same issue (p 656); D, the incidence of morbid events, corresponds to a negative utility function. The ratio R expresses the benefit in treating a true positive as a multiple of the loss in treating a false positive; such loss must include the limited nature of resources and inconvenience involved were all pregnancies to be monitored intensively as from 34 to 36 weeks of gestation. Dr Klipstein's formula (4) can be taken a little further: T_2 is better if $R > 7{\cdot}3$, apparently, though rounding errors demand that such calculations be performed on the original data rather than on summary statistics. In the same way, the low value of R, 5/3, in Card's example led to a preference for medical rather than surgical treatment.

Two further points warrant mentioning. The discrepancies of the false-positive rates from the expected 10% are nothing but artefacts of the smoothing process. The false-positive rates for all the potential screening variables could be standardised at, say, 44/438 by moving each 10th percentile line parallel to itself until exactly 44 of the points representing infants of normal growth lay below it. Such adjustment for the product of crown-rump length and trunk area in the paper of Dr Neilson and colleagues (17 May, p 1205) would be in a downwards direction and would thus almost certainly add two further false negatives, making 4/36 (11%) instead of 2/36 (6%) and matching the false-negative rate of the product with the circumference. The moral is that with only 36 positives these two screening criteria are indistinguishable in performance.

Furthermore, it is asserted that fetal-head measurements proved to be the least sensitive indicators of growth retardation. We are concerned, however, about growth retardation not for its own sake but as a marker for mortality, morbidity, and potential need for intervention. The 56-59% of growth-retarded neonates whose heads were small and in whom perhaps "brain sparing" has been less effective may well fare considerably worse than the corresponding "false negatives" and on clinical grounds may be a more appropriate group to identify.

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Perinatal practice and compensation for handicap

SIR,—Mr P Mitchell and Mr I G Chalmers (27 September, p 868) have done a public service in pointing out the risk that defensive attitudes will inevitably affect perinatal practice unless the Government brushes aside the objection of some lawyers and assumes both the responsibility for compensating handicapped children and the right to sue those responsible for the recovery of the costs involved. Such a ruling would put suer and sued on more equal terms than are likely to obtain when an individual parent tries to recover damages from a powerful international company, and would also obviate the temptation for parents to sue individual doctors and nurses for alleged mistakes of judgment made in good faith in the exercise of their duties.

The present situation is highly unsatisfactory, cases of real negligence being settled out of court without publicity while defensible cases are heard in public, thus exposing often innocent parties to professional denigration based on the natural sympathy that the public feel for handicapped children and their assumption that someone must be to blame as an alternative to accepting that we live in a difficult and dangerous universe. Our slogan should be "to each according to his need and from each according to his liability" and this can be achieved only if the State acts as broker and parents abrogate their right to sue in exchange for adequate assistance in rearing their handicapped child—assistance which is more likely to be appropriately directed than the residue left after the deduction of legal expenses when damages are awarded to the child for administration by his guardian.

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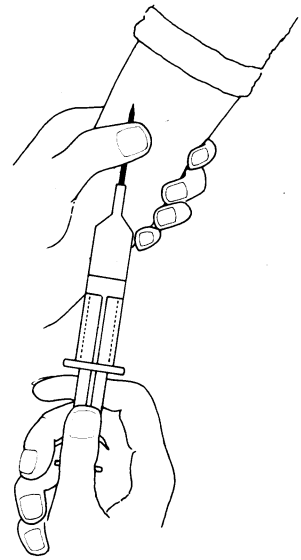
Taking blood and putting up a drip in young children

SIR,—In that it dismisses the external jugular, internal jugular, and femoral veins as sites for venepuncture, I assume that the article by Drs Andrew Whitelaw and Bernard Valman on how to take blood in babies (30 August, p 602) excluded the seriously ill baby where the peripheral circulation is compromised. On the assumption that the patient is well perfused, I cannot condone the routine use of butterfly needles for taking blood. This is a tendency which is creeping in among junior paediatricians and it is entirely unnecessary and expensive.

One butterfly needle costs 50p whereas the standard No 1 (green) needle costs 2p. If blood is taken from a hand, foot, or scalp vein, the needle can be inserted without a syringe and the blood allowed to drip into the collecting tube—unaesthetic maybe, but effective and cheap. If, as usual, the antecubital veins are used, then with experience an ordinary syringe and needle are perfectly adequate



for whatever technique the individual has developed. I find the one-handed technique shown in the figure helpful, as the thumb of the other (supporting) hand can be used to steady the needle and even vary its position in the vein.

In summary, there *are* indications for using butterfly needles for taking blood—for example, for taking large quantities or perhaps arterial puncture—but on the whole I think that the expense involved should discourage their routine use.

