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Repeated Acupuncture and Serum Hepatitis

SIR,—The letter by Dr. K. K. Hussain (6 July, p. 41) suggesting a possible link between repeated acupuncture and serum hepatitis is of considerable interest in view of the situation in China. On a recent visit to the People's Republic of China, under the auspices of the British Council, we saw the widespread use of acupuncture as a therapeutic measure, particularly in the commune hospitals. We even saw it being practised by children, on each other, in a class where acupuncture was being taught. The needles were placed in a sterilizing fluid but there was no evidence that any other form of sterilization was used.

We also noted the high incidence of hepatocellular carcinoma, and were told that one-third of these patients had a classical history of previous hepatitis and that the

liver usually showed cirrhosis at the time of diagnosis of the hepatic carcinoma. A sequence of events is suggested: virus hepatitis is extremely common, some patients proceed to develop cirrhosis, and some of these go on to develop primary liver tumours. The interest lies in how the virus is disseminated, and superficially it seemed to us that acupuncture could easily be responsible for this.—We are, etc.,

P. ALEXANDER

Chester Beatty Research Institute,
Belmont,
Sutton, Surrey

G. HAMILTON FAIRLEY
D. W. SMITHERS

Imperial Cancer Research Fund,
Department of Medical Oncology,
St. Bartholomew's Hospital,
London E.C.1

Smoking and Deep Vein Thrombosis

SIR,—The observation that venous thromboembolism is more likely to occur in hospital patients admitted for the treatment of myocardial infarction if they were non-smokers on admission than if they were smokers who then stopped smoking, which was reported by Dr. A. J. Handley and Mr. D. Teather and by Drs. P. Marks and P. A. Emerson (27 July, p. 230 and 232 respectively), is of considerable interest. At first sight the difference appears surprising, but it accords with a number of similar observations in other comparable circumstances. Avery Jones, Pygott, and I,¹ for example, found that gastric ulcers healed less well in non-smokers than in smokers who stopped smoking (61% average healing in four weeks against 83%), and Badaracco and Vessey² found that women admitted to hospital with "idiopathic" venous thromboembolism who had not been taking oral contraceptives were more likely to relapse than women who had been taking them but subsequently stopped

(36% within three years compared with 8%). Finally, we may note that smoking does not appear to be a cause of venous thrombosis.³

These results agree, therefore, in indicating that patients who have a probable cause for their disease fare better, once that cause has been removed, than patients whose condition has no such removable cause. Dr. Handley and his colleagues suggest that among those who develop myocardial infarction non-smokers tend to be intrinsically more susceptible to both arterial and venous thrombosis. In the light of the above observations their suggestion seems attractive.—I am, etc.,

RICHARD DOLL

Radcliffe Infirmary,
Oxford

¹ Doll, R., Jones, F. A., and Pygott, F., *Lancet*, 1958, **1**, 657.

² Badaracco, M. A., and Vessey, M. P., *British Medical Journal*, 1974, **1**, 215.

³ Vessey, M. P., and Doll, R., *British Medical Journal*, 1969, **2**, 651.

Endometriosis and Pseudopregnancy

SIR,—I think your leading article, "Endometriosis and Pseudopregnancy" (29 June, p. 682) underestimated the usefulness of hormone therapy. Andrews and Larsen,¹ whom you quote, treated only one-seventh of their patients by pseudopregnancy alone. Though in the much larger group treated by surgery alone results in terms of subsequent pregnancy were better than in the small group treated only with hormones they do not clearly indicate the reasons for their choice of treatment, and this selection pattern may have had a considerable effect on the results.

In a small personal series of 77 cases reported in 1969² there were 20 women who desired pregnancy: 14 of them became pregnant, some of them twice, after infertility which in three cases had been present for eight, nine, and 11 years respectively. The percentage success in this admittedly small group was materially higher than that quoted by Andrews and Larsen for their patients treated surgically. Subsequent surgery has so far been necessary in only two of the 77 cases in our series. One of these was a woman aged 23 with an extensive endometriosis of the uterosacral area causing dyspareunia so severe that she had to abandon intercourse. After treatment with norethisterone acetate and ethinyl oestradiol for nine months the mass in the pouch of Douglas was greatly diminished and her pain disappeared. She became pregnant and was delivered of a healthy child. A year later her pelvic mass and her dyspareunia recurred. Once again pseudopregnancy for 12 months was followed by such improvement that a second pregnancy occurred and was successfully concluded. She remained symptom free for a further year, but required hysterectomy at the age of 30 because severe symptoms reappeared. I am confident that conservative surgery in this patient would have been valueless and that a primary surgical approach would have deprived her of all hope of having children. We have not shared Page's³ experience that conservative therapy for endometriosis may be followed by prolonged anovulation. All the pregnancies in our series occurred within six months of the completion of treatment.

