kept unequivocally biochemically euthyroid. It is possible (and I feel worthy of serious consideration) that the addition of a small maintenance dose of L-thyroxine might enable the required dose of steroids (which of course also have a valuable role to play) to be reduced to a lower level than 10 mg of prednisone a day with an accompanying lessening of the risk of long-term steroid-induced complications.

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Possible environmental hazards of gas cooking

SIR,—Recent letters in your columns from Mr E A K Patrick and Dr M C S Kennedy (19 August, p 567) discussed possible hazards of indoor air pollution from gas stoves. In community studies of respiratory disease in Connecticut and South Carolina^{1,2} in which we obtained data on respiratory symptoms and lung function from about 7000 residents aged 7 years and over we compared residents in homes with gas stoves with those who used electric power for cooking. We took age, sex, race, height, weight, and smoking habits into account in these comparisons and found that respiratory symptoms were equally prevalent and lung function similar among men, women, and children living in homes with gas stoves and in homes with electric stoves. Using portable sampling equipment in selected homes³ we confirmed that nitrogen dioxide (NO₂) concentrations are higher in homes equipped with gas stoves than in those where electricity is used for cooking. Twenty-four-hour average NO₂ levels in 11 homes with gas stoves ranged up to 500 µg/m³, and short-term (2 h) peaks up to 3000 µg/m³ were observed in one such home. However, we have found no evidence that the use of gas for cooking is associated with increased respiratory illness among adults or among children aged 7 years and older. We did not investigate younger children, but any effect of indoor air pollution from gas stoves on their lungs would appear to be temporary, since no changes were detected after the age of 7.

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- ¹ Mitchell, C A, Schilling, R S F, and Bouhuys, A, *American Journal of Epidemiology*, 1976, **103**, 212.
- ² Bouhuys, A, Beck, G J, and Schoenberg, J B, *Nature*, 1978, **276**, 466.
- ³ Binder, R E, et al, *Archives of Environmental Health*, 1976, **31**, 277.

Ankylosing spondylitis in HLA-B27-positive individuals: use in diagnosis

SIR,—We wish to take issue with several of the major points in your leading article (2 September, p 650) "HLA-B27 and risk of ankylosing spondylitis." You recognise the obvious conflict between the early and the more recent population studies of the prevalence of ankylosing spondylitis (AS) but, for reasons not given, prefer to believe the more recent

studies. In order to resolve this conflict we have recently conducted a study of the population of Busselton, Western Australia, and have found a prevalence of AS similar to that predicted by the early studies rather than the much higher prevalence suggested by Calin and Fries.¹ We HLA-typed some 80% of the adult population of this relatively isolated, stable, small country town. Of 2745 people, 168 (6.1%) were B27-positive. Careful questionnaire and clinical assessment was undertaken on 139 of these together with an age- and sex-matched control group from the same population. Radiographs, which were available from 22% of the B27-positive individuals, were reviewed. Not one case of AS was detected and there were no differences in the frequency and character of the back pain between the two groups. We used a questionnaire similar to that of Calin and Fries and obtained sacroiliac radiographs in a higher percentage of the study group. These data suggest a maximum prevalence of AS of 0.04% in the population as a whole and 0.7% in B27-positive individuals and are therefore consistent with the earlier "classical" surveys rather than some surveys¹ which may be explained either on the basis of the population studied or over-interpretation of symptoms, signs, and x-rays.

Your article states that in a suspicious clinical setting (for example, persistent backache) the presence of B27 will help in making a diagnosis. We contend that it is the absence of B27 which should be used to exclude AS, while the finding of B27 in a patient is of little help. Such a contention is based on the relative rarity of AS and the relative frequency of B27. Even if one accepts the figures from the study of Calin and Fries four out of five people with B27 will not have AS. Furthermore, recent studies² indicate that the incidence of B27-negative AS may be as low as 1-2%. Therefore it is clear that HLA-typing for B27 should be used as an excluding test rather than a confirming test.

Because the prevalence of AS in B27-positive individuals is nearer 1% than 100% genetic counselling will cause needless anxiety. In fact, with modern treatment, as outlined in your article, AS is nowadays an infrequent cause of significant morbidity.

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- ¹ Calin, A, and Fries, J F, *New England Journal of Medicine*, 1975, **293**, 835.
- ² Joint Report on HLA and Disease, Seventh International Histocompatibility Workshop Conference, Oxford, 1977. In press.

* * * These new data from Dr Christiansen and his colleagues are of considerable importance and it will be of great interest to read a full report, when the size and age structure of the population and its ethnic origins will be clearer. The great variations which are known to exist in the frequency with which AS is found in different populations must be taken into account and it may be that a small, relatively isolated country town in Western Australia is not typical of the generality of Caucasians. Certainly the observations made by Calin and Fries are similar to those of Kidd *et al*¹ in suggesting a much higher frequency of clinical and subclinical AS in B27-positive individuals than Dr Christiansen has found. No doubt more studies will be completed and the

mystery resolved in time. We agree with Dr Christiansen that genetic counselling for ankylosing spondylitis should be approached with caution, and this was stated in the leading article. Also consistent with the leader is Dr Christiansen's point that the absence of B27 might be helpful in diagnosis.—Ed, *BMJ*.

