portion of these problems would be suitable for management by a GP obstetrician and, secondly, whether there is evidence to suggest that the labouring mother and her child fare better if a junior hospital doctor is the doctor of first contact rather than a general practitioner obstetrician. It is also regrettable that some of the papers quoted in the article were almost 10 years out of date, which with the rapid changes in obstetric practice in that time renders them of doubtful validity.

It may well be that most general practitioners will eventually opt out of intrapartum care, but I feel that it is important that those who wish to maintain and develop their obstetric experience should be given the opportunity to continue to manage selected patients unless evidence is produced showing that women are thus put at a greater risk. There is no such evidence as yet.

Given good will on both sides the system can work well; indeed, with the help and support of the consultant obstetricians at the Queen Mother's Hospital in Glasgow I have been delivering selected patients from the practice for the past two years. It is not difficult to become familiar with the techniques of attaching scalp electrodes, assessing the relevance of fetal heart variations, and other procedures associated with modern intrapartum monitoring. I have been present at almost all deliveries and in the vast majority have not needed to seek specialist help, although I was happy to know that it was always readily available if required. Given the support of one's partners it is quite possible in a group practice to make suitable administrative arrangements to allow one partner to supervise a large proportion of the labours in the practice.

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The intrauterine device and ectopic pregnancy

SIR,—In your leading article on contraceptive dynamism (11 December, p 1405) you draw attention to the association of the intrauterine contraceptive device (IUCD) and ectopic pregnancy. Recent published series of ectopic pregnancies treated in Britain report between 8%¹ and 25%² of patients at operation having IUCDs in situ.

Between 1966 and 1972 the numbers of ectopic pregnancies seen in Britain each year have risen from two and a half thousand to three and a half thousand. Part of this rise is accounted for by the increasing use of IUCDs. The influence that the device has on the frequency of ectopic pregnancy obviously depends on what proportion of the population uses IUCDs. But it will also be affected by the type of device used. Different generations of devices have different pregnancy rates and differing percentages of these pregnancies which are ectopic. The "prehistoric" Grafenberg ring has a rate of 5.4% ectopic pregnancies.³ For the first-generation devices such as the Lippes Loop, Dalkon Shield, and Saf-T-Coil the percentage of ectopic pregnancies is about 5%⁴⁻⁷; for the second generation (Copper-7, Copper-T) about 2%.⁸ Figures for the third generation of medicated devices show one-year pregnancy rates comparable to those of second-generation devices (1.2-2.4 per 100 women), but the proportion of these that are ectopic seems if anything higher.

Knowing the pregnancy rates and the proportion of these pregnancies that are ectopic we can estimate the number of ectopic pregnancies per million users of IUCDs per year. There is approximately four times the risk of ectopic pregnancy if first-generation devices (one-year pregnancy rate 1.8-5.6 per 100 women) are used instead of second-generation devices.

There are an estimated 15 million users of IUCDs world-wide. If all the devices in use were of the first generation then 20 000 of these women would have ectopic pregnancies each year. This figure would be reduced to 5 000 if all the devices in use were of the second generation. The conclusion would seem to be that to reduce the risk of ectopic pregnancy first-generation devices should be phased out in favour of second-generation devices.

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¹ Weekes, A R L, and Hutchins, C J, *British Journal of Clinical Practice*, 1976, **30**, 104.

² Burke, M, and Buck, P, *British Journal of Hospital Medicine*, 1976, **15**, 552.

³ Hill, A M, *American Journal of Obstetrics and Gynecology*, 1969, **103**, 200.

⁴ Tietze, C, *British Medical Journal*, 1966, **2**, 302.

⁵ Lehfelt, H, *American Journal of Obstetrics and Gynecology*, 1970, **108**, 1005.

⁶ Alvi, T T, *Obstetrics and Gynecology*, 1974, **41**, 894.

⁷ Steven, J D, *Journal of Obstetrics and Gynaecology of the British Commonwealth*, 1974, **81**, 282.

⁸ Stuart, W C, *Proceedings of the Third International Conference on Intrauterine Contraception*, p 151. Amsterdam, Elsevier, 1975.

Postcoital contraception

SIR,—I am not sure whose side Dr D J Hill (25 December, p 1562) is on, but I quite agree with him that the "legal and ethical problems involved . . . ought to be faced but are usually avoided." As he says, "there is a great difference between contraception . . . and the use of an abortifacient after fertilisation has occurred."

Dr Hill is kind enough to refer to my recent paper "Mortal coils"¹ in support of his argument, but he appears to have missed the main point of it, which is that quite apart from its postcoital use, the intrauterine "contraceptive" device works, in most cases, by aborting very early pregnancies² and its use as a method of birth control at any time contravenes section 58 of the Offences Against the Person Act 1861 unless the bureaucratic and other procedures of the 1967 Abortion Act are complied with.