Even if, as Andrews and Larsen found, the need for subsequent operation is not

significantly reduced by pseudopregnancy this therapy is valuable as a holding measure, preserving fertility for at least a few years. Pseudopregnancy is also valuable in cases with lesions inaccessible to surgery when castration might be the only alternative treatment. Even if relief is only temporary, hormonal treatment may enable surgical menopause to be postponed to an age when it is likely to be better tolerated. Precise diagnosis before treatment can be made without laparotomy. Formerly we used culdocopy for this purpose, but often unsuccessfully because of adhesions in the pouch of Douglas. With the more versatile laparoscope this difficulty can almost always be overcome. Nausea and weight gain occurred in 25-30% of our cases, but the former usually ceased after about two months and normal weight was re-established a few weeks after the completion of treatment. Thromboembolic disease was not a problem.

In view of the foregoing I would urge gynaecologists not to discard the weapon of pseudopregnancy. It is often curative and even when further treatment, either hormonal or surgical, is required its contribution to the solution of what Andrews and Larsen call "a vexing clinical problem" is too valuable to be ignored.—I am, etc.,

J. A. CHALMERS

Worcester Royal Infirmary

¹ Andrews, W. C., and Larsen, C. D., *American Journal of Obstetrics and Gynecology*, 1974, 118, 643.

² Chalmers, J. A., in *Proceedings of Second European Congress on Sterility*, Dubrovnik, 1969, p. 19.

³ Page, E. W., *American Journal of Obstetrics and Gynecology*, 1974, 118, 650.

Pitocin Buccal in Late Pregnancy

SIR,—In recent years we have pursued the policy, in certain cases, of prescribing Pitocin Buccal (oxytocin) tablets (Parke-Davis) in late pregnancy to "ripen" the cervix prior to inducing labour. Furthermore, from talking to colleagues, we believe we are not alone in advocating this practice. We have nevertheless been doubtful as to the efficacy of such therapy, though at first glance the concept of "priming the uterus" with repeated small doses of oxytocin would seem an attractive one.

We therefore undertook a small pilot study, conducted as a double-blind trial and in conjunction with Dr. David Evans of Parke-Davis. In this trial one Pitocin Buccal tablet (200 units oxytocin) or a placebo, was given three times a day to 44 primigravidae from 38 weeks onwards until delivery. Patients were fully informed and only those who were both certain of their dates and had had uncomplicated pregnancies were selected. All patients took a minimum of 21 tablets (seven days), the mean number being 57 for the 22 who took the active preparation and 42 for the 22 who took the placebo.

Cervical effacement (as judged by a cervix of more than 2 cm length at the beginning of therapy diminishing to less than 1 cm length at the onset of labour or induction) occurred in only 61% of those patients taking the active preparation, and when this figure is compared with 50% effacement in

those taking placebo the difference is not significant. When we looked at the duration of the first stage of labour the results were likewise not significantly different in the two groups, though a mean of 7.6 hours for the Pitocin Buccal patients undoubtedly compared favourably with 10.2 hours for those taking placebo.

