portant to exclude inhalation of the object. A 4-year-old boy was brought to this department recently thought to have ingested a foreign body, "ingestion" being associated with a short bout of coughing. Clinically he showed no adverse effects and he was not x-rayed. Three days later his mother brought him back stating that he seemed unwell in himself and appeared to be breathless. X-rays showed a foreign body lodged in the right main bronchus.

May we therefore suggest the following indications for x-raying children suspected of ingesting foreign bodies when presenting in accident departments:

- (1) Respiratory symptoms at the time of ingestion.
- (2) Respiratory signs.
- (3) Gastrointestinal symptoms and signs.
- (4) The ingestion of foreign bodies which by virtue of size and/or shape might sensibly be thought to be harmful.
- (5) A history of one week or more since ingestion.—We are, etc.,

P. F. PEARSON
G. R. SMERDON

Basingstoke District Hospital,
Basingstoke, Berks

SIR,—I was disappointed to find that Mr. Lewis Spitz (20 November, p. 469) revives the old theory that slender foreign bodies such as sewing needles or Kirby grips should always be removed surgically because of the danger of perforation. I would be interested to know just how many disasters Mr. Spitz has seen when these foreign bodies are treated expectantly.

Sewing needles and Kirby hair-grips are, of course, the commonest of all ingested foreign bodies in childhood, for the obvious reason that children see their mothers holding them in their mouths, and in a long surgical career, when I have seen enumerable such cases, I have encountered only one tragedy. This was an operative death while a Kirby grip was being removed from the stomach. Since that time, 21 years ago, I have advised expectant treatment in every case I have seen and have never had cause to regret it. Sometimes these foreign bodies have taken six or more weeks to pass, but I have never known a Kirby grip not to get through. Perhaps I have been fortunate. Even if perforation does occur that is not the end of the world. I have removed two sewing needles and one long, pointed ivory stick from the pouch of Douglas, where they were lying quietly doing nobody any harm.

Before removing any foreign body I think there are two things one must consider. Firstly, whether complications may arise if the foreign body is left alone, and, secondly, whether the morbidity of removing the foreign body is going to be greater or less than that of leaving it where it is.—I am, etc.,

ROBIN BURKITT

Ashford Hospital,
Ashford, Middlesex

General Anaesthesia

SIR,—The article entitled "Results in 1,000 Cases of Therapeutic Abortion Managed by Vacuum Aspiration" (20 November, p. 477) contains the following statement: "General anaesthesia has in most cases been replaced by a regimen whereby after premedication with Omnopon 10 mg and scopolamine

0.2 mg the patient is treated in the operating theatre with intravenous diazepam 10 mg, intravenous pentazocine 30 mg, and methohexitone sodium 100 mg. . . . If the patient is disturbed by the operation, further methohexitone may be given. . . ."

It is to be hoped that this statement does not imply that the authors are under the misapprehension that the technique described does not constitute a general anaesthetic and, as such, demand the presence of an anaesthetist and the enforcement of all necessary safety precautions.

Later in this interesting communication, the following words appear: ". . . are dealt with by vacuum aspiration using either the anaesthetic technique described. . . ."

It would be helpful if the authors would clarify their opinion as to whether they do or do not consider the technique described to constitute a general anaesthetic.—I am, etc.,

M. P. COPLANS

London W.14

Virus-free Blood?

SIR,—Bernard Shaw, in his preface to *Three Plays for Puritans*, headed a chapter "Better than Shakespear?" Those who missed the question mark thought he was claiming too much. The heading "Virus-free Blood" of the letter from Dr. L. R. I. Baker and his colleagues (6 November, p. 360) surely needs a question mark also. Are nitrogen-frozen washed red cells less likely to transmit hepatitis as a result of being processed in this way? The careful work of Tullis and his colleagues¹ is cited, but what is Tullis's evidence?

