evidence evisceration is associated with too low a ratio of suture length (SL) to wound length (WL). The risk of evisceration (and ventral hernia) rises increasingly rapidly as the ratio SL : WL falls from 2 : 1 to 1 : 1 (or lower). On theoretical grounds closure with a simple continuous stitch using a stitch interval of 1 cm and a ratio SL : WL between 4 : 1 and 6 : 1 gives conditions in the wound so that the effect of 30% wound lengthening leads to a rise in tension of less than 2% between the sutures and the tissues. Further, I have shown clinically that if the sutures are unabsorbable and monofilament, then sepsis in the wound or deep in the abdomen causes minimal risk of evisceration or ventral hernia.

It is not difficult and very little time is required to include with the operation notes the ratio SL : WL used in each fascial layer. Some surgeons fear that a large amount of non-absorbable sutures in the wound will lead to foreign body complications. I have a paper ready for publication showing that in the repair of ventral (incisional) hernia, having excised all scar tissue, closure of the fascial layers anatomically, using loop nylon, metric 3, at a ratio SL : WL of as much as 25 : 1 leads to sound wound healing without foreign body reaction and without recurrence of ventral hernia.

It now remains for other surgeons to apply measurement to the other methods of fascial closure in the interrupted stitch pattern, the continuous mattress stitch, and the Smead Jones (massive suture) stitch techniques. As a result of this scientific check on the art of wound closure our profession should be able to ensure that virtually every wound includes a margin of safety adequate to withstand all predictable postoperative stresses, including that of deep abdominal sepsis.

T P N JENKINS

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¹ Jenkins, T P N, *British Journal of Surgery*, 1976, **63**, 873.

SIR,—Burst abdominal wounds are preventable. They are the result of poor surgical technique. I was pleased that your leading article (26 February, p 534) referred to the paper by Mr Terry Jenkins,¹ which should be read by all practitioners of surgery. He has now brought science to the art he has been practising and teaching for so many years—namely, taking big bites and suturing loosely.

I was fortunate to be his registrar and have used his technique of abdominal closure for the past eight years, during which time only one of my wounds has dehiscid. This occurred in 1971, nine hours after resection of a carcinoma of the duodenojejunal flexure through an upper midline incision. Exploration of the wound showed that the nylon knot had undone—a technical fault of my knotting, not of the suturing technique.

During the past three years I have periodically measured the length of wounds and of the nylon used and, like Mr Jenkins, have found that the nylon length has a mean of four times that of the incision length. Further evidence of the validity of this concept comes from three burst wounds I have repaired recently for other people. In each case the nylon had torn out of the tissue, and in two cases the length of nylon was less than the length of the wound.

Other advantages of suturing wounds according to the Jenkins's principles are that

they appear more comfortable for the patient; pus can drain freely; the incidence of incisional herniae appears very low; and tension sutures with their many disadvantages are unnecessary.

I recommend this technique of closing wounds to all surgeons.

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¹ Jenkins, T P N, *British Journal of Surgery*, 1976, **63**, 873.

SIR,—I think you could have omitted the question mark from the title of your otherwise admirable leading article on burst abdomen (26 February, p 534).

There are as many ways of closing the abdomen as there are surgeons doing it, but the incidence of such a calamity as you mention and the related complication, incisional hernia, can be reduced below the alarming levels you quote.

Unabsorbable material, even monofilament polyethylene, is liable to leave a foreign body sinus in a percentage of infected cases. Any suture material should, as you say, retain its tensile strength a bit more than long enough for the relatively avascular linea alba or rectus sheath to heal strongly and should then entirely disappear. In the 10 years since polyglycolic acid sutures have been available, allowing easy and accurate apposition of the layers of the abdominal wall (the relatively avascular layer being protected with a lattice stitch for added security) the incidence of burst abdomen, incisional hernia, and wound sinus has, if not "virtually disappeared," at least been considerably reduced in this unit.

It is these iatrogenic complications which increase morbidity and mortality and consume time and money. With your encouragement we shall continue to strive to make them all real rarities.

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Training the trainers

SIR,—Recent *BMJ* articles and correspondence show the deep divisions between those who enthusiastically espouse quite sophisticated courses and methods designed to develop training skills in general practitioners and those who are sincerely sceptical—usually as a result of some personal experience. Between the holders of these polar beliefs and attitudes lie many trainers whose discriminatory skill has enabled them not just to sift the educational wheat from the chaff but also to recognise that learning to become a general practitioner depends on a judicious blend of different techniques—some very old, some recent. Direct instruction (you should see the secretary about these forms), apprenticeship (you've watched me, now try it yourself), self-teaching (you'll learn that best by seeing patients, or reading, or listening to cassettes), shared problem-solving (what is your own opinion about this problem?), sensitivity training (making contact in a group with your own feelings and relationships), and other instruments can all contribute to trainee learning provided the trainer feels at ease with them and is conscious of when he should use them.

