What did they say? Where did they come from?

SIR,—In Dr M B Skirrow's valuable account of campylobacter enteritis (2 July, p 9) we read that "several patients stated that they lost over 6.5 kg." Did no one say, "Doctor, I lost over 6.35029 kg" or even "Doctor, I lost over a stone"?

Standardisation may hamper expression. What are we to make of one patient who was "a fit 32-year-old Caribbean woman" and the next who was "a 30-year-old Caucasian male" (Mr M Harris and Mr J P Rood, p 20)? The words seem to be used in different ways. As her picture does not suggest that the woman is a Carib the adjective is presumably applicable to the sea of that name, so she comes from somewhere in Central America, or the West Indies, or the northern part of South America. But he, did he really come from the Caucasus (on a Russian passport?) or is the meaning simply that he is Indo-European? ("Now discarded" was the premature comment of the *Shorter Oxford English Dictionary* in 1933.)

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* * In fairness to Dr Skirrow it must be stated that his original text read "... they lost over a stone (6½ kg)." The published version was the result of an excess of subeditorial zeal for metrication and decimalisation—ED, *BMJ*.

Shortage of anaesthetists

SIR,—Ten years ago I suggested¹ that the science of anaesthetics was now sufficiently advanced for much of our routine work in operating theatres to be delegated, provided that satisfactory candidates for the work could be recruited. I had in mind, I wrote, those who had reached a standard of education close to that required for acceptance at a university—or graduates, preferably in science. Such people, I added, might also solve the problem of providing anaesthetists for work in dentists' surgeries.

In January 1970 I repeated² this suggestion in the correspondence columns of the *Lancet*, opening a debate that continued throughout the year. Surprisingly, the idea received some support from anaesthetists. My suggestion was also debated in the autumn of that year at a gathering of about 100 anaesthetists and was supported by 15 or more of those present, including two professors of anaesthetics. This was still more surprising since, as one speaker put it, to vote that way amounted to professional suicide.

Now that there is a serious shortage of anaesthetists, perhaps the time has come to reconsider the possibility of training and employing the kind of paramedical anaesthetists I had in mind.

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¹ Bourne, J G, *Studies in Anaesthetics*, p 159. London, Lloyd-Luke, 1967.

² Bourne, J G, *Lancet*, 1970, 1, 38.

SIR,—Mr Charles Langmaid (25 June, p 1665) and Dr T R Austin (9 July, p 120) call attention to, and the latter invites reports of personal experience with, alternatives to the usual methods of modern anaesthesia.

More than 40 years ago, as a final-year

student, I was privileged to be associated with the late Dr Howard Jones and the late Mr Norman Lake when, together, they were evolving and perfecting the use of the light spinal anaesthetic. From them I learned the technique, the pitfalls, the limitations, and the advantages of this method of anaesthesia, a knowledge which proved of utmost value when appointed to the Colonial Medical Service and posted to remote areas where the alternatives were either a "rag and bottle" in the hands of a local assistant or no surgery at all.

In the earlier years of a professional lifetime spent in developing countries I used spinal anaesthesia in hundreds of operations in situations of varying degrees of isolation. When, later and in less primitive conditions, skilled assistance became available and one handed over this responsibility to anaesthetist colleagues, at no time did I feel that spinal anaesthesia was thereby outdated. Similarly with regional block, of which Mr Lake was a superb exponent and teacher, and I remain convinced that these methods still have a place in the disciplines of surgical anaesthesia.

If, as your correspondence columns indicate, there is a grave and growing shortage of anaesthetists it would appear reasonable that these specialists should concentrate on those fields of surgery where their experience, skill, and knowledge are most needed, which would still leave a vast amount of elective surgery which could be performed under spinal or regional block administered by the surgeon or his assistant.

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Primary amenorrhoea

SIR,—The review of "primary amenorrhoea: the ambiguous non-entity" by Dr Jean Ginsburg and her colleagues (2 July, p 32) contains interesting data from patients with primary amenorrhoea, but the following comments are pertinent.