In a meticulously carried out prospective study over a period of 53 months Tullis *et al.* observed no case of hepatitis in a test group of 89 patients who were transfused with a total of 623 units of thawed deglycerolized red cells resuspended in albumin; they observed, on the other hand, one case of icteric and three cases of anicteric hepatitis in a control group of 94 patients who were transfused with a total of 442 units of similarly treated red cells resuspended in the plasma from which they were originally separated. Some of the patients were readmitted to their respective treatment groups for more than one series of transfusions and they were then counted as separate recipients. As, however, some patients are insusceptible to serum hepatitis, it is perhaps arguable whether the same patient should be counted more than once, unless on his next admission he is placed in the opposite treatment group. However, if each admission to the same group is so counted, there were 110 recipients in the albumin group and 104 recipients in the plasma group, which gives the following 2×2 Table.

	No hepatitis	Hepatitis	
Albumin	110	0	110
Plasma	100	4	104
	210	4	214

By Fisher's exact method, using a two-sided test, $P=0.11$; the difference between the two groups could therefore quite easily have arisen by chance. Tullis *et al.* appear to have constructed a 2×2 Table from the

number of cases of hepatitis and the number of units transfused. Even so, Fisher's exact test gives a probability of only 0.06. Moreover, as set out below, there is a marked difference in the number of units transfused to each group:

	Recipients	Units	
Albumin	110	623	733
Plasma	104	442	546
	214	1065	1279

$$\chi^2_1 = 4.60, P < 0.05.$$

Although such a distribution biases the trial against the recipients of the cells resuspended in albumin (in whom no case of hepatitis occurred) in a random allocation such a disturbance (or a greater one) would occur by chance in only about 1 in 30 such trials. This difference is therefore statistically much more significant than the difference found in the incidence of hepatitis.

In addition to the evidence presented by Tullis *et al.*, Huggins² has stated that no case of hepatitis has been observed in the recipients of approximately 3,000 units of frozen blood, but clearly more data are needed. Too often in the past work that has appeared to have freed plasma from the risk of transmitting hepatitis has not been reproducible.³⁻⁷

The testing of donations for the presence of Australia antigen (and antibody) will reduce the risk of transmitting hepatitis. Whether this risk can be further reduced by the processes involved in the storage of red cells in liquid nitrogen remains to be established. Nevertheless, if blood is frozen for six months before use sufficient time will elapse for at least icteric hepatitis to become manifest in the donor if he should be incubating the disease at the time of his donation. Recently in Aberdeen two blood donors who, unknown to the transfusion service, had been tattooed not long before giving their donations have subsequently developed icteric hepatitis. Both donations when tested by immunoelectro-osmophoresis for Australia antigen and antibody gave negative reactions. When jaundice developed in one three months later he had become Australia antigen positive; the other, just reported to us, has yet to be retested.

I am grateful to Professor D. F. Kerridge for statistical advice.

—I am, etc.,

H. B. M. LEWIS

Regional Transfusion Centre,
Royal Infirmary,
Aberdeen

¹ Tullis, J. L., Hinman, J., Sproul, M. T., and Nickerson, R. J., *Journal of the American Medical Association*, 1970, **214**, 719.

² Huggins, C. E., *Monographs in the Surgical Sciences*, 1966, **3**, 133.

³ Blanchard, M. R., Stokes, J., Hampill, B., Wade, G. R., and Spizizen, J., *Journal of the American Medical Association*, 1948, **138**, 341.

⁴ Medical Research Council, *Lancet*, 1954, **1**, 1328.

⁵ Allen, J. G., Enerson, D. M., Barron, E. S. G., and Sykes, C., *Journal of the American Medical Association*, 1954, **154**, 103.

⁶ Savman, W. A., Gauld, R. L., Star, S. A., and Allen, J. G., *Journal of the American Medical Association*, 1958, **168**, 1735.

⁷ Redeker, A. G., Hopkins, C. E., Jackson, B., and Peck, P., *Transfusion*, 1968, **8**, 60.

Cause of Death

SIR,—In your leading article "Cause of Death" (20 November, p. 441) you quote from the Brodrick report: "the main aim of public policy should be to ensure that the