technician to medical laboratory scientific officer was one step in the process, although the institute had undoubtedly set its sights on having medical laboratory scientist adopted as the new designation.⁷ The MLSO designations came into effect earlier this year and already it is common practice for MLSOs to shorten their descriptions with misleading effects. I know of recent instances where a principal MLSO and a chief MLSO have, respectively, described themselves as principal scientific officer and as chief scientific officer, designations properly reserved at present for the scientific Civil Service. All that glitters is not gold.

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¹ Institute of Medical Laboratory Sciences. *Future Staffing in the Medical Laboratory Service: A Policy Statement*. London, IMLS, 1976.

² Institute of Medical Laboratory Sciences. *Evidence to the Royal Commission on the National Health Service*. London, IMLS, 1977.

³ *IMLS Gazette*, 1978, 22, 125.

⁴ Department of Health and Social Security, *Hospital Laboratory Services (HML(70)50)*. London, DHSS, 1970.

⁵ Department of Health and Social Security, *Organisation of Scientific and Technical Services (HSC(15)16)*. London, DHSS, 1974.

⁶ *British Medical Journal*, 1978, 2, 1381 and 1438.

⁷ *IMLS Gazette*, 1978, 22, 121.

Naming of drugs

SIR,—While I accept what Dr G A MacGregor says, that some proprietary names are so similar that confusion may occur (18 November, p 1433), I take issue with your editorial comment that "BPC approved names . . . are carefully devised not only to avoid confusion with other names but also to indicate, wherever possible, the pharmacological group to which the drug belongs." I would maintain that there is greater similarity between generic names than proprietary names and that the risks of confusion here are greater. For example, are not trifluoperazine and trifluopromazine more similar than two suggested by Dr MacGregor—namely, Sinthrome and Sintisone? Trifluoperazine is approximately 40 times more potent than trifluopromazine and if the former were dispensed instead of the latter the risk of serious side effects occurring (for example, oculogyric crises) is extremely high. Usually generic names are abbreviations of chemical formulae, yet if they are to bear a reasonable resemblance to their chemical structure they need to be extremely long and therefore, for the clinician, beyond recall. If they are too short they tell us too little of the chemical structure and the dangers of similarity are increased. By comparison with our general vocabulary chemical structures are very few. "Chlor" appears as a prefix for no fewer than 300 generic names; stems are common to many drugs, as are suffixes. Fortunately proprietary names offer a greater opportunity for providing names which are easy to recall and which should lead to less confusion.

The editorial assertion that names in certain pharmacological groups are similar is not well founded, for there are countless anomalies to be found among groupings—for example, chlorthalidone among many benzodiazepines ending in -azepam, prothipendyl among a list of aliphatic phenothiazines ending in -promazine, and so on. Further, there are many drugs which do not belong to a pharmacological group—for example, chlormezanone

and benzocytamine, both of which have tranquillising properties, and their naming is virtually meaningless and therefore of no value to the prescribing physician.

Another point which is not generally appreciated is that generic names may vary from country to country—for example, adrenaline or epinephrine and pethidine or meperidine.

The question of prescribing proprietary or generic names was discussed at length at a recent symposium in London at which, apart from myself, members of the appropriate committees in the USA, WHO, and United Kingdom spoke. It was clear that the issue was not a simple one and therefore the short editorial comment needs to be put into perspective.

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SIR,—I read with interest the letter from Dr G A MacGregor (18 November, p 1433). I believe that the pharmaceutical industry is not solely to blame for the confusion in drug names and that the BP and the BPC must also bear some responsibility. To quote two examples, dothiepin (Prothiaden) and doxepin (Sinequan) are psychotropic drugs with similar indications. However, co-trimoxazole (Bactrim/Septtrin), clotrimazole (Canesten), and co-trimazine (Coptin) are prescribed for entirely different therapeutic indications. Even when prescriptions are written legibly and clearly (and this is often not the case) there is still room for confusion.

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SIR,—The disadvantage of the BPC approved names of drugs is that they are often too cumbersome for easy use. For instance, prednisolone steaglate is a longer and more tongue-twisting term than Sintisone. Although the brand names have the advantage of brevity, many, like "nicknames," could easily be shorter. The principles of basic English should apply to the naming of drugs. One-syllable words would be best, until the supply runs out.

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Progestogen modification of endometrial histology in menopausal women

SIR,—Recent studies by Mr D W Sturdee and others (17 June, p 1575) and Professor Ian Craft and others (5 August, p 429) have supported our original report¹ that progestins appear to be capable of protecting the postmenopausal endometrium against excessive oestrogen stimulation. Our latest data² give a 32% incidence of hyperplasia (22 of 69 patients) with unopposed oestrogens prescribed at the manufacturers' maximum recommended dosages and a 16% incidence (6 of 37 patients) with low dosages. The addition of a progestogen for five or seven days each month significantly reduced this incidence to 5% (4 of 76 patients) ($P < 0.001$).

