Using a similar cot we calculated the angle to be $9^{\circ}24'$. Even on the highest tilt position provided by the manufacturers the angle is $15^{\circ}33'$. In a series of 24 measurements on eight babies in our nursery we found that even while asleep none could be tilted to 45° without sliding. The mean angle at which they slid was $37^{\circ}34'$. This would put the coefficient of static friction for infants in our nursery at 0.765 (similar to that for steel) and would suggest that without some form of mechanical support or velcro nappies 20° might be a working maximum.

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Ear syringing

SIR,—I was interested to read your article (9 February, p 374) on ear syringing by Dr Stuart Carne. May I comment on the question of the risk of damage to the drum or meatus if the patient is a child or a restless adult?

When I was young I was taught to syringe ears in the ear department at Barts, where the tradition was to use an 8-oz bladder syringe for the purpose. It is imperative that this, or any ear syringe, should be held correctly. The pinna of the ear should be held between the index and the middle fingers of the operator's left hand, with the hand resting against the side of the patient's head. The operator's left thumb should then be put in such a position that the ear syringe will rest on the thumb at the point where the nozzle joins the barrel.

In this way, however much the patient moves the operator's hand, the syringe will move with the head, thus reducing any risk to an absolute minimum. Anyone who damages an ear when syringing it and who has not taken this elementary precaution may well find that with the modern trend of damages for negligence he is being sued for damages.

A G S BAILEY

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Setting up a drip

SIR,—With reference to the article on setting up a drip (16 February, p 463), one factor that improved my success rate in finding a penetrable vein was the practice in suitable cases of placing the arm or forearm proposed in a bowl of fairly hot water.

In conjunction with the use of a tourniquet for about 30 to 60 seconds, the arm should be suspended at a lower level than the rest of the body. For example, in a supine patient the upper arm should be allowed to point toward the floor by the side of the bed. Before actual penetration the area should, of course, be dry and sterile.

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SIR,—In their excellent article on setting up a drip (16 February, p 463) Drs Barbara Bannister and C W H Harvard list as the first problem "No veins are visible or palpable," a common and daunting happening for the pressed junior doctor. Rather than proceeding to perform a "cutdown" or attempt to cannulate the subclavian or jugular vein, may I

suggest a very simple and physiological way of increasing the blood flow in the arm and thereby dilating the superficial veins even in the most shocked patient?

If the patient's hand and forearm are immersed in hot water, at a temperature just bearable to one's own hand, and left to soak for approximately 10 minutes, the blood flow in the skin vessels is many times increased and the superficial veins become visible and easy to puncture. We have an arm bath available for this purpose but a bowl is suitable if large enough for immersing the hand and most of the forearm. The temperature of the water must be maintained by adding hot water.

I have used this method for many years and demonstrated and taught it to generations of house physicians, who are invariably impressed at the efficiency and simplicity of the procedure. It is an equally useful technique for venepuncture in blood sampling, and probably gives more reliable results as the sample is of free-flowing blood.

J HART

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SIR,—I was surprised to read that Drs Barbara Bannister and C W H Harvard (16 February, p 463) neglect to use local anaesthesia for setting up a drip or inserting a central venous catheter. The intradermal injection of lignocaine with a 25-gauge needle is simple and quick, and has three advantages.

Firstly, it is not nearly as painful as having a 16-gauge needle and cannula pushed through unanaesthetised skin (I have made observations on myself and others). Secondly, any flinching that the patient is going to do occurs on injection of the local anaesthetic and not at the moment of insertion of the cannula. Thirdly, the 25-gauge intradermal needle can be used to break the skin at the site of injection. This allows a smooth and sensitive approach to the vein, enabling one to feel the different resistances of the tissues that the needle and cannula are passing through. It also prevents the cannula rucking up on the needle.

Finally, the use of paper tape to secure a cannula is an open invitation for the drip to be accidentally torn out by the patient, the nurse, the trolley side, movement of the drip stand, etc. If it can come out it will, and always in the hypotensive patient, or the patient with no veins. There is no substitute for swathing the cannula, etc, in Elastoplast or other similar tape.

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Meningococcal infection

SIR,—Dr P Jan Geiseler (23 February, p 566) gives the results of his review of 1316 cases of bacterial meningitis in support of his contention that "the practice of pretreatment with suboptimal doses of antibiotics prior to hospitalisation is to be condemned."

The mortality rates for his two groups are, however, 7.1% (pretreated) and 8.7% (untreated). Though the decrease in mortality in the pretreated group does not reach statistical significance ($\chi^2=1.15$; $p>0.1$) and the figures cannot be used as a simple justification of pretreatment, they certainly do

not condemn it. It is indeed likely that the indication for pretreatment was the greater severity of the illness when first seen by the GP, and therefore that the prognosis of the pretreated group would have been far worse had the treatment been withheld pending hospitalisation.

Statistics can tell many tales, and perhaps greater caution is needed before condemning GPs out of hand.