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SIR,—I also read with interest the article about taking blood in young children (30 August, p 602), and the reply by Dr C G Peters (27 September, p 870). His use of a fibreoptic cold light source to transilluminate the patient's hand or foot to demonstrate vessels is a good idea. However, I would like to draw attention to a report in the annual report of the Medical Defence Union[1] of a case in which such a cold light source was used, and four partial or full-thickness burns resulted. The following day the procedure was repeated on the other arm, this time with a protector, with a gap of 2 cm between the light and the skin; and further burns occurred. The doctor then tested the light source on his own finger and burnt it. The instrument, intended for endoscopy, is thought to reach a temperature of 70° or 80°C. The incident was investigated; the instrument was not found to be defective, but its use had been unorthodox.

I would like to agree that I have found these light sources becoming very hot, and that doctors using them to demonstrate vessels should show extreme caution in order to protect the child's skin from burns.

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[1] Medical Defence Union. *Annual report*. London: MDU, 1980:22.

**Other readers have written on this point and we thank them for their letters.—ED, *BMJ*.

Progesterone, fluid, and electrolytes in premenstrual syndrome

SIR,—Dr Anthony Clare (20 September, p 410) has great difficulty in comprehending the definition of true premenstrual syndrome. Reading the chapter "Definition and diagnosis" in my book *Premenstrual Syndrome and Progesterone Therapy*[1] and a glance at figure 2.1 showing the common timing of symptoms should resolve his confusion. For simplicity's sake, premenstrual syndrome could be defined as "the recurrence of symptoms on or after ovulation, increasing during the premenstruum and subsiding during menstruation, with complete absence of symptoms from the end of menstruation to ovulation." A carefully recorded three-month calendar will enable him to decide the diagnosis in "the woman whose diary reveals symptoms occurring intermittently during the cycle." There are no characteristic symptoms of premenstrual syndrome, although there are common symptoms. The diagnosis can never be made on the presence of symptoms alone.

A strict definition of premenstrual syndrome is of practical as opposed to theoretical importance, because of the aetiological significance and the therapeutic response to progesterone. In her controlled trials of progesterone Sampson[2] was using patients with menstrual distress and, as stated in my letter to the *British Journal of Psychiatry*,[3] the correct title would have been: "Menstrual

distress: a double-blind trial of progesterone and placebo." Progesterone is no panacea for all the diagnoses which come under the umbrella of menstrual distress, including premenstrual exacerbations of symptoms present throughout the cycle, as well as spasmodic dysmenorrhoea (which responds to oestrogens and oral contraceptives) and endometriosis (which responds to progestogens).

Dr Clare also refers to the trial by Smith[5]; progesterone suppositories raise the plasma progesterone for 6-24 hours[4] so medication is required at least once or twice daily and, on an adequate progesterone dosage, there is a cumulative effect during the first four days.[4] Smith used a small dose of progesterone suppositories 200 mg on alternate days from day 19 until day 26; thus the progesterone was too late, too little, and too infrequent for this to be considered a trial. I would repeat that the common causes of failure with progesterone therapy are incorrect diagnosis and inadequate dosage.[1]

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[1] Dalton K. *Premenstrual syndrome and progesterone therapy*. London: Heinemann Medical Books, 1977.
[2] Sampson G. *Br J Psychiat* 1979;**135**:208-15.
[3] Dalton K. *Br J Psychiat* 1980;**137**:199.
[4] Nillius SJ, Johansson EDB. *Am J Obstet Gynaec* 1971;**110**:470.
[5] Smith SL. In Sachar EJ, ed. *Topics in psychoendocrinology*. New York: Grune and Stratton, 1975.

Gonorrhoea presenting as "sterile" pyuria

SIR,—Dr B Chattopadhyay and Mr I Hall draw attention in their paper (27 September, p 841) to the fact that gonorrhoea may occasionally present in men as "sterile" pyuria. In such circumstances urethral discharge is not noted by the patient (though careful examination after "milking" the anterior urethra will nearly always show evidence of purulent secretion).

The important clinical lesson to be drawn from their account is that symptoms of dysuria and frequency in young men should always be regarded as potentially serious conditions due either to sexually transmitted infections or to urinary tract infections secondary to some underlying abnormality. Among the common sexually transmitted infections, non-specific urethritis is often responsible for frequency and gonorrhoea for dysuria.

I would have thought that gonococcal infection in the female would be an extremely rare cause of "sterile" pyuria if a properly collected clean-catch specimen is sent to the laboratory. The short female urethra is usually adequately "rinsed" free of inflammatory cells by micturition with this technique.

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SIR,—Dr B Chattopadhyay and Mr I Hall (27 September, p 841) are correct to add gonorrhoea to their list of causes of sterile pyuria in women, but the reason may be perverse. We were recently aided in making the diagnosis of gonorrhoea in a woman who presented complaining of haematuria by the finding of "sterile" pyuria in a midstream urine.

Two urine specimens received two days apart, and containing more than 20 white cells per high-power field of a centrifuged deposit, showed no growth when cultured on CLED selective medium. Further incubation on non-selective blood agar in the presence of carbon dioxide failed to reveal the presence of significant numbers of fastidious organisms,[1] although no attempt was made to identify *Neisseria gonorrhoea*. Gram staining of the deposits revealed a mixed flora consistent with vaginal contamination. On being further questioned, the patient admitted to the recent onset of a vaginal discharge and *N gonorrhoea* was isolated from a cervical swab.