Our results lead us to conclude, therefore, that oxytocin as used in the form described here has no clinically detectable effect on uterine activity either in late pregnancy or labour. However, when one takes into account that as little as five years ago the mean duration of the first stage of labour at Guy's was 13.35 hours it is possible that any effect the oxytocin may be having in this respect is masked by the active approach to management of labour.—We are, etc.,

T. H. COLTART

T. G. NASH

Department of Obstetrics,
Guy's Hospital Medical School,
London S.E.1

Eugynon 30 and Dermatitis

SIR,—Dr. S. J. Jachuck and Dr. H. L. Franks (3 August, p. 347) may be interested to hear of a woman who developed urticaria each time she took an oral contraceptive pill. In 1967, 1972, and 1974 she was prescribed Lyndiol-2.5, Norinyl-1, and Microgynon 30 respectively. On each occasion, after only a few tablets, she developed a mild diffuse urticarial rash, mainly on the limbs and chest. She continued with the first two drugs for only two weeks but was persuaded to complete the course of Microgynon 30. The rash faded during the pill-free week. Pirition 4 mg was given with the second course of pills to see if it would modify the reaction. It had no effect and the woman stopped the pill of her own accord on the fifth day. During her first pregnancy, in 1957, she was found to be allergic to nickel but her subsequent pregnancies in 1961 and 1967 were uneventful. There is neither hormone, dye, nor manufacturer common to these oral contraceptives, and no light has been shed on the problem so far.—I am, etc.,

DELIA F. MORRIS

Family Planning Association,
Wellington, Salop

Tetracycline-resistant Group A Streptococci

SIR,—Dr. R. J. Fallon (22 June, p. 663) expressed interest in the prevalence of tetracycline resistance among streptococci in different areas of Britain. Though Ayrshire has both social and commercial links with Glasgow the incidence of tetracycline-resistant Lancefield group A streptococci (*Str. pyogenes*) observed here since 1970 differs markedly from that reported by Dr. Fallon for the Glasgow (north) area only some 30 miles (48 km) away.¹ In 1970 3.5% of strains isolated from hospitals and general practice in Ayrshire were resistant, in 1971 14.2%, in 1972 36.8%, and in 1973 33.3%. Currently (the first half-year of 1974) the incidence is 19.4%. Quarterly figures reveal

a much wider range of incidence, however, from 1.1% in the second quarter of 1970 to 53.7% in the same quarter of 1973.

Invariably during this time respiratory tract isolates overall have exceeded numerically those of non-respiratory origin, with a fairly even distribution not only year by year but also between hospitals and general practice. Interestingly, the twin peak-incidences of tetracycline-resistance of 51.5% (fourth quarter of 1972) and 53.7% both coincided with unusually high but non-epidemic incidences of non-respiratory isolates, though not of *Str. pyogenes* generally. Between these two quarter-years was one with an observed incidence of 26.8% for tetracycline-resistant *Str. pyogenes*, and for that reason as much as any other I hesitate to predict what trend our last quarter's incidence of 22.2% is supposed to show. As reiterated in your leading article on tetracyclines (25 May, p. 400), resistance is now common enough to make that group of antibiotics unsuitable as a first choice for infections caused by *Str. pyogenes*.—I am, etc.,

JOHN A. N. EMSLIE

Microbiology Department,
Ayrshire Central Hospital,
Irvine, Ayrshire

¹ Fallon, R. J., *British Medical Journal*, 1973, 4, 300.

Antiemetics, Prolactin, and Breast Cancer

SIR,—We agree with Dr. H. W. C. Ward (20 July, p. 169) that drugs which raise circulating prolactin levels should not be given to patients with breast cancer. Following our report on metoclopramide's potent prolactin-releasing properties (29 June, p. 729) we support his suggestion that it is best avoided in such patients.

It is well established that the phenothiazines raise circulating prolactin levels.¹ We have shown that prochlorperazine, a phenothiazine, which Dr. Ward suggested as an alternative to metoclopramide, also raises prolactin in man, thus making it, too, unsuitable. To find a suitable antiemetic which was without effect on prolactin levels we have screened a number of compounds. Of these cyclizine 50 mg intramuscularly did not alter circulating prolactin levels. We therefore recommend that cyclizine rather than prochlorperazine should be the antiemetic of choice for patients with breast cancer undergoing radiotherapy.—We are, etc.,

M. O. THORNER

G. VOLANS

G. M. BESSER

A. S. MCNEILLY

St. Bartholomew's Hospital,
London E.C.1

¹ Turkington, R. W., *American Journal of Medicine*, 1972, 53, 389.

Blood Donors and the Transfusion Service

SIR,—While agreeing with much of your provocative leading article (27 July, p. 212) I must protest at your statement that the British blood transfusion service is clearly a lame duck. Foreigners reading your journal—and many do—might be forgiven for believing that our blood transfusion service,