The 10 patients who had no ovarian biopsy data (50% of the series) include five of the six patients with elevated prolactin levels, four of the five patients with enlarged pituitary fossae, and apparently one of the six patients who achieved human menopausal gonadotrophin (HMG)-induced pregnancies. The value of accurate and informed reports on ovarian biopsy specimens from patients with primary amenorrhoea¹ should not be underestimated.

The authors properly define primary amenorrhoea but inaccurately define secondary amenorrhoea² and make no mention of oligomenorrhoea,³ primary or secondary. They make the bold statement that "the division of amenorrhoea into primary or secondary has no aetiological, diagnostic, or therapeutic basis" and go so far as to suggest that "the conventional distinction between primary and secondary amenorrhoea be abandoned. . . . Primary amenorrhoea should instead indicate an abnormality of the gonad itself and secondary amenorrhoea an abnormality that results from hypothalamic-pituitary dysfunction." But menstruation merely is shedding of the endometrium and a menstrual abnormality, whether this be absence of menstruation or an upset cycle, is no more than a symptom or sign associated with a legion of pathological conditions of which abnormality of the gonad and hypothalamic pituitary dysfunction are but two.

Dr Ginsburg and her colleagues have valid points to make, but they have overstated their cases, to the detriment of their good intentions, by taking a narrow view of this complex subject. Perhaps, with reflection, they can couch their ideas in terms just as cogent but less pejorative. The title of their paper is misleading. Primary amenorrhoea has a precise definition and embraces a complex multiple entity.

Their suggestion that "the conventional distinction between 'primary' and 'secondary' amenorrhoea should be abandoned" is unacceptable from the wider view of medical practice whatever the "current practice for other endocrine glands" happens to be. Perhaps the authors should tread lightly when they venture into the more or less fertile fields of gynaecology and obstetrics and their endocrinology, just as we are wont to do when we choose to explore territory more familiar to medical endocrinologists.

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¹ Black, W P, and Govan, A D T, *British Medical Journal*, 1972, 1, 672.

² Black, W P, and Govan, A D T, *American Journal of Obstetrics and Gynecology*, 1972, 114, 739.

³ Govan, A D T, and Black, W P, *European Journal of Obstetrics and Gynaecology, Reproduction and Biology*, 1975, 5/6, 317.

Prognosis of Henoch-Schönlein nephritis in children

SIR,—The paper by Dr R Counahan and others (2 July, p 11) makes a valuable contribution to our appreciation of the hazards of the Henoch-Schönlein syndrome.

In 1956 Dr Margaret Rogerson and I reviewed 94 cases of the syndrome seen at this hospital between 1943 and 1955.¹ Approximately 6% developed chronic nephritis. It seems likely that a longer follow-up would have revealed more cases with severe renal damage. In the cases we reviewed and in others seen by me since 1956 it has been evident that neither the age of the child nor the severity of the initial illness had any bearing on the likelihood of later renal sequelae. A special interest in this condition over many years has taught me one thing—the outcome in the majority of cases is unpredictable.

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¹ Derham, R J, and Rogerson, M M, *Archives of Disease in Childhood*, 1956, 31, 364.

Parallel aging of Achilles tendon and coronary artery

SIR,—Sir John McMichael (30 April, p 1155) correctly criticises the use of "dietary fat manipulation" to reduce the risk of coronary heart disease (CHD). Adams *et al*¹ at Guy's Hospital Medical School, have shown by necropsy studies that the connective tissue of the Achilles tendon and coronary artery age along parallel lines. This explains the rising popularity of formal marathon training in cardiac rehabilitation programmes (22 January, p 229), since the 42-km foot race is a good test of the integrity of the Achilles tendon. "Distance covered on foot" can be used as an index of both cardiac status and the state of the connective tissues of the Achilles tendon.

Two dietary factors have been implicated in the parallel aging of tendons and arteries. Schwartz² suggests that the trace element silicon is the factor in dietary fibre and hard water which reduces the incidence of CHD. Silicon contributes to the stability of connective tissue by forming -O-Si-O- bridges. Ascorbic acid is the second factor. Its role in collagen synthesis³ makes it essential for both tendons and arteries.

