24, 767.

#### Prevention of Deep Vein Thrombosis

SIR,—We were interested to read the comments of Dr. V. C. Roberts and his colleagues (4 March, p. 628) on the results of our trial of intermittent pneumatic compression in the prevention of deep vein thrombosis. We feel, however, that some of their remarks cannot be allowed to go unchallenged.

They suggest that we have confirmed their findings, but we would remind them that a preliminary report of our trial was presented to the meeting of the Surgical Research Society at Birmingham in July, 1971, and subsequently published.<sup>1</sup> Since this was the first reported series using intermittent pneumatic compression in the prevention of postoperative deep vein thrombosis, both the trial reported by Dr.

Roberts and his colleagues and our second report in fact confirm our original observations.

To compare the results of both trials overall (their 82% reduction against our 60%) is quite invalid for several reasons. The method of selection of patients in the two trials was not the same; in our series the patients were entered consecutively, stratified, and then randomly allocated to either group, whereas in Dr. Roberts's series the patients were "randomly selected", and it is not clear what this means. The assessment of the incidence of thrombosis was also different. Scintillation counts of the legs were measured for at least seven days in our series, whereas counting was performed for only three days in theirs. Our results therefore include all thrombi developing during that period, surely an essential assessment if we are to develop an effective prophylactic regimen.

We would question the use of the term "significant reduction" in respect to their series of 8 patients with malignant disease. Using an exact two-tailed test<sup>2</sup> the figures are not significant even at the 10% level of confidence.

It is not possible to claim therefore that one device is superior to the other in the prevention of postoperative deep vein thrombosis, since the evidence needed to compare the two is not available.—We are, etc.,

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- <sup>1</sup> Hills, N. H., Pflug, J. J., Jeyasingh, K., and Calnan, J. S., *British Journal of Surgery*, 1971, 58, 855.  
<sup>2</sup> Finney, D. J. *Bimetrika*, 1948, 35, 145.

#### Multiple Crashes on Motor Ways

SIR,—Without wishing to impugn one section or other of the community in this now all too frequent and alarming situation, might not the aggressive behaviour of the odd irresponsible driver, who apparently triggers off the tragedy, be related to blood sugar levels? Carrying this point further, has the incidence of diabetics taking insulin been higher in such accidents than that of the non-diabetic driver?

It is a well known fact that many diabetics prior to an insulin coma become irritable, aggressive, and irresponsible. I submit this suggestion in all humility, as it occurred to me following a recent episode under rather similar conditions with a patient who happily was not driving his motor car.—I am, etc.,

J. A. FRAIS

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#### Cardiac Arrhythmias during Laparoscopy

SIR,—I should like to thank your correspondents, for their interest in the article by Dr. D. G. Julian, who is at present in Australia, and myself (12 February, p. 411). I note that they all prefer to use controlled respiration for laparoscopy, and doubtless this serves their own situation very well. For the performance of laparoscopies in this hospital, however, we could consider this

a very large anaesthetic hammer for a relatively small surgical nut. As pointed out in our Table, the length of time from the start of abdominal insufflation to deflation is less than 10 minutes on average. I understand that in some centres the operation takes rather longer and different problems then arise for the anaesthetist.

Laparoscopy we consider to be different from any other minor operation only in so far as it requires some abdominal relaxation (to allow an adequate amount of gas to be introduced into the abdomen before the trocar and cannula are inserted) and quiet respiration. The latter is of some importance as excessive movement of the intestines with respiration (which can also occur if controlled respiration is too violent) can be dangerous during the tubal diathermy. (I know of one case in which the bowel was burnt in this way). The main advantage of nitrous oxide over carbon dioxide is the elimination of excessive respiratory drive.

There should be no morbidity associated with anaesthesia for the procedure. Vomiting, as we said, is no commoner than with other minor gynaecological operations. The cardiac arrhythmias seen are innocuous, but can largely be eliminated again by using nitrous oxide for insufflation. The use of small doses of gallamine has very little effect on respiratory performance as judged by  $\text{PaCO}_2$  levels with and without the drug. Like Dr. T. Sayer (26 February, p. 566) we do not use halothane if termination of pregnancy is also being carried out.

Dr. Nanette Gordon and colleagues (4 March, p. 625) quote a case from the literature in whom the  $\text{PaO}_2$  during laparoscopy was 46 mm Hg, but do not mention that the same patient had a  $\text{PaO}_2$  of only 50 mm Hg before pneumoperitoneum.

I can reassure Mr. P. C. Steptoe and Dr. F. N. Campbell (4 March, p. 625) that we do use a pressure-limiting device, having been convinced by him personally of its necessity before introducing the operation into our practice. The intra-abdominal pressure (properly measured) seldom exceeds 15-20 cm of water.

