The girl was brought into the surgery by one of the nursing attendants. She was not apprehensive and she sat down quietly in the dental chair semi-upright (45°). There was nothing about her manner to make me at all suspicious that she was in any way unwell. The dentist then examined her mouth and inserted the dental prop and pack. I induced anaesthesia in the normal fashion with a mixture of 75% nitrous oxide and 25% oxygen, using a mouth and nose piece with a pressure of 5 mm Hg. Induction was normal and smooth and after about 2 min I gave halothane, starting with 1 unit (0·05%) on the vaporizer scale and gradually increasing to 2 units (0·5%). The dentist then proceeded with the planned extraction of two teeth and the whole procedure was completed within 5 min. I then discontinued the anaesthetic and the dentist took out the mouth prop and pack while I waited for the patient to recover from the anaesthetic.

After approximately 1 min I noticed that she became pale and her respiration shallow. I immediately felt for the radial pulse, but this was not palpable so at once I shouted "cardiac arrest." At the same time I lifted her from the chair and put her on the floor and started giving her external cardiac massage. I then asked the dentist to administer pure oxygen by mask. The patient started breathing rather irregularly, but I still could not feel the radial pulse so I administered an intravenous injection of 15 mg of methylamphetamine while continuing the cardiac massage. Also an ambulance was summoned immediately. The first injection of methylamphetamine produced no response, so after 5 min I gave another 15 mg, all the time continuing external cardiac massage and oxygen. The ambulance arrived about 25 min after the call for it and before transferring the patient into the ambulance I gave another 15 mg of methylamphetamine intravenously. The patient was transferred to the ambulance and I accompanied her to hospital, where I passed a cuffed endotracheal tube and administered oxygen with the Boyle's anaesthetic machine. Her treatment was then taken over by the medical staff of the hospital, but the improvement was transient and she died 6½ hours after the collapse. Post-mortem findings were negative.

It appears that the child had a vasovagal attack, and the cardiovascular collapse occurred without apparent cause or warning. I strongly support the explanation given by Dr. Mehta that "once peripheral vasodilatation has begun and blood pressure is falling the sitting position will increase cerebral oligaemia by gravitational pooling of blood in the dependent portions of the body and will further lead to reduced filling of the right atrium and cardiac output. There is no doubt that whatever the cause of hypotension its outcome in terms of brain damage or death of the patient is greatly influenced by the upright position traditionally used in dentistry." I agree with him that, though death during dental anaesthesia is rare, one could avoid these occasional fatalities by altogether abandoning the sitting position or by intensive monitoring while anaesthetizing the patients in the dental chair.—I am, etc.,

A. S. MODY

Anaesthetic Department,
Bedford General Hospital (South Wing),
Bedford

Randomization

SIR,—Mr. J. A. Lewis (5 July, p. 41) raises queries about the use of month of birth as a technique for allocating patients in a clinical trial. He questions whether there is any real correlation between morbidity and month of birth. There is a clear association between month of birth and morbidity in infants 1 week to 2 months old with increased mortality from a range of disorders occurring in infants born during the winter months (this is shown by table 25 in the Registrar General's *Statistical Review of England and Wales*). At the opposite end of the age range I have recently detected an excess of deaths occurring in the month of birth and succeeding three months among persons aged 75 years and over.[1] This excess, though only accounting for about 1% of the deaths, is consistent in either sex and subgroups by marital status. The excess is statistically significant and again serves as a warning of potential association between month of birth and morbidity.

However, the use of an open technique such as date of birth has other major hazards in the allocation of patients. A fundamental concept in controlled clinical trials is that the patients should be entered for the trial and once they have agreed to participate random allocation to treatment group should then occur. Use of an open technique of allocation (such as date of birth, date of hospital attendance, hospital record number) enables differential recruitment to occur into the treatment groups. There are a number of reasons why this can occur and there are some well-documented studies where gross bias has occurred because of such action.[2] This latter point is a stronger argument for using an appropriate "blind" randomizing technique.—I am, etc.,

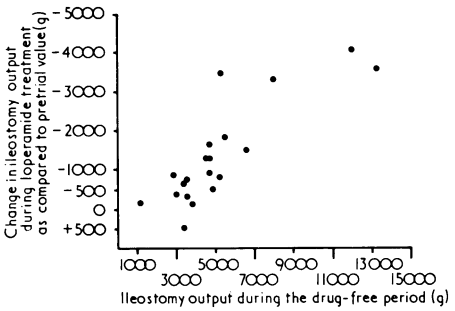
MICHAEL ALDERSON

Wessex Regional Health Authority,
Winchester

1 Alderson, M. R., *British Journal of Preventive and Social Medicine*. In press.
2 Alderson, M. R., *Gerontologica Clinica*, 1974, **16**, 76.

