or hypertension or both." There is no way of knowing which of these patients were actually suffering from pre-eclampsia as opposed to essential hypertension or chronic renal disease. McCartney² has shown by electron microscopic study of renal biopsies that the clinical diagnosis of pre-eclampsia is incorrect in primigravidae in 30% and in multiparae in 86% of cases.

Drs. Wardle and Menon refer to cryofibrinogen as "... the early breakdown product of fibrinogen." Although the identity of this substance is not entirely clear, it is more likely to represent an aggregation of fibrin monomer, perhaps in association with molecular fragments of fibrinogen, and as such represents critical evidence of the action of thrombin on fibrinogen. They also state that "toxaemia is increased when there is uterine distension ... [for example, in] accidental haemorrhage." Undoubtedly they intend to imply that toxaemia is increased in incidence rather than severity. However, an association of "toxaemia" and accidental haemorrhage is seriously questioned. Hibbard,³ for example, has produced figures to show that accidental haemorrhage is no more frequent in pregnancies complicated by hypertensive disorders than in normal pregnancies. However, accidental haemorrhage frequently produces severe disseminated intravascular coagulation, often with proteinuria, and, in the absence of hypotension, can resemble pre-eclampsia.

Finally they state that "inhibition of the fibrinolytic system alone never causes fibrin deposition." It is usually unwise to use the word "never" in medicine, and there are many authorities who believe that coagulation is a continuous process in the normal individual, nicely balanced by appropriate fibrinolysis. If this is so, it is conceivable that profound inhibition of the fibrinolytic system could lead to unopposed and progressive intravascular coagulation.

The question of changes in coagulation/fibrinolysis in pregnancy and their relationship to the development of pre-eclampsia is a fascinating one. Further study, preceded by careful selection of cases, may well establish a definite relationship between aberrations in the coagulation and fibrinolytic systems and "toxaemia of pregnancy."—I am, etc.,

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- ¹ Coopland, A. T., *Canadian Medical Association Journal*, 1969, **100**, 121.
- ² McCartney, C. P., *Circulation*, 1964, **30**, Suppl. No. 2, p. 37.
- ³ Hibbard, B. M., and Hibbard, E. D., *British Medical Journal*, 1963, **2**, 1430.

Unnecessary X-rays?

SIR,—For several years, in fact nearly 20, both as a casualty officer and as a country general practitioner with access to x-ray facilities, I have felt that x-rays for traumatic injuries are requested far too often, and often only for legal reasons or because it is customary. This is not a complaint, particularly against my local colleagues and friends, but seems indicative of a slightly woolly approach many people have about fractures. Instead of asking themselves the rather obvious question, is this x-ray going to serve

any purpose in the event that it does or does not show a fracture? will this affect or alter the treatment? they request an x-ray at once.

I have three questions that I ask before requesting an x-ray: (1) If night has intervened before I have seen the patient, has sleep been disturbed by pain? I must admit that I have never seen somebody sleeping easily or without analgesics when he has a bony injury. (2) If the x-ray reveals a fracture will it alter my approach on treatment? And (3) If longitudinal pressure applied at both ends of the bone causes referred pain or not. I find this rule is invaluable if fractures of ribs are suspected. Pressure on the sternum simultaneously with pressure on the dorsal vertebrae will certainly produce a referred pain if there is fracture of a rib.

I am sure many other doctors also apply these kinds of criteria, and I have no desire to denigrate those who request x-ray examinations for every injury, but I feel that the work load on x-ray departments is enough without unnecessary demands being made, and our patients should not be subjected to potentially harmful procedures if avoidable.—I am, etc.,

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Vaccination against Whooping-cough

SIR,—You state in your leading article (8 November, p. 316) that the "vaccines used before 1967 have now been found to be inefficacious in protecting home contacts with *B. pertussis* strains of type 1, 3. . . ." Can this statement be justifiably applied to the Wellcome product on the evidence contained in the recently published report of the Public Health Service Whooping-cough Committee and Working Party (8 November, p. 329)? The report shows that only 9 of the 120 exposed fully immunized contacts reported to have had whooping-cough had received the Wellcome vaccine. The diagnosis rested on the report of "a member of the nursing or sometimes the medical staff of the health department" who visited the home about three weeks after the source patient had been reported and "obtained details of any further patients with paroxysmal coughs. The diagnosis of whooping-cough in these nine children (eight were under 5 years of age) appeared to be based solely on a nurse's (sometimes the doctor's) assessment of whether they had a paroxysmal cough. Although the paroxysmal coughs were bacteriologically shown to be due to *Bord. pertussis* in two-thirds of all exposed contacts in the survey no information is given on the bacteriological confirmation of the diagnosis in any of the nine children immunized with Wellcome vaccine. If the diagnosis of whooping-cough was wrongly made in only one of these nine children then the efficacy of the Wellcome vaccine was underestimated by a factor of 8%.

In view of the fact that the Wellcome vaccine has been marketed at a potency of not less than four international potency units per dose since 1964 in order to meet W.H.O. requirement, while until 1966 the British requirements were 2:1 international potency units, is it justifiable to use such flimsy evidence to cause such serious doubt on the efficacy of Wellcome vaccine? Your leading

article concedes that there is a difference between pre-1968 and current vaccines but does not concede that there were differences between the vaccines used before 1968 even though their method of manufacture and reactogenicity¹ differed.

In the absence of bacterial confirmation the diagnosis of whooping-cough is uncertain, to say the least of it, and the numbers involved are too small to provide a statistically significant comparison of the efficacy of the Wellcome vaccine against the other vaccines used in the trial.—I am, etc.,

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REFERENCE

- ¹ Haire, M., Dane, D. S., and Dick, G., *Medical Officer*, 1967, **117**, 55.

Anaesthesia in the Dark

SIR,—Whether or not anaesthesia in the dark is justifiable (15 November, p. 383) I would suggest as a result of personal experience in 210 myelographies that the most efficient monitoring system is a simple stethoscope with long tubing. One limb is connected to an Ayre's T-piece inserted into the respiratory circuit; the other to a diaphragm on the chest. A stretched rubber diaphragm made out of the finger of a surgical glove gives the best acoustic results. The listening-in, of course, must be continuous while the lights are out.

For blood pressure recording, the most satisfactory illumination has been the replacement of the metal dial in a 7 in. (17.8 cm.) Accoson aneroid manometer by a translucent perspex dial and the insertion behind the dial of two semicircular Beta lights (obtained from Saunders-Roe and Nuclear Enterprises Ltd.). The figures on the dial should be 2 mm. thick, 1 cm. high, and placed at intervals equivalent to 20 mm. Hg.—I am, etc.,

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Voluntary Sterilization

SIR,—Despite the excellent literature distributed by the Simon Population Trust it would seem from the Union's experience that some practitioners still do not appreciate that a man may remain fertile for several weeks or even months after vasoligation. The patient should continue to take contraceptive precautions until two consecutive negative sperm tests have been obtained. Neglect of this precaution may lead to unwanted pregnancies and possible litigation. According to the Simon Population Trust the seminal fluid should be examined not less than 12 weeks after the operation, and a further examination should be carried out about three weeks later. The patient should not be deemed to be sterile until two consecutive examinations have shown complete absence of spermatozoa. It cannot yet be stated with complete finality that spermatozoa will not reappear in the seminal fluid after two consecutive and negative sperm tests. But the chances of this happening, if they exist, are