After the very large number of cases performed in this hospital, I am still unable to understand Dr. J. E. Utting's description of our method as "entirely inappropriate" (26 February, p. 566). Have your correspondents ever considered the side effects of their methods? While I am sure that they are minimal in their hands, we have found that heavy premedication can cause delayed recovery and, if opiates are used, a high incidence of nausea and vomiting postoperatively; muscular relaxation with suxamethonium causes muscle pains (often severe in these early ambulant cases); and intubation causes sore throats. Modern anaesthesia has contributed many advantages but nothing in life is free.—I am, etc.,

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#### Mode of Action of Verapamil in Man

SIR,—We read with interest the report of Dr. L. Schamroth and others (11 March, p. 660) which confirmed our observation made in 1969 and recently reported (9 October 1971, p. 113) that verapamil is a

drug of promise in the treatment in man of dysrhythmias arising in the specialized conducting tissue of the heart. However, in the treatment of atrial fibrillation the mechanism of the regularizing effect of verapamil is not as uncertain as Dr. Schamroth and his colleagues suggest. Conduction along the atrioventricular specialized conducting tissue is under vagal control and augmentation of this activity will prolong the refractory period. The phenomenon of delay in conduction along the atrioventricular specialized conducting tissue was first demonstrated by Trendelenberg,<sup>1</sup> and can present a problem when fast atrial pacing is used to achieve high heart rates with 1:1 specialized conducting tissue conduction. We have observed (unpublished) that intravenous atropine (0.6—1.2 mg) can overcome this Trendelenberg effect. In addition, we have noted that the effect of verapamil on slowing the ventricular rate in patients with atrial fibrillation can be reversed by atropine. We would therefore repeat our suggestion that an important action of verapamil is to augment the effects of vagal tone on the specialized conducting tissue of the heart.—We are, etc.,

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<sup>1</sup> Trendelenberg, W, *Archiv für Anatomie und Physiologie*, 1903, p. 271.

#### Trimethoprim Resistance determined by R Factors

SIR,—In our paper "Trimethoprim Resistance Determined by R Factors" (Mr. M. P. Fleming and others, 18 March, p. 726) I failed to make it clear that the routine bacteriology in the U.C.H. Group is undertaken in two separate laboratories. The methods used for antibiotic sensitivity testing in the two laboratories are similar, but the patients from whom specimens derive are not. Thus my department handles, for instance, material from a large geriatric department and from general practice, while the other laboratory does not. As a result, the patterns of antibiotic resistances observed are not always alike. For instance, in my laboratory one third of urinary *Klebsiella* strains are found to be resistant to trimethoprim whereas in Dr. E. Joan Stokes's department the figure is lower (3 out of 41 strains in the last 3 months).

I apologize for any misunderstanding which might have arisen over this.—I am, etc.,

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#### "Asthma and a Lump in the Breast"

SIR,—In the article "Second Opinion, Please: Asthma and a Lump in the Breast" (11 March, p. 681) it was assumed that the asthma was precipitated by the psyche. It was by no means established that the asthma was not due to allergy to the budgerigar. In favour of such a diagnosis would be the period of exposure to the budgerigar; that, for what it is worth, skin testing showed sensitivity only to feathers; and that the bird was looked after in the house of a friend while she had her mastectomy and may well not have been returned to the house when seen by the health visitor.

The asthma cannot be blamed on the psyche until it has been established that the return of the bird does not precipitate an attack.—I am, etc.,

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SIR,—It is disappointing to read an article (11 March, p. 681) such as this and find that no assessment is made of respiratory function. The patient had haemoglobin, E.S.R., and urine examination; Bencard skin test, x-ray, and E.C.G., and it was suggested that she be treated with steroids. At no time was even as simple a respiratory measurement as the peak expiratory flow rate made, although this is possibly the most relevant investigation. In the absence of this information it is very difficult to assess the success of the response to her plea for "someone to look at her body".—I am, etc.,

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#### Births and Deaths

SIR,—Your note on the number of births and deaths in England and Wales in 1970 and 1971 (18 March, p. 758) includes a most misleading sentence, "The net result was an increase of 6,000 in the population from these changes". This should have read, "The net result was an increase of 6,000 in the population *growth* from these changes." In fact the excess of births over deaths in 1970 was 209,292, and in 1971 215,681, which produces a net increase in the population of 424,973 over the two years.

At a time when the growth of population is causing concern, it is important that the facts should be made completely clear.—I am, etc.,

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#### Record Folder for General Practice

SIR,—Dr. Gillian Strube (19 February, p. 513) discusses the advantages of the proposed use of the A4 sized folder in general practice. I was recently awarded an Upjohn travelling fellowship by the Royal College of General Practitioners to study records in general practice. Briefly, I had 800 A5 size double-pocket wallets made and these were tried by a large number of general practitioners. My conclusions were that a new form of record system in general practice was essential. The old medical record envelope has had good service for over half a century, and general practitioners are well aware of the inadequacies of this record, which is of no accepted paper size.

The new international paper size has been introduced into Britain and is here to stay. Hardly any general practitioners I have met have any idea what this new paper size revolution really means. It is a most logical system and has already been accepted by industry and most hospitals. The A4 size is slightly larger than the traditional foolscap sheet of paper. The A5 size is exactly half this size.

Most doctors are agreed that some change in the medical record system of general practice is inevitable. Our problem is which size should be adopted, the A4 or the A5.