Correspondence

Letters to the Editor should not exceed 500 words.

Buccal Oxytocin

SIR,—Oxytocin can have a powerful effect upon the pregnant uterus, however it reaches the blood-stream. The intravenous "pitocin drip" was at first regarded by some as a dangerous technique; and so it was until experience had made plain the pitfalls and careful investigation had shown how these could be avoided. To-day it can only be accepted as safe and efficacious when used "under careful control in hospital practice" (19 September, p. 705). Even then the intravenous method has disadvantages for both the patient and her attendants. It behoves us therefore to continue to investigate other ways of administering the drug.

The buccal route has recently been shown to be an effective and convenient method. Just how safe it is compared with the intravenous method is not yet established. One thing, however, is quite clear; oxytocin should not be given buccally in circumstances which would preclude its use intravenously. This has been said over and over again by the manufacturers and by those who have most experience of the method. Nevertheless, judging by the case histories reported in your columns recently the lesson has not yet been learned. Up till the present I know of six uterine ruptures in association with the use of buccal oxytocin. And in four of these there were clear contraindications to the use of oxytocin by any route—namely, previous caesarean section for disproportion; hydrocephalus and disproportion; history of previous uterine sepsis, previous forceps delivery and manual removal of the placenta, and poor facilities for supervision; delivery in a "private hospital" apparently without adequate supervision or facilities.

In the other two cases the rupture occurred during labour in association with an abnormally rigid cervix several hours after the oxytocin had been discontinued, and may have had no direct relation to the method of induction at all.

I agree with Drs. J. A. Chalmers and J. L. Ng (24 November, p. 1070) that it would be a great pity if a potentially valuable method were to be abandoned prematurely and unnecessarily. Therefore, in view of our present relative lack of detailed information about the method, which will only gradually be overcome as experience accumulates, it is imperative not only in the interests of the method but also of our patients that buccal oxytocin should only be used where its effects can be recorded accurately and studied fully until it is certain that it is safe, and until the optimum dosage has been determined. In particular, I would agree with Professor MacGillivray,[1] that "a practitioner should only contemplate using buccal oxytocin in its present form if he can devote as much time and care to the patient as he would if she were on a continuous intravenous infusion and if he is prepared to do a caesarean section should some abnormality develop."—I am, etc.,

J. H. M. PINKERTON.
Department of Midwifery and Gynaecology, The Queen's University of Belfast, Belfast 12.

REFERENCE
[1] MacGillivray, I., *Practitioner*, 1964, **193**, 415.

Urinary Infection in Childhood

SIR,—There seems good reason to believe that much of chronic pyelonephritis has its origin in childhood even although it may not reveal itself until adult life. The paper by Dr. Jean M. Smellie and her collaborators (14 November, p. 1222) is valuable in focusing attention on this.

There are many variables to be considered in examination of the urine of children as a diagnostic measure in suspected urinary tract infection, and these have not been fully evaluated. Dr. Smellie and her co-authors state, "In our experience, laboratory confirmation of the diagnosis rarely presents difficulty provided a fresh clean specimen of urine is collected before treatment is started," and again, "in young children urine may be collected into adhesive plastic bags . . . [and] . . . plated within two hours of voiding." Our experience has led us to be less confident about the bacteriological diagnosis of urinary tract infection in young children based on such methods of collection. We have found that under 2 years of age bacteriological examination of urine collected into plastic adhesive bags or as "clean catch" samples of mid-stream urine can be quite misleading. From equal numbers of randomly selected, apparently normal, male and female children under 2 years of age who had no suggestion of urinary tract infection we have examined 218 specimens of urine collected into adhesive plastic bags and 190 mid-stream (clean catch) specimens. These were collected from different patients and all were examined and plated for culture within half an hour of voiding, or, where delay in bacteriological examination was unavoidable, were immediately refrigerated. A significant growth of potentially pathogenic organisms (10^5 organisms per ml. or more) was found in 48% of the bag-urine specimens and in 33% of the mid-stream urines. Only 20% of bag urines and 30% of mid-stream urines showed no growth. There was little difference between males and females. Further, no matter how rapidly the urine was examined bacteriologically after voiding into the bag similar bacterial counts were obtained, indicating that the initial inoculum of contaminant organisms from the skin and perineum was very heavy. Both with plastic bags and with mid-stream specimens cleansing of the perineum and genitalia with sterile water did not significantly reduce the bacterial count.

Forty-five per cent. of Dr. Smellie and her co-workers' cases of urinary infection were under the age of 1 year. The diagnosis in these cases, in so far as it was based on bacterial counts on urine collected in plastic bags, or by mid-stream specimens, might be in some doubt. It is our impression that contamination can be so gross at this age period that bacterial counts on non-catheter specimens of urine can often lead to conclusions of doubtful validity.—We are, etc.,

H. BRAUDE.
J. O. FORFAR.
J. C. GOULD.
Western General Hospital, Edinburgh, and Department of Child Life and Health, University of Edinburgh.

Systemic Embolism in Mitral Stenosis

SIR,—Systemic embolism is a major hazard in patients with rheumatic heart disease, and successful mitral valvotomy reduces this risk. We feel embolism should be regarded as only one aspect of the assessment of patients for surgery. The recent articles on systemic embolism and mitral valve disease (7 November, pp. 1167 and 1169) have prompted this brief account of our experience with some comments.

From 1953 to 1961, 850 patients with mitral valve disease were seen in this centre, and 533 had a closed mitral valvotomy. Analysis of these cases showed:

(1) The pre-operative incidence of systemic embolism was 20%, increasing to 24% in patients over 40, and 31% in patients with atrial fibrillation.

(2) Operative systemic emboli occurred in 5.8% of patients; 3.5% at finger-split and 6.5% at instrumental valvotomy. Operative embolism occurred in 16% of patients with both valve calcification and atrial fibrillation compared to 1.5% in patients with sinus rhythm and no calcification.

(3) The operative mortality was 3% for finger-split valvotomy, and 9% for instrumental valvotomy, increasing with worsening disability.

(4) An average follow-up of 4½ years for 445 patients revealed a 5.6% post-operative embolic incidence; 3.7% with sinus rhythm; 12.3% with atrial fibrillation. 21 of 25 embolic incidents occurred in patients after an atrial appendagectomy.

Our patients fall into four main groups:

Group I.—Mitral stenosis judged operable with symptoms. In these cases we advise operation independently of systemic embolism.

Group II.—Mitral stenosis, judged operable, minimal or no symptoms. With sinus rhythm, no operation should be undertaken. With atrial fibrillation, no symptoms, and the patient below 35 years decision is more difficult. We generally recommend surgery, and certainly over 35 operation is advisable as the embolic risk is high.