Women with vaginal discharges may frequently contaminate a midstream urine and the use of a vaginal pack inserted before collection has been recommended (Association of Clinical Pathologists Broadsheet 80). The finding of "sterile" pyuria in women may suggest a vaginal origin for these cells, and thus lead to a diagnosis of gonorrhoea.

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[1] Maskell R, Pead L, Allen J. *Lancet* 1979;i:1058-9.

Systematic review of the benzodiazepines

SIR,—The recommendations presented by the Committee on the Review of Medicines in its article "Systematic review of the benzodiazepines" (29 March, p 910) appropriately focuses concern about the use of these drugs. However, I would like to comment on one point that perhaps should be expanded on.

In the opening paragraphs the committee refers to the differences between the "long"-acting and the "short"-acting benzodiazepines, yet in the guidelines for lorazepam, oxazepam, etc, it mentions that paradoxical aggressive outbursts and uncovering of suicidal tendencies have been reported with the benzodiazepines. It should be noted that these reactions have been reported only in relation to the long-acting drugs—namely, diazepam,[1 2] chlordiazepoxide,[1 2] clorazepate,[3] and flurazepam[4] but not with oxazepam.[2 5] In fact, oxazepam reduced anxiety scores in such patients and in this respect may be the drug of choice for the susceptible population. Furthermore, the uncovering of suicidal tendencies is probably a reference to the report by Hall and Joffe,[6] which implicates only diazepam.

As the opinion of the committee will no doubt exert considerable influence on the labelling and use of benzodiazepines, I feel it would be appropriate to clarify this seeming inconsistency.

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[1] Anonymous. *Mass Gen Hosp Newsletter* 1978;**1**:38.
[2] Gardos G, DiMascio A, Salzman C, Shader RI. *Arch Gen Psychiatry* 1968;**18**:757-60.
[3] Karch F. *Ann Int Med* 1979;**91**:61-2.
[4] Einarson TR. *Drug Intell Clin Pharm* 1980;**14**:291-2.
[5] Kochansky GE, Salzman C, Shader RI, Harmatz JS, Ogeltree AM. *Am J Psychiatry* 1975;**132**:861-3.
[6] Hall RCW, Joffe JR. *Am J Psychiatry* 1972;**129**: 738-42.

Heart rate variations in the Guillain-Barré syndrome

SIR,—We have read with interest the paper by Dr J C Frison and others (6 September, p 649) reporting a reduction in cardiac beat-to-beat interval variation among patients with Guillain-Barré syndrome, which was taken to indicate a disorder of the autonomic innervation of the heart. We would like to point out that beat-to-beat variation, recorded as the standard deviation, is strongly dependent on both age and size of the mean R-R interval, so that these factors must be considered when comparisons are made between patients and normal groups.

Our measurements of beat-to-beat variation in normal subjects (to be published) show that the standard deviation declines with age and increases with the size of the mean R-R interval. Thus with a mean interval of 600 ms (or 100 beats/min) the standard deviation will average 45 ms at age 7 years but 24 ms at 50 years. Similarly, at age 33 years the predicted standard deviation will be 31 ms at a mean interval of 600 ms and 66 ms at a mean interval of 1000 ms (or 60 beats/min).

Thus although the two groups studied by Dr Frison and his colleagues were matched for age the difference in the size of the standard deviation between the two could be explained, at least in part, by a tachycardia in the Guillain-Barré patients. Since tachycardia is a common feature of the acute phase of the illness, its resolution during recovery should necessarily be accompanied by an increase in the size of the standard deviation.

In conclusion, we do not believe that the reduction in beat-to-beat variation noted by these authors can properly be ascribed to autonomic neuropathy in the absence of a consideration of resting heart rate.

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No to euthanasia

SIR,—Having read the statement on euthanasia (6 September, p 691), may I voice the opinion of a geriatric nearing 80 who has had a lengthy experience of hospitalisation in different medical and surgical wards (not strictly geriatric) for painful afflictions, which at times in my weakened state made me feel that I would be better off dead? Despite the almost unbearable pain I sometimes endured I am strongly opposed to the belief, expressed by EXIT in a recent broadcast on Radio 4, that euthanasia should be officially legalised. I find it difficult to appreciate the benefits (and what really are they?) which are claimed.

So far as those who are younger and normally able are concerned, it can seldom be needed, as most can—and in fact do—voluntarily provide their own euthanasia by overdoses of drugs, etc. With regard to the elderly, whether or not the illness is terminal—and indeed how can any doctor be *quite* sure?—there exists an emotional ebb and flow, with depression deepening more at some phases of the illness than at others. At such low levels the patient is most suggestible, and may quite well give a misconceived consent to euthanasia, whereas in a better phase he might have decided more reasonably otherwise. During this highly suggestible time there is some risk of exposure to the insidious persuasion of anyone who might have personal reasons for not wishing him or her to live longer. Under such circumstances distressed close relatives might reluctantly agree to give their consent, only to