CORNELIUS KATONA

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Cardiac tamponade

SIR,—Your leading article (23 February, p 505) on cardiac tamponade is timely, but the final sentence is irreconcilable with effective clinical practice. You rightly say that most physicians see cardiac tamponade only sporadically but that it constitutes a real emergency calling for prompt treatment. Most cases are going to occur in the district general hospital and will be the responsibility of the general physician. If he waits for the arrival of an experienced operator or relies on the availability of a thoracic surgeon most of the patients will not survive. May I suggest that your final sentence should read, "The prerequisites for aspiration are the strong possibility of the presence of a life-threatening amount of pericardial fluid and the presence of a punctilious operator."

In a previous leading article, "The physicians' dilemma" (24 February 1979, p 507) you commented on the decline of the general physician, and similar anxieties have been expressed on both sides of the Atlantic. Your approach to the management of cardiac tamponade highlights one of the main reasons for this decline—that is the progressive transfer from the generalist to the specialist of many of the activities which have previously fallen within the generalist's sphere. The training of general physicians should ensure that they have a working knowledge of all emergency procedures which may be required, albeit rarely, within the confines of the district general hospital and such procedures will include the management of cardiac tamponade.

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SIR,—I was interested to read the statement in your recent leading article (23 February, p 505) that "cardiac tamponade is rare in the postmyocardial infarction syndrome (Dressler's syndrome)." This conclusion is, I feel, somewhat misleading.

Dressler's syndrome is an infrequent complication of myocardial infarction, probably occurring after no more than 3% of cases.¹ However, in Dressler's syndrome, cardiac tamponade has been reported in at least three clinical situations. The first is tamponade due to non-haemorrhagic pericardial effusion.² This was mentioned in your editorial and is, as stated, unusual. Secondly, tamponade may follow leakage of a ventricular aneurysm. Recent reports have suggested that Dressler's syndrome is encountered more frequently when myocardial infarction is complicated by ventricular aneurysm,³ and as these are prone to leakage or rupture⁴ haemopericardium should be borne in mind as a cause of unexplained cardiac failure in Dressler's syndrome.

Thirdly, tamponade may follow inappropriate anticoagulant therapy. Although this is

per se a benign condition, Dressler himself described the main complication of post-myocardial infarction syndrome as being cardiac tamponade in patients who had been prescribed anticoagulants, this being the cause of death in one member of his original series.⁵ It also appears that prior to Dressler's original paper there were numerous reports in the literature describing haemopericardium (often fatal) in postinfarction patients on anticoagulants. Many of these appear in retrospect to have had features of the then unrecognised postmyocardial infarction syndrome.⁶

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¹ Niarachos AP, McKendrick CS. *Br Heart J* 1973;35:49-54.

² Tew FT, Manthe JA, Russell RO, Rackley CE. *Chest* 1977;72:93-5.

³ Curry CL. *J Nat Med Assoc* 1972;64:480-2.

⁴ Hislop WS. *Postgrad Med J* 1978;54:129-32.

⁵ Dressler W. *Arch Int Med* 1959;103:28-42.

⁶ Bernstein A. *Br J Hosp Med* 1977;17:560-71.

Assessment of fitness for surgical procedures

SIR,—Despite Dr Alex Paton's re-writing (23 February, p 529) of the paper by Dr M E Wilson and others (p 509), it is still difficult to understand. Perhaps the message is not so simple—and if it is it could be stated more succinctly in the discussion. Great ideas have been expressed in the form and in particular the length of a letter, as did Watson and Crick when putting forward their theory of DNA. "Writing makyth the exact man." I suggest that the paper should have been written by one anaesthetist and read by sceptical surgeons: should they understand it, all well and good. Many, however, would consider that pertinent variables have been overlooked. The skill of the surgeon, the morale of the patient, and the experience and technique of the anaesthetist are all very relevant, as is the presence in the ward of adequately trained nurses during the pre-operative and postoperative phases.

Surgeons select the more prolonged and difficult operations for the experienced anaesthetist, with whom they may have worked for many years, while the more straightforward procedures may be delegated to trainees. Anaesthetists learn the peccadillos and varying skills of surgeons, but if they are genuinely convinced that the operation is really necessary then they will anaesthetise the gravely ill: the girl deeply shocked and unconscious from her ruptured ectopic pregnancy needs concomitant resuscitation and laparotomy, when the skilled anaesthetist welcomes his responsibility.

ALAN M SMITH

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SIR,—The excellent and original paper on this subject by Mr M E Wilson and others (23 February, p 509), so ably subedited by Dr A Paton, is an important contribution to medical cost-effectiveness. They show that a simple questionnaire can accurately sort out a large proportion of obviously fit people prior to operation. Nevertheless, at present, area health authorities all over the country (particularly those with a (T) added to their name) usually employ expensive young anaesthetists,

and they are expensive, to do this work for them. The implications are obvious.