But to deluge potential or actual trainers with jargon-loaded educational theory the

application of which to general practice training has been inadequately tested is both discreditable and dangerous. Overdosage with such teaching leads either to a false misunderstood mystique or to near-total rejection of the need to learn to train. As a medical teacher I am just as dismayed by the wrong but sometimes understandable resort to nostalgic empiricism (you can only learn by apprentice-type experience like we did) as I am by the evasive resort to reflected questions (why did you respond to my question about how you felt about this patient by asking me why I asked you that question?) or by the arid insistence on excessive or unevaluable objectives (the trainee shall be able to keep records—records of what, in what form, and how much?). To perpetuate these opposing standpoints by intransigent statements serves neither trainee nor trainer well.

Until accurate, valid, credible, and useful tests of training skills are available—and in my view we are a long way from such consummation—surely the best way forward is a continued reliance mainly on traditional on-the-job learning by trainees, with day release courses for some theoretical teaching but accompanied by a programme of evaluation of different techniques under more controlled conditions than has obtained hitherto. The diversity of training policies and practices must now be quite sufficient to allow measurement of trainee learning to tell us how effective various patterns are. Such experiments of opportunity are badly needed if the way ahead is to become respectable and acceptable.

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Metabolic and cardiotoxic effects of salbutamol

SIR,—We have read with interest the recent article relating to the metabolic effects of salbutamol by Mr A Neville and others (12 February, p 413) and the comparison of salbutamol with aminophylline in severe asthma by Professor D Femi-Pearse and others (19 February, p 491).

Studies in stable asthmatics and in normal subjects have demonstrated metabolic changes, including a rise in plasma nonesterified fatty acid (NEFA) concentration following salbutamol infusion. However, in a study of 63 asthmatic patients admitted with an acute attack pretreatment NEFA concentration was greatly elevated, and no further rise was measured during a subsequent salbutamol infusion.¹ The infusion (10 µg/min) was associated with an increase in plasma glucose and insulin and a fall in plasma potassium concentrations similar to those in stable asthmatics and normal subjects.^{2,3} Thus the stress of acute asthma may in itself cause an increase in NEFA concentration and the response to salbutamol may not necessarily be as hazardous as claimed.

Professor Femi-Pearse and his colleagues state that aminophylline may be cardiotoxic and responsible for sudden death if administered rapidly. Intravenous salbutamol (200 µg) given in one minute caused a rise in pulse rate of 13.4 beats/min when measured five minutes later. They do not report the pulse rate before administration, nor did they measure the pulse rate at a shorter interval after administration. Fitchett *et al*⁴ have

reported a rise of 22 beats/min after a similar dose in acute asthma, while we have shown a rise of 28 beats/min one minute after salbutamol 200 µg injected over a one-minute period to convalescent asthmatics.⁵ Indeed, Mr Neville and his colleagues reported a rise of 31 beats/min on completion of their intravenous injection. It would seem likely that had Professor Femi-Pearse measured the pulse rate sooner after completion of the bolus injection of salbutamol a greater tachycardia would have been apparent. In view of these other reports we feel that salbutamol should be administered intravenously *slowly* (over at least five minutes) and that, with time, cardiotoxic effects or deaths will be observed if this effective bronchodilator is given rapidly as suggested.

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¹ Tickner, T R, *et al*, *Thorax*, 1976, 2, 240.

² Taylor, M W, *et al*, *British Medical Journal*, 1976, 1, 22.

³ Goldberg, R, *et al*, *Postgraduate Medical Journal*, 1975, 51, 53.

⁴ Fitchett, D H, McNichol, M W, and Riordan, J F, *British Medical Journal*, 1975, 1, 53.

⁵ Spiro, S G, *et al*, *British Journal of Clinical Pharmacology*, 1975, 2, 495.

Obstetric flying squads and mobile resuscitation units

SIR,—You published a letter from us last year (13 March 1976, p 650) in which we supported the use of mobile resuscitation units (MRUs) for the early management of patients with myocardial infarction. We went on, however, to stress our conviction that measures for resuscitation and immediate care are similar irrespective of the nature of the emergency. We suggested that there was no need to restrict the use of MRUs to one type of emergency and that immediate care teams could provide skilled aid to patients whether for a myocardial infarction, road accident, or some other medical emergency.

We were therefore especially interested to read two recent articles on obstetric flying squads. While Mr I L C Fergusson and Miss J M Watson (21 February, 1976, p 446) concluded that "few if any circumstances in modern obstetric practice merit continuing the flying squad in the urban area," they reported that in three of the 25 calls in their series the patients were clinically shocked. Dr D K James (22 January 1977, p 217) reported a series of 81 calls, of which 36 were made to general practitioner units and 45 to patients' homes. He concluded that the service was still of great value and "represents a much safer method [than an emergency ambulance call] of transporting an obstetric patient in an emergency" (our italics). It was notable that in all the calls to patients' homes the patients were brought back to the base hospital after being treated by intravenous infusion with or without sedation.