Our incidence of hyperplasia with unopposed oestrogens is higher than that

reported by Mr Sturdee and his colleagues, but the mean duration of treatment in our series was longer—15.8 months as against 9.7 months. Prolongation of therapy can be associated with a sudden change of normal to hyperplastic endometrium² and biochemical data suggest that this results from an alteration in end-organ sensitivity.³ Thus in Mr Sturdee's on-going trial we would expect an increase in the incidence of hyperplasia to accompany a longer duration of treatment.

The reversal of hyperplasia to normal endometrium following progestogen therapy^{4 5} provides further evidence for this protective effect. This ability is of great benefit as hyperplasia is already present in about 6% of women seeking oestrogens for relief of symptoms. At pretreatment curettage this condition was diagnosed in two of 31 patients by Professor Craft and his colleagues and in eight of 163 patients in our series.⁵ Unopposed oestrogen therapy may not reverse hyperplasia and indeed adenocarcinoma can develop subsequently.¹

Proof that synthetic progestins such as norethisterone are physiologically active and possess progesterone-like activity comes from two sources—studies of cell fine structure and biochemistry; and such data are likely to result in a reassessment of the biological potency of these synthetic analogues. For example, ovulation is followed by the appearance of three fine-structural features: subnuclear accumulations of glycogen, giant mitochondria, and nucleolar channel systems. Until now it has not been known whether these factors are due to ovulation per se or a particular balance of oestrogen and progesterone.⁶ However, we have found these structures in 10 postmenopausal women receiving Premarin 1.25 mg every day and norethisterone 5 or 10 mg daily for 10 days of each month (see figure). Therefore these features are not specific to ovulation and norethisterone can effect the same ultrastructural changes as progesterone.

Electron micrograph of an endometrial cell from a 54-year-old woman five years after the menopause. N=nucleolar channel system; M=giant mitochondrion; G=basal glycogen. Treatment as described in text.

Biochemically, norethisterone reduces nuclear oestrogen and soluble progesterone receptor formation and induces the formation of oestradiol 17 β -dehydrogenase.^{3,7} We have evidence that in oestrogen-treated climacteric women the principal intranuclear oestrogen is oestradiol⁸—in spite of the plasma excess of oestrone.⁹ The dehydrogenase is likely to be of paramount importance in protecting against endometrial hyperstimulation as it converts oestradiol to the less potent oestrone, thereby reducing the biological effectiveness of the oestrogenic stimulus.

We believe that sequential oestrogen/progesterone therapy is to be preferred to unopposed oestrogen therapy. However, the type and dosage of progestin and the duration of administration most effective in depressing cell proliferation still need to be determined. We believe a better understanding of intracellular steroid interactions, and of their fine-structural and biochemical sequelae, will enable rapid development of a highly efficacious regimen which does not hyperstimulate the endometrium.

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¹ Whitehead, M I, *et al*, *Acta Obstetrica et Gynecologica Scandinavica*, 1977, suppl 65, p 91.

² Whitehead, M I, *et al*, in *The Role of Oestrogen/Progesterone Therapy in the Management of the Menopause*, ed I Cooke, p 121. Lancaster, MTP, 1978.

³ King, R J B, *et al*, *Cancer Research*. In press.

⁴ Gambrell, R D, jun, *Second International Congress on the Menopause*, Jerusalem, 1978.

⁵ Campbell, S, *et al*, *Postgraduate Medical Journal*, 1978, 54, suppl 2, p 59.

⁶ Wynn, R M, in *Biology of the Uterus*, ed R M Wynn, 2nd edn, p 341. New York, Plenum Press, 1977.

⁷ King, R J B, *et al*, in *The Role of Oestrogen/Progesterone Therapy in the Management of the Menopause*, ed I Cooke, p 111. Lancaster, MTP, 1978.

⁸ Dyer, G, King, R J B, and Whitehead, M I, *International Symposium on Progress in Menopausal Therapy*. London, 1978.

⁹ Whitehead, M I, *et al*, *Postgraduate Medical Journal*, 1978, 54, suppl 2, p 69.

Immunisation of adults against diphtheria

SIR,—Dr R Mitchell and Mr A Barr (11 November, p 1371) report that few random blood donors in the west of Scotland have diphtheria antitoxin levels high enough for the production of a potent specific immunoglobulin. Their conclusion is in agreement with an earlier WHO report¹ that the best yields of human antitoxin will be obtained from the plasma of deliberately immunised donors. However, it would be unwise to assume that the antitoxin levels of all recently immunised donors will be high. A Medical Research Council report² observed that only one of 211 vaccinated healthy young persons in Dundee had an antitoxin content of more than 10 IU/ml, whereas such high levels were found in 37 of 106 inoculated persons in Copenhagen. Many

of the Danish subjects had been given three injections of refined antigens of high immunising efficiency and this might explain the difference in response.