If I may quote myself, "The coil hangs over the present Parliamentary Select Committee like a sword of Damocles. It cannot expect the profession to accept grave and serious restrictions of the 1967 Act on the grounds that abortion is a kind of murder while at the same time remaining silent about the uncounted thousands of potential human beings slaughtered each day by a widely accepted birth control device. Those who would curtail abortion must also curtail the coil. Those who wish the coil to be freely available without legal hindrance must wish the same for abortion. To think otherwise is to be illogical and inconsistent—or simply dishonest."

I have put this point to several MPs, including Mr Leo Abse. All of them have evaded it. May I once again ask any member of Parliament—or of the profession—to explain why it is apparently permissible to destroy a fetus at two hours or two days but not at two weeks or two months? And if it is permissible, what is the legal and moral basis for the

distinction? I do hope that Damocles will not disappoint us.

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¹ Brewer, C, *World Medicine*, 1976, **11**, No 17, p 33.

² Duncan, G W, and Wheeler, R G, *Biology of Reproduction*, 1975, **12**, 143.

Smallpox vaccination for nurses?

SIR,—A few days ago a girl who had applied for admission to the nursing school of a general hospital near London came to see me for a preliminary medical examination. With her other forms she presented one stating that she would not be admitted to the school unless she had been vaccinated against smallpox. There was not a word about vaccination against diphtheria, typhoid, poliomyelitis, tuberculosis, rubella, or measles, diseases which are still to be found in these islands. I think I am correct when I say that there are only two small areas of smallpox left in the world and neither of them includes London.

Primary smallpox vaccination in a young adult can be very unpleasant and there is a small risk of major side effects. If the girl was going abroad to an underdeveloped area I would advise her to take such a risk, but surely this is an unnecessary precaution for nursing duties in England. Could it be that the form she presented is one automatically sent to every applicant because of a rule which has now outlived its usefulness?

Before we condemn the hospital in question maybe we should all examine our own routines and ask ourselves how many of them have become out of date, pointless, or even dangerous?

JOHN F FLEETWOOD

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"Dear Doctor . . ."

SIR,—As a new general practitioner and hence recently from the hospital side of the profession I was appalled to see how poorly hospital medical staff communicate with GPs. I therefore examined the weekly mail of an average general practice in the Midlands to see how many letters explained to the GP what the patient had been told by the hospital medical staff. Of 114 discharge letters or hospital outpatient communications only 12% told the GP what had been said to the patient about his illness and in 2% of the total there was no mention of what had been done to the patient. One does not know the function of these latter communications. A fine example received read, "This patient was seen in outpatients today and will be sent for in three months' time."

It is so embarrassing and irritating to face a patient recently seen in hospital who comes to the GP for clarification of his understanding of his condition when necessary information has not been passed on from the hospital to the GP. No doubt on occasion GPs are very bad at communication too—I remember seeing patients in the casualty department with "Please diagnose and advise" written on an EC10—but usually in these cases the patient can describe his symptoms, and the fault on the GPs part is more one of gross bad manners.

I am sure this communication problem has been aired in these columns before but it seems to have done little good.

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Prevalence of primary and secondary hypertension

SIR,—We have read with interest the report of Dr Göran Berglund and others (4 September, p 554). While agreeing with the sentiments expressed in this article concerning hypertensive men in the middle age group, we believe that the methods employed in the investigation and the data from which the conclusions were drawn are open to question.

(1) Screening for renal artery stenosis was performed on 287 men, with four cases diagnosed, giving a prevalence of 1.4% in the screened group, not 0.6% as stated.

(2) Despite the statement that "renography as a screening instrument for renovascular hypertension cannot be recommended" this technique was used as the primary means of investigation for renal artery disease. Isotope renography at best diagnoses only 85% of these lesions.¹ Thus in a group of 689 patients up to 103 may have false-negative results, or in 287 patients 43 cases may be missed. While not suggesting that these figures are likely, we wish to illustrate the possible statistical error in the methods used.

(3) We agree, although not from the data presented by the authors, that population screening for renal artery disease is unwarranted² but agree with McNeil and Adelstein³ that "there is some potential benefit to the identification and surgical treatment of patients with renovascular disease. . . . The patients who appear most likely to benefit from surgical treatment are those not likely to follow a medical regimen." Surgical management also is of value to patients unresponsive to a medical regimen and to those with severe stenosis or severe hypertension.⁴ When reviewing statistical data one must not ignore the potential benefits of a positive diagnosis and effective management to the individual patient.

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¹ McNeil, B J, et al, *New England Journal of Medicine*, 1975, **293**, 216.

² Ferguson, R K, *Annals of Internal Medicine*, 1975, **82**, 761.

³ McNeil, B J, and Adelstein, S J, *New England Journal of Medicine*, 1975, **293**, 221.

⁴ Hunt, J C, and Strong, C G, *American Journal of Cardiology*, 1973, **32**, 562.