Unfortunately, during its many vicissitudes prior to publication, what the paper has gained in concision it has seemed to lose in clarity. The final and most important statement in the summary is as follows: "A simple questionnaire predicted fitness for operation by all 10 anaesthetists in 96% of cases." Happily, of course, anaesthetists do not do operations—let alone in teams of 10. The real conclusion of the paper is, in fact, as follows. Out of the 200 routine surgical cases studied, the consensus opinion of 10 anaesthetists was that 149 of them were fit for surgery (fitness category 1 or 2). This conclusion was arrived at after they had studied the answers to a simple 10-point questionnaire. However, after further study of the case notes and results of special investigations the consensus opinion of the 10 anaesthetists was that nine of the patients should be downgraded to fitness category 3.

How important was this? The question still remains unanswered, but that it can be asked is perhaps a sign that we are at last on the road to a more rational use of scarce resources.

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Randomised controlled trials and retrospective controls

SIR,—The methodological problem addressed by Sir Richard Doll and Mr R Peto in their contribution (5 January, p 44) to the debate on my article (17 November, 1979, p 1265) is that of measuring small (20%) differences in the effectiveness of alternative treatments of common disease. This is a problem quite different from any discussed in my article, where the issue is the "inherent fallaciousness" of retrospective controls—a doctrine conspicuous in an influential part of the literature for several decades and not explicitly questioned. My analysis dealt with that issue in the contexts of rare disease, common disease with a wide range of manifestations (cancer), and the early stages of clinical investigation generally, but not at all with the problem addressed by Sir Richard and Mr Peto.

In view of that fact, it is surprising that they assert that I have "encouraged" retrospective controls "at the expense" (their italics) of randomised concurrent controls, as though the two were mutually exclusive. Reasons why they should be considered complementary, even where the large, randomised trial is indicated, are given below.

(1) *Pretrial role*—To determine that the expense and labour of a large randomised trial are justified, smaller trials are necessary to establish the bounds of the results to be expected and to explore the pitfalls of large trials. This is a phase to which I particularly referred in connection with the use of retrospective controls.

(2) *Implementing the finding*—Once a randomised trial has been carried out and the finding of a small but statistically significant improvement has been publicised, one must look to its impact on practice. Since the margin of improvement is small it is certain, on statistical grounds alone, that in many instances experience will seemingly contradict the result of the large randomised trial. More serious is the possibility that a trial which has randomised physicians may have washed out real benefits which individual physicians with great experience in diagnosis and in the administration of an alternative treatment may be able to provide for their patients. The dilemma is one which has to be resolved by

the individual physician. Clearly, the judgment of the physician in evaluating his own experience and in weighing it against the verdict of the trial may be seriously biased by factors which have been frequently enumerated as defects in the use of historical methods. If he does not alter his mode of treatment, however, he has been challenged to hone his skills of objective analysis in the evaluation of the histories of his own cases—a clearly complementary aspect of randomised and historical technique.

(3) *Post-trial analysis and final assessment*—So-called "retrospective stratification" is itself a recognised tool of analysis of the results of randomised trials. And if there is ever to be a "final" assessment of the worth of a given randomised trial and of the benefits realised from application of its results to practice, how is it going to be arrived at except by historical analysis?

A final comment is in order on the application of computers to the storage, retrieval, and objective analysis of case histories. Clearly, a computer provides a means of performing these tasks on a hitherto unprecedented scale with programs of every-increasing sophistication. A local computer would enable an individual physician to press a few buttons and have before him the records of all his past cases which match most closely that of the current patient. A terminal connected to a central computer could supplement that information with the case histories compiled by colleagues. In research trials the matching of individuals for assignment to experimental and control groups by computer would seem to provide important advantages over the simple flipping of a coin.

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*.*This correspondence is now closed.—Ed, *BMJ*.

The editor regrets . . .

SIR,—You observe in the opening line of your leading article (23 February, p 508) that rejection of a paper for publication can make an author angry, frustrated, or miserable. I would say "paranoid" as well. What I find so disheartening is that after spending months on a paper it can be rejected, sometimes months later, without any reason being given. In some cases a further approach to the editor still does not extract any clues about the reason for rejection. It may well have been the case that my papers were non-starters. The difficulty was not that I could see no reason for the papers to be rejected but that there were several possibilities. For this reason I have given up trying to write full-length papers.

Why cannot editors of medical journals conform to the principles laid down by the *Journal of Neuropathology and Experimental Neurology*? Referees have to report back within two weeks according to six criteria; if the editor rejects the paper he will give reasons, often by quoting the referees. This is all the more essential for those authors who do not have colleagues to criticise or evaluate their papers.

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*.*Our justification for giving no reasons for rejecting most articles is simple; with 4500 rejects a year we cannot find time to write detailed appraisals on every occasion. When, however, authors ask for specific reasons we can usually provide them.—Ed, *BMJ*.