bicycle with a 10% increase using salbutamol or a 12% increase using portable oxygen. One must compare like with like. If increase in distance covered is the aim then a car would be better than a bicycle.

The authors suggest that lightweight tricycles may be useful in rehabilitating patients with chronic airflow limitation, but they offer no evidence for their assertion that cycling is a good form of exercise for such patients. Controlled studies have shown that simple exercises, aimed mainly at the legs, can improve exercise tolerance and everyday activities in breathless patients.[1 2] We are currently studying the mechanism of this improvement. It will be interesting to see whether, in a properly controlled trial, cycling is any better than simple exercises at improving the ability to walk and perform daily activities. Meanwhile, we hope that those breathless patients who can raise the necessary £250 do not all rush onto the roads on tricycles. Many of them are elderly with poor eyesight, hearing, and coordination and would be a hazard to themselves and other road users.

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Scoliosis in the community

SIR,—Professor Robert A Dixon (19 February, p 615) has attempted to "clarify" and "inject some sense" into the question of school screening for scoliosis. Unfortunately, he has achieved neither. His terms pelvic scoliosis, spinal scoliosis, progressive scoliosis, and the flippant scholiosis are unnecessary and confusing. The terminology committee of the Scoliosis Research Society has published a glossary of scoliosis terms that have gained widespread acceptance and usage.[1] Pelvic scoliosis is in most cases non-structural scoliosis and in a person older than 10 years it may be termed adolescent idiopathic scoliosis regardless of whether it has the usual characteristics of side and of sex and regardless of whether it is progressive, non-progressive, or resolving.

The epidemiological survey outlined by Professor Dixon would not serve as a satisfactory screening programme and is not a sound basis for criticism of such programmes. Despite the fact that screening uses "a crude visual test" our own experience has shown that the test is valid and sensitive. The specificity is such that some false positives do occur, but this causes no harm to the patient. Early screening programmes have in effect been epidemiological surveys, and the data obtained have given us new information on the natural history of this disease.[2] Professor Dixon should remember that any screening test is not intended to be diagnostic. Persons with positive or suspicious findings must be referred for accurate diagnoses and treatment.[3]

Primary screeners need not be "experienced senior physiotherapists." Trained, non-medical volunteers quickly develop a sharp eye and with a little extra training can also recognise inequality in leg length in most cases. Children with suspicious findings should be referred for rescreening by a doctor. In our programme about 50 children are seen once a month and can be processed easily by a doctor in a teaching setting in less than an hour, hardly an "enormous waste of time, energy, and money."

In Professor Dixon's study all those with visual evidence of asymmetry underwent *x* ray examination. This is unnecessary. Discretion in doctors is important, and referral for *x* ray examination will depend on factors such as age and physiological maturity. Suspicious cases may be reliably followed by rescreening in six months or by other follow up methods.[4]

For optimal response the newer types of conservative treatment of scoliosis require that they be used in the relatively immature patient with progressive but less severe curves. Scoliosis screening is an effective first stage in identifying these curves.

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HLA antigens and acetylcholine receptor antibodies in penicillamine induced myasthenia gravis

SIR,—Without wishing to divert attention from the most striking observation in our paper (29 January, p 338)—that is, the association of the HLA antigens Bw35 and DR1 with myasthenia gravis induced by penicillamine—we would like to clarify some of the points raised by Dr A Vincent and Dr J Newsom-Davis.

Although they now seem to agree that there are differences between penicillamine induced myasthenia gravis and spontaneous myasthenia gravis in cross reactivity, their position was far from clear in the previous paper.[1] In that paper they state that sera from patients with penicillamine induced myasthenia gravis showed insignificant reactivity with rat or mouse acetylcholine receptor, and a table seems to indicate that this was true of the group of 11 sera studied. Apparently this cross reactivity differed when higher concentrations of acetylcholine receptor and longer incubation times were used, but no comparisons were made and indeed no data were given. There is an unequivocal statement that cross reactivity was lower with penicillamine induced myasthenia gravis than with generalised idiopathic myasthenia gravis. To our surprise, however, the discussion states that penicillamine induced myasthenia gravis anti-acetylcholine receptor reacted with mouse and rat acetylcholine receptor, suggesting that these antibodies are not as restricted as we reported previously.[2] The statement is quite unjustified. As far as we can interpret the data of Dr Vincent and Dr Newsom-Davis they support our own conclusion, with which they now appear to agree. Since they did not compare the cross reactivity of sera from patients with disease of recent onset we wonder why they conclude that the difference was a function of technical variation or of the duration of disease.

