It shows that over 13% of cycles may be less than 24 days in duration and over 22% are 31 days or longer. These data should be kept in mind when considering the effects of exogenous oestrogens and progestogens on the menstrual cycle.—I am, etc.,

Speen,
Nr. Aylesbury, Bucks.

H. J. E. COX.

REFERENCES

- 1 Haman, J. O., *Amer. J. Obstet. Gynec.*, 1942, **43**, 870.
- 2 Pincus, G., *The Control of Fertility*, 1965. London.
- 3 Cox, H. J. E., *Bull. Kandang Kerbau Hospital, Singapore*, 1965, **4**, (2), 90.

Jaundice of Pregnancy

SIR,—Your leading article "Jaundice of Pregnancy" (2 December 1967, p. 499) should stimulate further interest in this important subject, and especially in the fascinating condition of recurrent jaundice. You stress that the prognosis in recurrent jaundice is excellent, and Dr. J. E. A. Oni-Orisan writes subsequently (16 December 1967, p. 683) to re-emphasize the benign nature of this condition for mother and child. I write now to question this.

I recall one of my former patients who was jaundiced towards the end of five successive pregnancies. In the second and fourth pregnancies the child died in utero near to term, and a day or two before labour began spontaneously; in the first and third pregnancies the child was born alive at about 38 weeks; and in the fifth pregnancy labour was induced at about 37 weeks and a live child resulted. I have come across another report, by Moore,¹ in which he refers to one patient who had stillbirths in association with jaundice in her first and third pregnancies; the second and fourth babies were born alive and well, although the mother was again jaundiced.

Your correspondence columns often bring to light interesting information, and it could be that others of your readers have known of cases of recurrent jaundice in which the disease was less benign than has been believed. Incidentally, from my own experience I would agree with your leading article that pruritus disappears quickly and the jaundice more slowly, and not in the reverse order as Dr. Oni-Orisan suggests.—I am, etc.,

C. J. DEWHURST.

Institute of Obstetrics and
Gynaecology,
(University of London),
Queen Charlotte's Maternity Hospital,
London W.6.

REFERENCE

- 1 Moore, H. C., *Lancet*, 1963, **2**, 57.

Personal Viewpoint on Anaesthesia

SIR,—In reviewing my book *Studies in Anaesthetics, Including Intravenous Dental Anaesthesia*, Dr. P. W. Thompson (13 January, p. 106) criticizes my dosage of methohexitone as being somewhat high and takes me to task for not suggesting how long the resulting respiratory depression or brief period of apnoea should be allowed to last before needing treatment. May I say that I have used this dosage in more than 2,500 patients, and in not a single case has respiratory depression or apnoea been other than momentary or needed any treatment whatsoever?

He also criticizes me for saying that in treating minor degrees of laryngeal spasm during extraction of teeth the use of a muscle relaxant is absolutely contraindicated, but he does not give my reasons for saying so. I think that few, if any, anaesthetists with experience of extraction work in outpatients would disagree with those reasons.—I am, etc.,

Salisbury, Wilts.

J. G. BOURNE.

Congenital Dislocation of the Hip

SIR,—I am grateful to Mr. G. C. Slee (6 January, p. 57) for calling my attention to a more recent description by Ortolani¹ of his C.D.H. test. My observations on Ortolani's test were based on his original description in 1937.² This was the reference quoted by Barlow³ and by your leading article (18 November, 1967, p. 371). From Mr. Slee's description it is evident that Ortolani subsequently modified his test, and advised that the hip should be examined by direct palpation with the thumb in front and the index finger behind.

Mr. Slee is right when he says that many people lack confidence in testing for C.D.H. because of difficulties in interpreting "clicks." I should like to repeat my plea for the abolition of the word "click" in this context. Ortolani did not use the Italian equivalent of this word—he called it a "jerk." Barlow emphasized that clicks were often not elicited, and described the femoral head as slipping in and out of the acetabulum.