We would like to reiterate our belief that all medical emergencies can be effectively handled in the first instance by a general purpose MRU. Immediate care teams who staff these MRUs can be trained in basic methods of resuscitation and such teams would be effective in handling obstetric emergencies since the objective is the same—safe transport to hospital. Obstetric help to general practitioner units would seem to be in a somewhat different

category since such units should be adequately equipped and only require specialist staff. The basis of an MRU is that it can convey both skilled help and equipment to the seriously and suddenly ill from whatever cause. Only by adhering to this principle will the widespread development of MRUs become economically possible.

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SIR,—While we do not wish to be critical of Dr D K James's excellent article (22 January, p 217) we do feel that some different conclusions may be drawn from his figures. We do agree with him that in assessing the value of a flying squad service certain facts concerning time and manpower are quantifiable but personal judgments are not. In assessing the whole future of the flying squad service we feel that it is these judgments that need reconsideration and that they should not be too greatly preconditioned by old obstetric ideology.

With reference to his figures for the West Berkshire area, we would accept that in general terms the calls to the GP maternity units were justifiable on the grounds that 40% of these patients subsequently required transfer to hospital and that 60% actually had an obstetric procedure carried out in the unit. In this sense time and hospital accommodation were saved in these cases. However, we would take issue as to the efficacy of the flying squad when called to the patient's home. All these patients required subsequent transfer to hospital and no obstetric procedures were carried out in the home. Indeed, the only treatment given before arrival at hospital was an intravenous infusion for some patients. It is clear to us that in view of the times quoted in Dr James's article these patients could have received their infusion earlier had they been taken directly to hospital by the ambulance service. The question that remains to be answered is whether any of them benefited by being assessed before transfer. Here the facts seem to speak for themselves in that, since transport was inevitable in every case, any assessment would be confined to whether an infusion was required, and since an infusion would have been effected more quickly had the patient gone straight to hospital we can only conclude that the expense and manpower involved in supplying a flying squad are not justified in calls to the patient's home.

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Allergy to diazepam—or vehicle?

SIR,—Dr Louis Milner's report (15 January, p 144) concerning a case of allergy to diazepam describes a local reaction in the buttock after an injection of Stesolid 10 mg. Diazepam is very slightly soluble in water and all injection formulations of this drug contain solubilisers such as propylene glycol, phenylcarbinol, or macrogoliricinoleas, which in their turn are known to cause allergic side effects.

In an investigation¹ we have used a new type of vehicle for diazepam. The drug is dissolved in soybean oil, which is then

emulsified in water in the same way as is used for the intravenous administration of fat for parenteral nutrition (Intralipid). About 1500 patients have been treated intravenously so far and we have not seen any case of allergic manifestations except one in which the connection with diazepam was doubtful as the patient was concomitantly treated with co-trimoxazole. This diazepam emulsion preparation has been found to be markedly free from side effects, thus confirming the opinion that diazepam per se is very well tolerated even in parenteral use.

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¹ Dardel, O von, Mebius, C, and Mossberg, T, *Acta Anaesthesiologica Scandinavica*, 1976, 20, 221.

Growth in renal failure

SIR,—The interesting and valuable paper by Dr P R Betts and others (12 February, p 416) demonstrates the difficulty in assessing the importance of a single variable such as energy intake on growth in children with different degrees of renal insufficiency where other factors are involved. The majority of patients in their study were growing normally and did not have very marked impairment of renal function; thus any effect of energy supplementation could easily have been obscured. Of the 11 children who received supplements, only four had a height below the 3rd centile, and three a growth velocity below the 3rd centile. In addition, growth data were not available for one patient (an infant) and two children had entered puberty and therefore were not strictly comparable with the others.

The method of allocation of the children is unclear to us. If it was on the basis of one nutritional assessment at the start of the study, then analysis of the influence of diet on growth in the preceding year is precluded. On the other hand, if the dietary data refer to analyses during the preceding year rather than at the end it can be shown by a Wilcoxon rank sum test¹ that the five children (excluding the infant and pubertal children) who had energy intakes of less than 80% of recommended daily allowance (RDA) had significantly poorer growth velocities ($P > 0.02$). After energy supplementation when the energy intake improved in three of the five children, no difference in growth velocity is apparent. Only seven of the children (including the pubertal children) had a glomerular filtration rate (GFR) of less than 25 ml/min/1.73 m², which is the level the authors have previously reported to be associated with a reduction in growth velocity.² In the period before supplementation a significant correlation ($r = 0.82$, $P < 0.05$) exists between growth velocity and energy intake, whereas in the supplement period no significant correlation ($P > 0.1$) can be demonstrated.

The question is, do the authors provide enough data to suggest that the decreased energy intake of some children with chronic renal failure is not a causal as well as a related factor in the reduced growth velocity? Only three prepubertal children had both a low GFR (< 25 ml/min/1.73 m²) and a low energy intake ($< 80\%$ RDA) and growth velocity in all of them was greater when on supplements, although in one energy intake did not alter.