¹ Kidd, K K, et al, in *HLA and Disease*, ed J Dausset and A Sveigaard, p 72, 1977.

Another hazard of pierced ears

SIR,—I should like to draw readers' attention to another hazard of the present fashion for pierced ears. Three children from the same family attended our casualty department 24 h after having their ears pierced at a reputable Sheffield jewellers. They had all received ethyl chloride spray to anaesthetise their ear lobes before piercing.

However, instead of the usual 20-30 seconds' worth of spray, each received several minutes of spraying to each lobe (the mother thought about seven minutes in one case). All the children had deep purple areas of chemical "frost bite" from their ears in a thick area 2 in (5 cm) wide across their necks running to the midline of their throats. These burns have subsequently completely healed with conservative treatment without leaving a scar.

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Polymyalgia rheumatica and primary biliary cirrhosis

SIR,—In reply to the comments of Drs A L Ogilvie and P J Toghill (25 November, p 1501) on our case reports (21 October, p 1128), we are aware that hepatic biochemical abnormalities occur in polymyalgia rheumatica (PMR) and, indeed, not uncommonly in the investigated population. However, the abnormalities we found were gross, consistently so, and were indicative of hepatic disease. The antimitochondrial antibody titres were high and remained so in our unbiopsied patients. Our patients did not have serositis, fever, or peripheral arthritis or arthralgia. Therefore we cannot agree that the systemic lupus erythematosus-like syndrome they mention should enter in the differential diagnosis.

Liver biopsy was not performed on two patients. One was a recluse and it reflects credit on her daughters and her general practitioner, Dr Nigel Shield, that we were able to investigate her as far as we did. A biopsy would not have altered management and neither would it have done so in our first case, in which it would probably not have contributed to diagnosis either. We believe that careful and repeated biochemical examination is sufficient to establish a probable diagnosis of chronic hepatitis in appropriate clinical circumstances. Walker *et al*¹ noted that in all of their biopsied cases the results confirmed the biochemical findings of significant hepatic abnormality. We also note that one of their patients, a doctor, refused liver biopsy because there were no hepatic symptoms.

With regard to Drs Ogilvie and Toghill's comments concerning our mention of a first case report of PMR and chronic hepatitis, we are surprised at their use of the word "categorically." Our reading of the text was that

liver biopsy did not "on the whole" warrant any more specific a diagnosis than chronic hepatitis, but that the patient with PMR had clinical and biochemical features more suggestive of primary biliary cirrhosis (PBC) than chronic aggressive hepatitis. We were anxious to give credit to the workers who first mentioned the coexistence of PMR with chronic hepatitis having features of PBC, particularly as only one of Sherlock's 100 cases of PBC had a rheumatological presentation and that was rheumatoid arthritis. Finally we wish to point out that our opening paragraph states that our cases had "features of PBC," and that we stated that we merely believed that the two conditions were not associated by chance.

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¹ Walker, J G, Doniach, D, and Doniach, I, *Quarterly Journal of Medicine*, 1970, **153**, 31.

Precordial exercise mapping

SIR,—I was interested to read the papers of Drs Andrew Selwyn and Kim Fox and their colleagues (9 December, pp 1594 and 1596) on the uses of precordial exercise mapping.

While I am glad to see the use of different electrocardiographic systems for the diagnosis of myocardial ischaemia, I feel the differences in the accuracy of such systems need careful investigation. For many years the relative merits of the orthogonal three-lead system and the 12-lead system in routine electrocardiographic analysis have been controversial. Many of these arguments have been due to individual preferences for one or other system. Vectorcardiographers have preferred the three-lead system and most British cardiologists have preferred to use 12 leads. Objective verification more recently has shown few differences. Sensitivity of the electrocardiogram can be improved slightly by combining measurements from the 12 leads and the orthogonal leads.¹ However, specificity is reduced somewhat. These results have been found to apply to exercise testing and even extensive precordial mapping has not improved sensitivity by more than about 5%.²

Dr Fox and his colleagues found a greater increase of sensitivity (27%) without loss of specificity. Different methods of exercise testing are important causes of variation in results, but differences in subsequent electrocardiographic analysis may also be important. The sample of patients examined was smaller than that of Kilpatrick³ and since it contained a minority of patients with normal coronary arteries specificity could never be less than 75%. It would be interesting to know the results of mapping in a wide variety of conditions, perhaps by computer analysis.

