Salvation Army, through Colonel D. Anderson, 101 Queen Victoria Street, London E.C.1., the United Society for the Propagation of the Gospel, through Dr. Kathleen Wright, 15 Tufton Street, London SW1P 3QQ, and Oxfam through Dr. Tim Lusty, Upper Whitley Farm, Cumnor, Oxford. All will welcome inquiries.

Secondly, I obtained, as have others, much help, interest, and kindness in Dr. David Morley's department at the Institute of Tropical Child Health. They have a useful library of books, tapes, and slides, much expertise, and very useful contacts. A few days of orientation spent there is invaluable. Contact Miss Margaret Woodland, c/o the Institute of Child Health, 30 Guildford Street, London, WC1N 1EH, tel. 01-837 4825.—I am, etc.,

MICHAEL VAILE

Whitstable

Intratracheal Anaesthesia

SIR,—In your leading article on "Post-intubation Granuloma" (10 November, p. 313) it is stated that intratracheal anaesthesia was introduced in the 1930s.

It is generally held that the method was brought into this country in 1912 by the Liverpool surgeon Mr. R. E. Kelly.¹ When an anaesthetic clerk in 1917 I myself watched Mr. H. E. G. Boyle use the technique for a thyroid operation. By the end of the first world war, Dr. (now Sir Ivan) Magill and Dr. E. S. Rowbotham were regularly using naso- and orotracheal tubes for insufflation anaesthesia, thus enabling Sir Harold Gillies and his team to develop their splendid work on plastic surgery for facial wounds at the Queen's Hospital at Sidcup. By 1923 endotracheal anaesthesia was commonplace at Barts for suitable operations.

I would therefore suggest that it is hardly fair to ignore the sterling work carried out by the above-mentioned pioneers and others in this field.—I am, etc.,

C. LANGTON HEWER

London N.6

¹ Kelly, R. E., *British Medical Journal*, 1912, 3, 617.

SIR,—As an enthusiastic protagonist of endotracheal anaesthesia for some 50 years I must consider myself lucky not to have caused the post-intubation granuloma referred to in your leading article (10 November, p. 313). Nevertheless, considering the extensive use of the method in many branches of surgery it is perhaps timely that some notice should be taken of the occasional hazards associated with intubation of the larynx and trachea.

No doubt these hazards have diminished markedly since the introduction of muscular relaxants. But they do exist. The patient who suffers from a sore throat following an unnecessary intubation has just cause for complaint. In this context mention may be made of the effects on the tracheal mucosa of over-inflation of the cuffs now commonly used on endotracheal tubes. Judging from advertisements in the September issue of *Anesthesiology* there is apparently a demand for low-pressure cuffs in the U.S.A.

Instrumentation, including intubation, involves the risk of trauma, a risk which

should be justified by the requirements of the operation and not merely to ensure easy control of the patient's airway by the anaesthetist. It is sometimes forgotten that this can be achieved with a pharyngeal airway or even by manual support of the patient's jaw. I know one consultant anaesthetist who avoids intubation for ophthalmic operations and I know two surgeons who object to intubation for tonsillectomy in children. At the same time I have heard of an infant being intubated for circumcision. I have had the good fortune in the past to work with a famous surgeon who specialized in prostatectomy and never intubated one of his patients.

Has the old art of anaesthesia been lost entirely?—I am, etc.,

IVAN W. MAGILL

London W.1

Fall-out from Bomb Tests

SIR,—Dr. M. A. Denborough's letter (6 October, p. 46) illustrates some of the difficulties referred to in your leading article (8 September, p. 510) which make calculations of likely numbers of casualties from fall-out such an uncertain guide to thought and action.

First, the facts may be misunderstood. Radioactive isotopes may indeed dissipate their energy in a small volume of tissue, but this is automatically taken account of when estimating radiation doses: a millirad is a unit of energy absorption *per gramme*. Radiation exposures from atmospheric bomb tests are *not* qualitatively different in their effects from exposures to natural background, and this is in fact the premise for all calculations of expected numbers of cases of leukaemia and of thyroid cancer, etc., including those cited by Dr. Denborough.

Second, "the maximum published figures for radiation risks" were used in these calculations. If one wishes to ban something dangerous, is it more persuasive to use a maximum estimate or something more realistic?

Third, radiation effects are outside most people's experience so that an appeal has to be made to "authority." The only cited reference for the numbers of Australian casualties to be expected from French bomb tests was an Australian Parliamentary Paper,¹ which presents conclusions but no supporting bibliography of any kind. The reader has no way of checking the calculations, and internal evidence suggests that checking is needed—the risk coefficient used for leukaemia seems to have been about 100 cases per million per rad, rather out of line with what is generally accepted.

Fourth, the assumption of direct proportionality of known biological effects to radiation is not without difficulties for cancer induction. The Parliamentary Paper seems to use an average risk coefficient of around 100 cases of thyroid cancer per million persons per rad, which will have been derived from the experience of individuals receiving tens or hundreds of rads to the thyroid. If direct proportionality is correct, background radiation must be responsible for perhaps 10 cases per million per year. This is close to the natural death rate for thyroid cancer in the U.K. and thus seems much too high, even when allowance is made

for the very long survival after diagnosis of radiation-induced thyroid cancer in man.