Loperamide and Ileostomy Output

SIR,—By chance we had to look again at the data collected during our double-blind trial of loperamide in ileostomy patients (21 June, p. 667). From this new analysis we learnt that the greater the ileostomy output the more benefit from loperamide treatment is obtained by the patient. This phenomenon is illustrated in the fig. The correlation ($r = 0{\cdot}850$, Spearman rank correlation coefficient) is highly significant ($P < 0{\cdot}0001$).



We feel that this new feature confirms our opinion that loperamide is effective in controlling excessive ileostomy losses of water and electrolytes.—I am, etc.,

G. N. TYTGAT

Wilhelmina Gasthuis,
Amsterdam

"Most Physicians Believe . . ."

SIR,—Your leading article on diabetic nephropathy (5 July, p. 5) states: "most physicians believe that these patients should receive the best possible diabetic control . . . but it remains disappointing that there is no scientific evidence of a reward for doing so."

In medical literature we are repeatedly told that "most physicians" or "most surgeons" or "most endocrinologists" believe this or that, as if to imply that the truth is revealed by consensus. We should remember that 20 years ago "most cardiologists" believed that all the victims of cardiac infarction should be kept bedfast for six weeks, "most gastroenterologists" believed that those with peptic ulcer should be treated by graduated diets, and "most physicians" who specialized in poisoning believed that analeptic drugs saved life.

When we read an article about treatment we wish to be told about its efficacy. If there is evidence that it is beneficial this should be reviewed; if there is no such evidence or if the evidence is conflicting this should be noted. In particular, the author should distinguish between treatment based on theory and treatment based on evidence, and this is what so many lamentably fail to do. As well as urging that both you and your contributors avoid the phrase "most physicians believe," may I also urge that you cease to refer to "scientific evidence"? For this implies that there can be non-scientific evidence.—I am, etc.,

JOHN W. TODD

Farnham, Surrey

Levodopa in Breast Cancer

SIR,—I should like to comment on the E.O.R.T.C. study by Dr. Engelsman and co-workers (28 June, p. 714) describing the apparent failure of levodopa therapy in an unselected group of patients with advanced breast cancer. It seems to me that levodopa has a specific if somewhat minor role in the management of breast cancer and it would be unfortunate if it was completely abandoned as a result of this study.

All the positive reports of the use of levodopa in breast cancer refer to its value in the relief of bone pain, and this response was used for selecting patients who subsequently responded to hypophysectomy.[1-3] In the E.O.R.T.C. study only six patients had bone metastases. The Westminster group[4] have provided evidence that only about 16% of breast cancers are dependent on prolactin alone and about a further 23% show prolactin dependence in conjunction with other hormones. Furthermore, prolactin dependence appears to be common in premenopausal patients.[4] Thus of the six patients with bone metastases, all of whom were postmenopausal, only one or two would have been expected to respond to levodopa. In my opinion a full trial of levodopa specifically in premenopausal patients with bone pain is required.—I am, etc.,

K. K. MAHAJAN

University Department of Surgery,
Newcastle upon Tyne

1 Minton, J. P., and Dickey, R. P., *Surgery, Gynecology and Obstetrics*, 1975. **136**, 971.
2 Minton, J. P., and Dickey, R. P., *New England Journal of Medicine*, 1972, **286**, 843.
3 Stoll, B. A., *Lancet*, 1972, **1**. 431.
4 Hobbs, J. R., *et al.*, *British Journal of Surgery*, 1974, **61**, 785.

Emigration of Doctors

SIR,—It is interesting that a member of the staff of the Liverpool School of Tropical Medicine should write (26 July, p. 229):