Given that there is solid protection with a serum antitoxin concentration of 0.01 IU/ml or more, the observation that only 4.9% of 3000 random donors in the west of Scotland had antitoxin above that critical level is surprising and disturbing. In contrast Sheffield *et al*³ found that 41 (68.3%) of 60 medical students had protective levels of diphtheria antitoxin. Certainly donors over 40 years of age are unlikely to have received diphtheria vaccine, but many were probably exposed to subclinical doses of *Corynebacterium diphtheriae* in their earlier years. Indeed, a recent report⁴ from Finland, where immunisation of children with diphtheria toxoid was introduced in 1943, showed that 55% of hospital patients aged between 40 and 59 years had an antitoxin concentration above the protective level. Although the report from the west of Scotland is concerned primarily with the possible production of human anti-diphtheria immunoglobulin, the incidental observation on the apparent high susceptibility of adults to diphtheria surely merits a detailed investigation by all interested parties.

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¹ World Health Organisation, *Technical Report Series No 327*. Geneva, WHO, 1966.

² Hartley, P, and Tulloch, W J, *Medical Research Council Special Report Series No 272*. London, HMSO, 1950.

³ Sheffield, F W, *et al*, *British Medical Journal*, 1978, 2, 249.

⁴ Kerttula, T, *et al*, *Proceedings of Scottish-Scandinavian Conference on Infectious Diseases*, 1977, p 30.

Manipulation for low back pain

SIR,—The article by Dr H Sims-Williams and others on manipulation in low back pain (11 November, p 1338) well illustrates the difficulty of trials in manipulation, all of which have been too limited and most of which have used relatively ineffective methods of manipulation. In this study a rigid age group aged 20-65 was used, whereas in practice one encounters all age groups from the very young, to the very old. Indeed it is the two extremes, the very young child and the older age group, who respond very well to manipulation. The acute cases in these groups are the ones which are frequently cured by one treatment. Pregnancy also should never be excluded; these also are very rewarding cases, for during pregnancy and the puerperium spinal ligaments relax, thus allowing subluxations of the apophysial joints to occur more frequently than usual. These can be manipulated at all stages of pregnancy without any fear of disturbing the pregnancy and are usually very easy cases to treat. The same applies to the aftermath of spinal surgery, when adhesions which are very amenable to manipulation occur. They are usually very simple to treat by manipulation, and indeed these cases should not be treated with a surgical belt but by manipulation; if this fails re-exploration of the spine is probably needed. Muscle wasting is also no contraindication, nor is any loss of reflexes or gross restriction of straight-leg raising. Lesions which have been present for over a month respond just as well as those which have just occurred.

In the series reported treatment was given by one form of manipulation only, in combination

with traction and spinal exercises. A true trial of manipulation should be of manipulation only, for to bring in traction and exercises is to cloud the issue. Both traction and exercises can increase the pain and the length of time for the patient to become pain-free. The Maitland method is disliked by most of the 30-40 physiotherapists whom I train every year. They believe that it is ineffective when compared with other forms of manipulation. Even if the Maitland method of manipulation is a good one it is useless to treat patients with low back pain with only one system of manipulation; some need one system and some another, and in some cases it is necessary to use different systems on the same patient at the same treatment. When I treat patients I may use the osteopathic method first; then at the same attendance, if this should fail, I would use the Oregon or hold system; and if this fails the Hoover or static system. One manipulation per attendance is rarely enough as one may get some improvement from a thrust technique and then more improvement from a rotational technique at the same attendance. Thus it will be seen that apart from the very young or the elderly one treatment is rarely enough and indeed 3-6 treatments may be necessary—very rarely more. In the trial reported manipulation was given daily for a week. In practice set rules are impossible, for if a patient is improving he is usually made worse by being treated too frequently. I was surprised to see in this article that treatment was continued for one month, for in practice one aims at getting at least 50 degrees improvement of straight-leg raising and forward bending at the first treatment and to have the patient back at work in a week and completely pain-free in two weeks. In this trial it was stated that "manipulation inevitably affected other joints." If this was indeed true one can see why the trial failed to show the superiority of manipulation over placebo methods. If the operator cannot lock a spinal joint so that only that joint is manipulated success will rarely be achieved, for it is that joint which is in spasm and is the most difficult to manipulate. If it is not locked the operator will manipulate the joint above and the joint below.

I would exclude from the trial any patient not improved either objectively or subjectively two days after the first manipulation. If the slightest degree of improvement is not achieved the patient should be referred either to a more experienced manipulator or for a surgical opinion. In conclusion, any clinical trial on manipulation can only be as good as the manipulator and the method of manipulation.

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SIR,—The trial reported by Dr H Sims-Williams and others (11 November, p 1338) included 108 consecutive patients, half having active physiotherapy and the other half placebo therapy. The active treatment consisted of mobilisation, traction, or manipulation but unless all 108 patients included in the trial were suitable for this treatment the active group would have been diluted by patients whom many would regard as unsuitable for these types of treatment; these would particularly include patients who were hypermobile, especially if the pain was arising at a mobile segment of the lumbar spine. For these patients manipulation may actually be detrimental. The clinical assessment by the doctor is not recorded as including detailed examination of spinal mobility at different levels, or whether the pain was localised at the same level as any loss of movement which the physiotherapist was presumably treating.

What is needed is a trial of manipulation on patients who are all suitable for this type of treatment, half the patients then receiving