The definition of "recent onset" myasthenia gravis is difficult. We have now studied four patients with idiopathic myasthenia gravis, who had circulating antiacetylcholine receptor up to two years before the development of clinically obvious myasthenia gravis; sera had been stored for other reasons and were tested retrospectively. Clinically apparent myasthenia gravis developed only when the antiacetylcholine receptor titre rose above a certain threshold.[3] Although these patients might have been regarded as "recent onset," it is clear that the autoantibody had been present for a much longer time.

The binding curves in figure 1 in our paper (29 January, p 338) were used as examples to illustrate the method of determining "average affinity" of antiacetylcholine receptor. The sera had been arbitrarily labelled serum samples 1 and 2. In fact they corresponded to cases 10 (idiopathic myasthenia gravis) and 17 (penicillamine induced myasthenia gravis) respectively in table 1.

We chose this method of saturation analysis[4] as a simple way to estimate and compare "average affinities" of antiacetylcholine receptor because it is generally accepted that curved lines are more the rule than the exception when Scatchard plots are used to study affinity of antibody binding to complex antigens.[5] Factors that predispose to curved Scatchard plots are: (*a*) the presence of populations of antibodies with a range of affinities; (*b*) bivalency of the antibody molecule; and (*c*) the presence of multiple binding sites on the antigen molecule.[4 5]

Dr Vincent and Dr Newsom-Davis have taken the data from one of our sera (sample 1), subjected them to Scatchard analysis, and shown that the resultant plot was curved. This observation serves only to underline our point. Figure 1 of our paper (29 January, p 338) shows that saturation of binding had occurred "at the range of concentrations of acetylcholine receptors used." Had saturation not been reached, however, this should not have altered the shape of a linear plot. Indeed, Scatchard analyses may be done under non-saturating conditions.[6] Their second proffered explanation for this curvature was that ^{125}I-α-bungarotoxin had been displaced by antibody at low concentrations of acetylcholine receptor. If this were the case then analysis by Scatchard plot alone is clearly unsuitable. Dr Vincent and Dr Newsom-Davis have themselves shown that antiacetylcholine receptor is polyclonal and that the acetylcholine receptor molecule has multiple determinants capable of binding antiacetylcholine receptor.[7]

In this context we find their comments on avidity or "affinity" somewhat surprising. A detailed examination of their results shows that they contribute little if anything to the study of anti-acetylcholine receptor affinity but confirm our views that Scatchard plots should not be used, at least in the circumstances where they were applied by Dr Vincent and Dr Newsom-Davis. Many of their "linear" Scatchard plots would have been best fitted by a curve. The drawing "by eye"[8] of a line of best fit between three or four points does not prove that a Scatchard plot is linear. Indeed, the fact that 11 data points were available on our figure (29 January, p 338) may have helped the recognition of curvature. In conducting their experiments[1 8] Dr Vincent and Dr Newsom-Davis incubated sera with acetylcholine receptor from "2-72 hours, usually 6-8." They showed differences in avidity dependent on time of incubation, but it is not clear whether all their data were obtained under the same conditions.

Bray and Drachman[6] have also used Scatchard analysis to determine antiacetylcholine receptor "affinity." These authors, however, calculated the coefficient of determination for each Scatchard plot to ensure linearity. The high dilutions of high titre sera which were used may have contributed to the linearity.

It is clear that if Scatchard analysis is to be used to determine affinity then the coefficient of determination must approach unity.[6] If not, then saturation analysis[4] may provide a better estimate and will at least allow comparison of antibodies with linear and non-linear Scatchard plots. Whichever method is used it is essential to use many data points and to ensure that all sera are assayed using similar conditions. We do not believe that the data of Dr Vincent and