From the data on exercise testing it seems likely that the 10 patients in whom ambulatory monitoring showed only 14% of episodes of ST depression at V5 that were found at the point of maximal ST depression represented mainly patients without any ST depression at V5 (Dr Selwyn and colleagues). It would seem likely that the 35 patients with lesser ST changes at V5 than at the maximal point would show about 50% of the episodes of ST depression at V5 that could be found at the

point of maximal ST depression. It would be interesting to know if the point of maximal ST depression corresponds to the point of maximal R wave (or the direction of the QRS loop). If so, many cardiologists could immediately improve the sensitivity of single-lead ambulatory monitoring and bipolar exercise testing by placing the precordial electrode on the position of the maximal R wave in the 12-lead ECG.

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¹ Talbot, S, et al, *British Heart Journal*, 1976, **38**, 1247.
² Block, P, et al, in *Proceedings of the 7th European Congress of Cardiology*, 1976, p 737.
³ Kilpatrick, D, *Lancet*, 1976, **2**, 332.

Training in internal fixation of fractures

SIR,—Mr J C Griffiths (9 December, p 1615), perhaps unintentionally, has given the impression that the orthopaedic surgeon involved in the treatment of fractures may only pursue excellence by the regular use of the ASIF system, assuming that he has the time, patience, necessary facilities, and supporting staff.

Nothing could be further from the truth than this astonishing suggestion. An orthopaedic surgeon who has the time and patience and the necessary facilities and supporting staff may just as well pursue excellence through conservative treatment, provided that he has been properly trained and the methods are properly applied. Modern fracture management requires an eclectic approach which will come naturally to the orthopaedic surgeon whose training has preserved a sense of balance.

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"Therapy Options in Psychiatry"

SIR,—In his reviews of psychiatric books for the *BMJ* Dr Henry R Rollin demonstrates his traditional "medical model" approach to psychiatric practice and this denies your readers the opportunity of learning about the various forms of treatment which are available to, and used by, many psychiatrists today. This is especially important as he writes in a general medical journal and therefore acts as the "shop window" for psychiatry to many hospital specialists and general practitioners.

In his recent review of "Therapy Options in Psychiatry" (18 November, p 1423) he writes regarding marital therapy and family therapy, "By what token are doctors entitled to interfere in or to control the lives of other mortals?" We would like to point out that the aims of these therapies are no more to "control" or "interfere with" the lives of patients than are other forms of psychiatric treatment. In fact, establishing a contact at the outset will often give the patient a very much clearer choice and more control over the treatment than, say, with the use of drugs.

These forms of treatment may also be more appropriate and effective than the traditional psychiatric treatments. For instance, general practitioners and hospital practitioners are increasingly faced with sexual problems, behavioural disturbances in children, and forms of anxiety and depression in which drug treatment or traditional psychotherapy

are either ineffective or inappropriate and marital or family therapy is the treatment of choice.

While Dr Rollin may wish these therapies to remain outside medicine, we believe that all doctors should at least become acquainted with them in the best interest of their patients.

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What is a cohort?

SIR,—In their series "Epidemiology for the uninitiated" (2 December, p 1558 and 9 December, p 1616) Professor Geoffrey Rose and Dr D J P Barker use the word "cohort" to describe a group of persons selected by exposure to an environmental factor and studied during a subsequent period of time. This is not the meaning of "cohort" as it was first used in epidemiology by Wade Hampton Frost in a letter¹ to Dr Edgar Sydenstricker dated 29 July 1935. His meaning is quite clear from the text of that letter, published posthumously, and from its use in his paper^{2,3} "The age selection of mortality from tuberculosis in successive decades," also published posthumously. Subsequently this useful term was used in the sense which Frost intended, of which the shortest and best definition is that given by Case⁴ in 1956: "The essential feature of cohort analysis is that it follows the mortality rate of a population defined by its birth years throughout the life time of the surviving portion of that population" (my italics). The extension of its use to the study of population attributes other than mortality would be reasonable, but to extend its use to a group not characterised by birth within a defined period of time destroys the usefulness of the term and I hope that this is not now generally accepted. If it is, yet another word has lost its true value.

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¹ *Papers of Wade Hampton Frost*, ed K Maxcy, p 580. New York Commonwealth Fund, 1941.

² *Papers of Wade Hampton Frost*, ed K Maxcy, p 593.

³ Frost, W M, *American Journal of Hygiene*, 1939, **30**, 91.

⁴ Case, R A M, *British Journal of Preventive and Social Medicine*, 1956, **10**, 172.

Tetracycline preparations for children

SIR,—Dr R J Rowlatt (18 November, p 1436) suggests that it is irresponsible of us as manufacturers to make available liquid oral preparations of tetracyclines owing to the possibility of causing stained or deformed teeth in children.

In no way do we support the routine use of tetracyclines for the management of infections in children or pregnant women. However, these products have a significant role in the management of infections in which tetracyclines are the treatment of choice. The commonest of these are, of course, brucellosis and mycoplasma infection. As mycoplasma is being identified more and more as the cause of respiratory tract infection in children we feel that the continued manufacture of these formulations is justified on this basis. We do, of course, include a statement referring to the possibility of tooth staining and enamel hypoplasia on our data sheets so as to make