pines, which raise seizure thresholds and reduce seizure durations; the use of minimal currents with unknown consequences to seizure pattern and duration; and the failure to monitor seizures, both to describe the electrographic durations and patterns and to assure us that an effective fit did, indeed, occur. The last factor is particularly important. In their survey of British practice, Pippard and Ellam[15] found that even sophisticated observers could misjudge seizure duration and decide that a short fit was "effective," when it was not.

The controversy about seizures and currents will continue until seizure durations and seizure patterns are monitored objectively and researchers determine the parameters of the fit that have the highest correlation with antidepressant efficacy. A change of focus is needed—from the electric current (and, in the United States, the electrode location) to the biochemical, neurohumoral, and electrographic factors in the fit which accompany and which facilitate clinical change.

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Clinical range of neonatal rotavirus gastroenteritis

SIR,—I read with interest the paper by Dr J Dearlove and others (7 May, p 1473) concerning rotavirus gastroenteritis. In the past four months rotavirus has been identified by enzyme immunoassay in nine out of the 10 babies who died from sudden infant death syndrome whom I examined and in neither of the two infants who died from other causes in the same period. In no case was there morphological evidence of definite enteritis but in four of the cases of sudden infant death syndrome, including the rotavirus negative case, there was evidence of an upper respiratory infection, with *Haemophilus influenzae* being cultured in three. This is similar to the findings of Yolken and Murphy in Baltimore.[1] No other viruses were isolated.

The significance, if any, of rotavirus in sudden infant death syndrome is not apparent, but I would be interested to know if other colleagues have had any similar experiences.

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SIR,—Dr J Dearlove and others (7 May, p 1473) suggest that "neonatal rotavirus infection may occasionally cause severe gastrointestinal problems." We would like to add our own similar experience of rotavirus infection in a neonatal intensive care unit, which was gained while undertaking a prospective study of non-bacterial infection in preterm babies between November 1981 and September 1982.

Rotavirus was detected by immunoassay (Rotazyme, Abbott Laboratories) and confirmed by electron microscopy in the stools of 5/170 babies. In two cases rotavirus infection was subclinical, a feature which has been reported previously,[1] but among the other cases gastrointestinal infection was associated with necrotising enterocolitis in two babies and bloody diarrhoea in the remaining case.

Although these numbers are small, we believe that they may be important in providing corroborative support to the evidence provided by Dr Dearlove and others, in that they come from a unit where rotavirus infection is not endemic, thereby preventing the suspicion that infection was coincidental.

Rotavirus infection was identified, however, in only two of the eight cases of necrotising enterocolitis studied during this period, and it is clear that a multicentre study is needed to collect sufficient cases of necrotising enterocolitis and bloody diarrhoea in newborns to evaluate properly this association.

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Scoliosis in the community

SIR,—I was concerned to read the letter by Dr Andrew G King (30 April, p 1442) about my article on scoliosis in the community (19 February, p 615), as it was precisely this confusional state about terminology, the difference between screening and early detection, and the fundamental principles of epidemiology that my article was intended to clarify.

The wise and experienced men on the terminology committee appreciated full well that in defining idiopathic scoliosis as "a structural spinal curvature for which no cause is established"[1] reconsideration would be indicated as and when epidemiological research produced more information. Accordingly, when 40% of scolioses detected in the community are attributable to an inequality in leg length it would be incorrect by any terminological standard to regard all scolioses in the community as idiopathic. The term "schooliosis," which Dr King regards as flippant, is therefore a splendid word and was indeed coined by a distinguished past president of the Scoliosis Research Society, although unfortunately he was never a member of the terminology committee.

There is regrettably little evidence concerning the validity of screening,[2] and there are no data about sensitivity and specificity as published programmes do not include information referable to false negatives. Important prerequisites for a screening programme are that it should make better use of finances than available alternatives and that there should be a satisfactory form of treatment.[3] The former is certainly not the case, and the latter is questionable.

With curve magnitude at presentation so high, nobody would dispute the need for early detection. What is being disputed is whether screening is the way to go about it or whether these funds should not be channelled in other directions. A recent review by an academic epidemiological team came to conclusions similar to my own.[4] They stated that there is an urgent need to coordinate and increase research designed to determine the aetiology, incidence, prevalence, and course of idiopathic scoliosis and that this is where resources should be directed.

Dr King therefore has a lot of homework to do, and I would suggest careful perusals of *Epidemiology for the Uninitiated*[5] and the review by Professor Warren and his colleagues, both compulsory reading for all those interested in the epidemiology of scoliosis and screening fanatics in particular. An admirable starting point, however, would be my recent article in the *BMJ* (19 February, p 615) entitled "Scoliosis in the Community."

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Underdiagnosis and undertreatment of asthma in childhood

SIR,—The recent articles by Dr A N Speight and others (16 April, p 1253) and Dr D A Lee and others (16 April, p 1256) clearly show the underdiagnosis of asthma in children; but this is a problem that applies equally to adults. In children the definition of asthma in the purely functional terms of the Ciba Foundation[1] has led to the recognition that recurrent "bronchitis" and asthma in childhood are virtually always the same thing. Paradoxically too literal interpretation of the Ciba definitions in adults has resulted in an artificial separation between asthma on the one hand and chronic airway obstruction on the other, leading to the misconceptions that obstruction in asthma is always potentially completely reversible and that there is no variability in the chronic obstruction associated with chronic bronchitis.