Plasma cyclic AMP response to glucagon in liver disease

SIR,—We read with great interest the paper by Dr T F Davies and others (17 April, p 931) on the role of cyclic adenosine-3', 5'-monophosphate (cAMP) in differentiating surgical from non-surgical jaundice.

We have had the opportunity to study the levels of cAMP in newborn infants with abnormally high plasma bilirubin concentrations. These levels proved to be unreasonably high and considerably greater than those of non-jaundiced newborns.¹ The method we

used was that of Gilman,² a competitive protein-binding method similar to the method used by Dr Davies and his colleagues. We have further investigated the effect of bilirubin on protein kinase activity³ and found that bilirubin inhibits the binding of cAMP to protein kinase. This inhibitory effect of bilirubin has been proved to be competitive.

To comment on the work of Dr Davies and his colleagues, we wonder if the high basal values of plasma cAMP and the abnormal response to glucagon reported were the results of increased plasma bilirubin values. Table I of their paper shows that all their patients had abnormally high levels of bilirubin. We have found (unpublished data) that high values of cAMP are constantly found in the plasma of patients with high levels of bilirubin. When the bilirubin has been extracted with ether, however, levels of cAMP are found which are within normal limits (see figure).

Levels of plasma cAMP of full-term and premature newborn infants with different concentrations of bilirubin.

Conversion: SI to traditional units—cAMP: 1 nmol/l $\approx$ 0.03 μ g/100 ml. Bilirubin: 1 μ mol/l $\approx$ 0.058 mg/ml.

In conclusion we would suggest that further investigations must be carried out on the patients studied by Dr Davies and his colleagues before the abnormal cAMP response to glucagon can be established as a diagnostic test in differentiating surgical and non-surgical jaundice.

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¹ Constantopoulos, A, Karaboula, K, and Matsaniotis, N, *Archives of Disease in Childhood*, In press.

² Gilman, A G, *Proceedings of the National Academy of Sciences of the USA*, 1970, **67**, 305.

³ Constantopoulos, A, and Matsaniotis, N, *Cytobios*, In press.

Treatment of axillary hyperhidrosis

SIR,—Further to Dr I B Sneddon's letter (11 December, p 1447) I would like to confirm the efficacy of 20% aluminium chloride hexahydrate solution as a treatment for axillary hyperhidrosis. There are at least 10 patients at the Liverpool Royal Infirmary who have benefited from this application following failure of other standard methods.

However, these patients were all successfully

treated without the need for polyethylene occlusion. Twice-daily topical application for one week decreasing to once or twice weekly has produced satisfactory results in all patients to date. A primary irritant eruption is a common complaint in the first days, but this does not usually recur following a 48-h break in therapy.

G A R PRICE

Brecon, Powys

Tetracyclines in the elderly

SIR,—With reference to your expert's answer to the question on tetracyclines in geriatrics (4 December, p 1378) am I wrong in believing that the physiological deterioration of renal function in the elderly makes the tetracyclines—with the exception of doxycycline—undesirable if not contraindicated in a geriatric ward?

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* * * Our expert writes: "Your correspondent is right in that doxycycline is the only tetracycline which does not raise the blood urea and may be safely given to those with renal failure. Other tetracyclines cause elevation of the blood urea level by increasing the rate of breakdown of protein. In a few rather rare cases, usually associated with degraded tetracycline, a Fanconi-like syndrome has been noted. Although many old people have decreased renal function, they do not all have incipient renal failure and it would be going too far to say that doxycycline is the only safe tetracycline for the elderly. Most physicians will have had experience of the widespread safe use of tetracycline, particularly in the early 1960s, but the point made by your correspondent is, in general, valid."—Ed, *BMJ*.

Microbiological samples from the vulva

SIR,—The polystyrene foam sponge used by Professor W Brumfitt and his colleagues in their study of the vulval flora (18 December, p 1471) is undoubtedly an excellent microbiological sampling material. Using a similar method for specimen collecting in female genital infections my colleagues and I found that the sponge absorbed at least three times as much secretion as ordinary swabs; in addition, delicate organisms such as gonococci survived considerably longer than on charcoal-coated swabs.¹

However, Professor Brumfitt and his colleagues describe a contact-transfer procedure using this material, and both the advantages and disadvantages of such procedures must be fully appreciated. They are well adapted to qualitative studies and provide valuable "maps" of surface organisms in their original spatial relationship. Apart from plastic sponge, velvet pads, agar discs, and adhesive cellulose tape (the latter being suitable only for the drier and more resilient skin surface) have all given useful qualitative information in this way.² Unfortunately, the statement by Professor Brumfitt and his colleagues that their sponge transfers allow bacteria to be adequately quantified is unlikely to be borne out by detailed comparative studies. The yield of micro-organisms from such a transfer is very low; the closely comparable velvet pad, when moistened, transfers to a nutrient agar surface