Fifth, since there are no known qualitative differences between the risks from different kinds of radiations, the same method of risk calculation is as valid for medical uses of radiation as for bomb fall-out. If routine diagnostic exposures are made in the same way in Australia as in Britain so that the associated radiation dosage is similar (which must be roughly true) then the cancer risk in Australians for which Australian doctors are responsible will be 100 times greater than the cancer risk from the fall-out from all the French weapon tests. Why is this not a compelling argument against further use of medical radiation? Clearly either because we do not accept the risk calculations or, alternatively, because we believe that the benefits outweigh the risks. The arguments about bomb tests are not really about whether one or 10 or 100 cases of induced malignant disease are enough to justify their banning but about the magnitude of the benefits which they bring and to whom.

I hope no one will think that I am questioning Dr. Denborough's sincerity or that they can deduce what my personal views may be about bomb tests.—I am, etc.,

R. H. MOLE

Oxford

¹ *Biological Effects of Nuclear Explosion Fallout. Report of a Meeting between Australian and French Scientists*, Parliamentary Paper No. 29, Canberra, Government Printer of Australia, 1973.

Traveller's Ankle and Traveller's Back

SIR,—Traveller's ankle (Mr. H. Daintree Johnson, 14 July, p. 109, and 3 November, p. 300) and traveller's back would seem to stem from the same cause—sitting in a restricted space for long periods. Though many advertisements continue to inform us that seats for planes, trains, coaches, and cars are designed on orthopaedic principles, that influence ceases at knee level and the commercial need to pack occupants into a limited space takes over and is met by discounting the importance of structures below the knee in the correct sitting posture. Traveller's back may well start from the lack of below-knee comfort; it first starts in the buttock and progresses to such lumbar discomfort as finally to force the car driver and particularly his rear-seat passengers to leave their car to "stretch their legs." Motorway service areas must owe much of their custom to this need.

Some years ago I persuaded a friendly car designer to build a comfortable and solid support for my left foot at the left side of the clutch pedal and relate it to my seated position. This not only allowed exercise to my left foot, but intermittent pressure on it relieved my buttock discomfort and allowed a temporary change in the position of the lumbar spine, giving more comfort than anything previously tried.

I would be interested to know just how long the average healthy person can sit in the restricted space now generally provided and still maintain physical and mental efficiency. In this regard current and common industrial practice in air travel is in-

formative. For journeys of under eight hours tourist-class tickets are booked; for longer periods first-class travel is provided—not for the free drinks but for the greater leg room and seating comfort so that the company's representatives can, on arrival, meet their tasks with greater efficiency.—I am, etc.,

WILLIAM GISSANE

Road Injuries Research Group,
Birmingham Accident Hospital,
Birmingham

Euphoriant Elixirs

SIR,—A recent unpublished survey of more than 90 teaching and district general hospitals throughout the United Kingdom demonstrated considerable variation in the composition of "euphoriant (or Brompton) cocktails" used for the relief of pain and suffering in terminal cancer. Differences existed both in the active constituents and in the vehicle in which they were dissolved. Some included morphine, some diamorphine, the majority cocaine, while in others chlorpromazine or prochlorperazine was added. In a number of instances both morphine and diamorphine were included, though the pharmacological rationale for this is not easy to see. The variation in the vehicle was even greater—for example, the alcohol content ranged from 0 to 40% and was frequently replaced by gin, whisky, or brandy, while the syrup content varied from 0 to 50%, with honey often substituted for sucrose. The chloroform content was equally diverse.

In the light of this, the introduction of a standard diamorphine and cocaine elixir in the new edition of the *British Pharmaceutical Codex*,¹ which becomes operative on 1 December 1973, must be considered a step in the right direction. Yet information about its keeping properties is minimal—the *B.P.C.* simply states that it ought to be freshly prepared and, if chlorpromazine is added, it should be protected from the light.

At St. Christopher's Hospice we have studied the stability of diamorphine in an almost identical elixir.

	St. Christopher's	B.P.C.
Diamorphine hydrochloride	10 mg	10 mg
Cocaine hydrochloride	5 mg	10 mg
Ethyl alcohol	1.25 ml (95%)	1.25 ml (90%)
Syrup (B.P.)	2.5 ml	2.5 ml
Chloroform water to	10 ml	10 ml

We examined not only the effect of time but also the effect of light and temperature and of varying the alcohol and sugar content. Assay was by visual assessment of thin-layer chromatographic plates by two independent observers.² As preliminary assays indicated that the cocaine had no effect on the rate of degradation of diamorphine it was subsequently omitted from the mixture. After eight weeks at 22°C, 10% of the diamorphine had hydrolysed to *O*⁶-monoacetylmorphine (that is, 10% = 8 weeks). *O*³-Monoacetylmorphine and morphine were not detected. Storage at higher temperatures, 30° and 37°C, accelerated hydrolysis whereas at 4°C the rate was halved. There was no difference between replicates stored in the dark and those stored in diffuse light. The substitution of honey or an aldose sugar for sucrose increased the rate of hydrolysis. Doubling the concentration of alcohol resulted in a prolonged $t_{10\%}$, whereas halving it had the opposite effect. Further, and perhaps most important, the inclusion of chlorpromazine or prochlorperazine shortened the $t_{10\%}$ to two weeks. These results are summarized in the table. Finally, in an attempt to determine the effect of gastric acid and body temperature, diamorphine was added to simulated gastric fluid at 37°C.³ A greatly increased