Our observations on cardiac patients who take up marathon running show a conspicuous increase in silicon-rich foodstuffs (pectin, bran, and alfalfa) and also ascorbic acid (fresh fruit and vitamin C supplements). The longest run for cardiac patients is the Primo 100-km run on the island of Oahu. It is supported by the cardiac rehabilitation programme of the Honolulu YMCA. The event is social and non-competitive.

In our view any dietary manipulation should be aimed at increasing "distance covered on foot."

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¹ Adams, C W M, *et al*, *Atherosclerosis*, 1974, **19**, 429.
² Schwartz, K, *Lancet*, 1977, **1**, 454.
³ Turley, S D, *et al*, *Atherosclerosis*, 1976, **24**, 1.

Paget's disease of bone

SIR,—Your leading article (4 June, p 1427) dismisses glucagon and mithramycin in the treatment of Paget's disease of bone because of disadvantages which were not enumerated.

The dose of mithramycin used by Ryan *et al* was 25 µg/kg/day and was reduced below this only for elderly patients and those with side effects. If the dose of mithramycin used is 15 µg/kg/day patients respond clinically and biochemically and no anorexia, nausea, vomiting, drowsiness, fever, thrombocytopenia, or hepatic damage is encountered.² A dose of 10 µg/kg is also effective. I know of no reports of the use of oral cytotoxic drugs in patients with Paget's disease of bone, but a number of years ago I had the opportunity of observing the effect of azathioprine 50 mg/day for three weeks given to a patient who happened to have Paget's disease of bone. There was no relief of bone pain but both serum alkaline phosphatase and urinary hydroxyproline levels fell gradually. Paget's disease of bone is a relatively common disorder and clinicians should from time to time have opportunities to observe the response of the bone disorder to oral cytotoxics given for other reasons.

Glucagon when given intravenously or subcutaneously is remarkably free of side effects. The only problem encountered in the frequent use of the drug over seven years^{3 4} has been nausea and vomiting, which occurs when large doses of the hormone are given by intravenous infusion. These symptoms are dose-dependent and are rapidly relieved by stopping the infusion for a few minutes, glucagon being rapidly cleared from the blood.⁵ Glucagon when given intravenously at a dose of 8-10 mg/day results in relief of bone pain and a fall in serum alkaline phosphatase and urinary hydroxyproline levels within 10 days. A similar clinical and biochemical response takes many months with calcitonin.

The patient with Paget's disease of bone can be maintained on subcutaneous glucagon

2-3 mg once or twice daily without side effects. Alternatively glucagon can be given in combination with calcitonin and I hope to publish further data on this variation of therapy in due course. Glucagon may actually have some advantages over the calcitonins. Hypocalcaemia is uncommon during glucagon therapy and the hormone may be less likely to give rise to antibodies. Furthermore, I have not encountered a patient with Paget's disease of bone who was resistant to glucagon therapy.

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¹ Ryan, W G, Schwartz, T B, and Northrop, G, *Seminars in Drug Treatment*, 1972, **2**, 57.

² Condon, J R, *et al*, *British Medical Journal*, 1971, **1**, 421.

³ Condon, J R, *British Medical Journal*, 1971, **4**, 719.

⁴ Condon, J R, Knight, M, and Day, J L, *British Journal of Surgery*, 1973, **60**, 509.

⁵ Assan, R, Tchobrousky, G, and Gross, G, Abstract submitted to the meeting of the European Association for the Study of Diabetes, Southampton, September 1971.

Psychotropic drugs and road accidents

SIR,—Your leading article (11 June, p 1492) "Deaths due to drug treatment" made no mention of one possible lethal consequence of drug treatment: road traffic injury. The relationship between the taking of drugs and subsequent accidental injury or death is a field which has attracted little interest or research effort to establish the facts. This is surprising in view of the importance of the matter.

In 1970 in the USA Kibrick and Smart¹ reported that 11-15% of accident-involved drivers had taken a psychotropic drug before the accident. A report of the Medical Commission on Accident Prevention² has reviewed the present situation and summarised the scanty amount of information which has accumulated since Kibrick and Smart published their findings. This report states that "doctors must constantly bear in mind the possibility of adverse drug effects on driving and much will therefore depend upon the information which is available to doctors on this important subject. Unfortunately . . . information about the effects of drugs on driving behaviour is extremely hard to obtain."