"Click" is defined in the *Shorter Oxford English Dictionary* as "a slight, sharp, hard, non-ringing sound of concussion." Confusion will persist so long as we continue to use a word implying a sound for a test which depends essentially on palpation. Moreover, the confusion is not likely to be resolved by your leader writer's use of the hybrid term "palpable click."

Testing the newborn hip is a major advance in preventive medicine, and it is essential that the test be properly taught and clearly understood by all concerned. Unfortunately a certain mystique has become centred around the elicitation of the "click." In my opinion this is largely due to the fact that this expression is altogether inappropriate to apply to the Ortolani test or any of its modifications.—I am, etc.,

Bridgend General Hospital,
Bridgend, Glam.

A. W. FOWLER.

REFERENCES

- 1 Ortolani, M., *La Lussazione Congenita dell'Anca*, 1948. Cappelli, Bologna.
- 2 ——— *Pediatrics (Napoli)*, 1937, **45**, 129.
- 3 Barlow, T. G., *J. Bone Jt Surg.*, 1962, **44B**, 292.

Haematemesis

SIR,—I have read with interest the series of articles on diseases of the digestive system.

Concerning the treatment of bleeding from the upper gastrointestinal tract (23 December, p. 723), mention is made of the fact that usually gastrointestinal bleeding stops within 12 to 24 hours of admission and that blood may be given over this time, a reasonable criterion for transfusion being a systolic blood pressure of 110 mm. Hg or less and pulse rate of 110 or more.

On two occasions I have seen patients who on admission to hospital have had blood pressures and pulse rates within normal limits who subsequently "collapsed" and in whom the putting up of a drip even by a cut-down proved exceedingly difficult. Is there not a case for setting up a slow-running infusion of Hartmann's solution on all patients admitted with a history of upper gastrointestinal tract bleeding to cover this early danger period and allow an easy rapid transfusion of blood if required?—I am, etc.,

Royal Infirmary,
Edinburgh 3.

Q. L. A. ROBINSON.

Localization of Intracranial Tumours

SIR,—I was interested to read Mr. C. A. Gleadhill's letter (6 January, p. 53). Unfortunately lumbar punctures are done in some hospitals in cases of suspected brain tumours, and, what is even more disturbing, Queckenstedt's test may be performed into the bargain, thereby increasing the risk of disaster. Of course Queckenstedt's test should never be performed as a routine, but only when it is indicated—that is to say, where a spinal block is suspected—and it should then be done carefully, with the neck flexed and extended if necessary.—I am, etc.,

W. REX MORRIS.

Newcastle Regional Hospital
Board,
Newcastle upon Tyne 6.

Carcinogenicity of Tobacco and Tobacco Smoke

SIR,—I regret that I missed Dr. M. Glass's letter on carcinogenicity of tobacco and tobacco smoke (5 August, 1967, p. 373) and the reply from Dr. F. J. C. Roe and Mr. A. L. Levene (2 September 1967, p. 615). Dr. Glass pointed out that millions of tobacco-stained fingers have been continually contaminated with tobacco condensates and tar, but that not a single instance of papilloma or carcinoma caused by this has ever been seen or reported. He writes: "Does not this argue for some other essential factor, besides cigarette smoke, for the evolution of malignant disease in any particular heavy cigarette smoker?" Dr. Roe and Mr. Levene explain the discrepancy by saying that the keratin layer of human skin is thicker than that of mouse skin, and therefore human skin does not develop skin cancer as readily as mouse skin.

In fact Dr. Glass's observation proves one thing only: that the tobacco tar which contaminates the fingers of smokers is not carcinogenic to the skin of the human finger. It follows that the substance in tobacco smoke which causes or helps to cause lung cancer is *probably* not the same as the substance in coal tar which produces skin cancer in man. Incidentally neither of these substances has been certainly identified, and for ethical reasons is not likely to be identified. The statement by Dr. Roe and Mr. Levene that human keratin is thicker than mouse keratin is probably irrelevant, since it has been shown (particularly by Ghadially's brilliant work⁴) that molluscum sebaceum (or keratoacanthoma), much the commonest form