Approximate $t_{10\%}$ of Various Diamorphine and Cocaine Elixirs

Elixir	Temperature (°C)	10% (Weeks)
Standard	22	8
"	4	>24
"	30	4
"	37	2
Double-strength alcohol (24%)	22	10
Half-strength alcohol (6%)	22	6
No syrup	22	10
Honey	22	6
Glucose	22	6
Prochlorperazine (1.25 mg/10 ml)	22	2
Chlorpromazine (6.25 mg/10 ml)*	22	2

*Half the recommended strength.

rate of hydrolysis was noted, but even so after four hours more than 75% of the diamorphine remained.

It is, of course important to remember that a 10% loss of diamorphine does not mean a 10% loss of analgesic potency. If *O*⁶-monoacetylmorphine is regarded as equipotent with morphine the loss of potency at the $t_{10\%}$ will be only 3%.⁴ For an elixir usually prescribed in quantities sufficient only for one or two weeks a shelf-life of this length is more than adequate. Even so, our results suggest it may be unwise to follow local tradition and, for example, exclude alcohol completely. Similarly, an increase in the syrup content to 50% might adversely affect the keeping properties of the elixir, which would be more marked were honey substituted for sucrose. The considerable hydrolysis-accelerating effect of phenothiazines must also be emphasized.

The present elixir represents the end point of an evolutionary process that began in the last century when morphine began to replace opium in medical practice. However, some patients find an elixir of this nature unpalatable owing either to its "sickly" taste or to its alcoholic "bite." Thus there would seem to be a good case for reviewing the need for such an elaborate vehicle for orally administered diamorphine. Therefore, while welcoming the attempt to standardize opiate-containing elixirs, we would suggest that there is need for further studies to determine the best formulation in terms of both shelf-life and patient acceptability. In addition, and perhaps even more fundamental, there is a need to evaluate objectively the contribution of the cocaine to the pharmacological effect of the mixture.—We are, etc.,

ROBERT G. TWYCCROSS

St. Christopher's Hospice,
London S.E.26.

R. A. GILHOOLEY

Medicinal Products Subdivision Laboratory of the
Government Chemist,
London S.E.1

¹ *British Pharmaceutical Codex*. London, Pharmaceutical Press, 1973.

² Twycross, R. G., and Gilhooley, R. A. In press.

³ *United States Pharmacopeia*, 18th Revision. Easton, Pa., Mack Publishing Company, 1970.

⁴ Twycross, R. G., *British Journal of Pharmacology*, 1972, 46, 554P.

Are PUFA Harmful?

SIR,—Your leading article under this title (6 October, p. 1) concluded with the words: "It is disturbing that we consume commercially processed foods without considering what they contain, how they are made, or what harm they may do."

Pertinent to this exclamation and to the subject which provoked it is the current vogue of so-called "coffee whiteners," which, as it happens, were featured in the issue of *Which?* for November 1973. These are convenient prepacked substitutes for milk or cream, generally containing glucose syrup solids and vegetable fat, which forms about 85% of the total, usually along with sodium caseinate. When these products are offered for sale in Britain the composition must, by law, be stated on the container. It happens that at least two of the principal British airlines dispense their whiteners in flight packets labelled "creamer," with no clue to the contents at all. To me, this verges on the positively misleading, though our British laws hardly impinge on this exercise because (1) the product is not actually sold and (2) it is doubtful whether the Food and Drug Act applies to aircraft in flight, frequently on overseas flights.

Nevertheless, this is surely a far from commendable practice and, if only for reasons of public health and welfare, it is regrettable that these corporations and organizations are not enlightened enough to see that what they do is as much in conformity with the meaning of the law as it is with statutory enforcement.—I am, etc.,

ROY GOULDING

New Cross Hospital,
London S.E.14

False Interpretation of Fetal Heart Monitoring

SIR,—Mr. I. L. Craft and others point out (29 September, p. 694) the possibility of a fetal scalp electrode erroneously recording the maternal heart rate. We would like to report an even more bizarre instance in which the fetal scalp electrode recorded the fetal and maternal heart rates additively.

The electrode was spiral and was made by Messrs. Rocket. The fetal heart rate was recorded continuously using a Sonicaid FM 2 machine. The recorded fetal heart rate apparently rose after 11 hours of recording from 150 beats/min to 220 beats/min.

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Contractions

However, auscultation of the fetal heart rate simultaneously, using a Sonicaid D 205 fetal pulse detector, showed that the fetal heart rate was in fact 140 beats/min. The maternal heart rate was 80 beats/min. Adjustment of the sensitivity control on the FM 2 machine had no effect on the artefactual recording.

We would advocate regular auscultation of the fetal heart when a fetal scalp electrode