The situation as regards the effects of psychotropic drugs and other medications on driving behaviour is certainly complex and little is known with certainty. Clayton³ states that "the role of psychotropic drugs in the causation of driving accidents remains unclear and will remain so until carefully controlled large-scale field studies are undertaken." There is little hard evidence that the acute consumption of a drug with a sedative effect may adversely affect skill in driving and there is practically no knowledge of the effects of chronic consumption of such drugs on driving or other activities requiring a high degree of motor co-ordination and judgment. Yet the evidence incriminating alcohol consumption in impairment of ability to drive is now overwhelming and the response of society to this evidence has been the introduction of the breathalyser.

Sedative drugs, of course, are not detected in the breathalyser and if they were there would be no legal implications. Yet there is good evidence that the sedative drugs commonly prescribed (previously the barbiturates and now the benzodiazepines) do potentiate the

effect of alcohol. A standard work on the side effects of drugs⁴ states that "diazepam has a tendency to produce drowsiness and ataxia. . . . There is a summation effect with alcohol: patients should be warned to be very moderate with alcohol and to take none before driving."

Prescriptions for sedative drugs and hypnotics now run into millions a year. The people to whom they are prescribed are also likely to be using alcohol, perhaps in unaccustomed amounts; yet there is no requirement placed upon the medical profession to issue a warning of this interaction effect when the drugs are prescribed.

We believe that research in this field has been too long delayed and that it should now be accorded priority. Until such time as definite information is available to guide doctors we consider that it should be made obligatory to issue warnings of interaction effects with alcohol whenever sedative drugs are dispensed. Complaints may be made that issuing such warnings and recommendations will inevitably decrease treatment compliance, but if this is to be set against the potential saving of life then the balance will be favourable, for sedative drugs are frequently prescribed for relatively trivial reasons. If the concealed dangers were to be forcibly pointed out to patients and to doctors, then it would increase the chances that they are only prescribed for, and taken by, those patients who really need them.

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¹ Kibrick, E, and Smart, R G, *Journal of Safety Research*, 1970, **2**, 73.

² *Drugs and Medicines* (reprinted from *Medical Aspects of Fitness to Drive*, 3rd edn). London, Medical Commission on Accident Prevention, 1976.

³ Clayton, A B, *Human Factors*, 1976, **18**, 241.

⁴ Meyler, L, and Herxheimer, A, *Side Effects of Drugs*, vol 7. Amsterdam, Excerpta Medica, 1972.

Abdominal tuberculosis in Britain

SIR,—Your leading article (18 June, p 1557) reminds us that abdominal tuberculosis still occurs in the indigenous population of the United Kingdom. Gynaecologists would do well to recall its possibility, particularly in Asians but also in other races. Four cases have been seen in a short personal experience in the West Midlands. The fourth and most recent one illustrates how difficult it can be to establish the diagnosis, however great the clinical suspicion.

An Asian woman, 22 years of age, was seen at the 30th week of her first pregnancy because the uterus was larger than expected. An x-ray of the abdomen confirmed the presence of twins. The patient was admitted to hospital because she looked ill and weighed only 44 kg. The haemoglobin concentration was 12.3 g/dl and the chest x-ray normal. Labour started spontaneously at the 35th week and she was delivered of living twins (1.68 kg and 1.75 kg). Within 48h pyrexia with marked tachycardia developed and anaemia, jaundice, and hepatomegaly followed. The haemoglobin fell to 8.1 g/dl with 8.6% reticulocytes and the urine was free of bile. The plasma albumin concentration fell to 25 g/l and that of globulin rose to 38 g/l, while the alkaline phosphatase activity rose to 95 King Armstrong units. A pure growth of *Escherichia coli* was obtained from the urine and a high vaginal swab. Initial blood cultures were sterile. Antibiotic therapy was commenced. By the 10th day the