Indeed, it has become increasingly apparent that in some patients with asthma the obstruction may be partly reversible, particularly in the elderly. This can be seen both after an

acute attack[2] and in long term management.[3] Equally, in many patients with airway obstruction related to chronic bronchitis the obstruction may be partly reversible. This can be shown both by an acute response to bronchodilators[4] and by increased circadian variation.[5] These facts have important implications in both treatment and epidemiological studies.

Perhaps the time has come to return to making a diagnosis in the form of a clinical syndrome—for example, "atopic asthma" or "airway obstruction related to cigarette smoking"—and defining the obstruction more precisely but independently. Ventilatory function should be described in terms of (*a*) the best lung function which can be achieved and the method used to achieve it and (*b*) the extent of the variability in function and the method used to demonstrate it. For example, one might describe as an atopic asthmatic a patient whose ventilatory function will return to 90% of normal with corticosteroids and shows a variability of 25% as shown by spontaneous variation of peak expiratory flow rate. This encourages the trial of each method available to achieve as near normal function as possible in every patient. At the same time it gives a realistic background on which to judge the success or failure of long term management.

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SIR,—Dr D A Lee and others (16 April, p 1256) point out the difficulties with studies endeavouring to estimate the prevalence of asthma in childhood and show how careful these studies must be.

They found a prevalence of attacks of wheezing of 11·1% with a ratio of twice as many boys as girls. In most epidemiological surveys this sex ratio is more than one, but not in all.[1] The reasons for this inconsistency in sex ratio are less clear than those for prevalence, which can probably be explained by differences in methodology, as emphasised by Dr Lee and others. The French National Survey, PAARC, looking at the relation between air pollution and chronic respiratory disease, enabled us to look at this point.[2] We examined 1495 boys and 1443 girls aged 6-10 living in seven French towns whose parents were not manual workers. One member of the family (generally the mother) provided an answer to the question: Has your child ever had attacks of asthma? There were 7·7% positive answers for boys and 5% for girls (sex ratio = 1·5, p = 0·01). The prevalences varied according to social class (as already reported in some papers,[3] but not in that of Dr Lee and others) and level of education of the parents (which has not been considered in previous studies). The sex ratio, however, was even more variable.

The table shows that in families where neither parent had obtained the baccalaureat diploma the sex ratio was 2·2. It was close to one when both parents had obtained this degree, but less than one (0·7) in atypical families in which the mothers were better educated than their husbands. When the mother lived alone we found a ratio of 2·8 when the mother did not have the baccalaureat diploma and 0·5 when she did. The lowest sex ratios, 0·7 and 0·5, were not significantly lower than one, but the numbers of children were very small. These differences in sex ratio remained after controlling for the ages of both mothers and children, to which it was also slightly related.

Sex ratio of the cumulative prevalence of asthma according to educational level of the parents. (Number in parentheses = number of children)

	Level of education of mother	
	Without baccalaureat diploma	With baccalaureat diploma
No husband	2·8* (227)	0·5 (94)
Husband without baccalaureat diploma	2·2* (1254)	0·7 (163)
Husband with baccalaureat diploma	1·2 (468)	1·2 (577)

*Significance of sex ratio: p = 0·01.

These results suggest that cultural factors play a role in answers to questions on children's asthma and that that role could be different with respect to sons and daughters. They emphasise the need for studies including both objective measurements, like that of Dr Lee and others, and cultural variables in order to better understand the history of objective and perceived asthma.

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Tuberculosis in unvaccinated children, adolescents, and young adults

SIR,—Last year we had an outbreak of tuberculosis similar to that described by Dr J D Hill and Dr D K Stevenson (7 May, p 1471) but fortunately ours was much smaller.

In February 1982 a two year old white boy was admitted with miliary tuberculosis complicated by severe meningitis. No source case was found despite extensive contact screening. Two months later a 20 year old man, who had had symptoms for nine months, was found to have cavitating disease with a positive smear. He frequented a shoppers' bingo and amusement arcade, where he also worked occasionally. His contacts from the arcade were screened, and a further five cases were found, all of whom were white with no history of BCG vaccination (table). Inquiries revealed that the first patient was taken regularly to the arcade.

Details of patients who were traced as contacts

Age (year)	Sex	Type of tuberculosis	Comments
23	F	Positive smear; cavitating	Symptoms for two months
3	F	Primary	Attended arcade with mother
18	F	Primary	Regular arcade attender
3	M	Primary	Manager's son
18	M	Pleural effusion	Grade 4 tuberculin test on screening. Refused *x* ray; presented four months later

Our district health problems with regard to tuberculosis are similar to those in Bradford. We fully support the view of Dr Hill and Dr Stevenson that an efficient contact tracing service is much more effective than mass *x* ray examination of casual contacts and that discontinuation of the routine BCG vaccination may well be a false economy in the longer term. Tuberculosis, as shown by small outbreaks in this district and others mentioned by Dr Hill and Dr Stevenson, remains a contagious disease in those without immunity.

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SIR,—One particular aspect of the report by Dr J D Hill and Dr D K Stevenson (7 May, p 1471) struck me as curious. They describe 41 cases, of whom six were receiving "prophylactic chemotherapy." These they describe as children with grade 4 reactions on Heaf testing and normal chest radiographs.

In the new procedures of notification,[1] which they quote, the section on chemoprophylaxis is quite clear that "cases of chemoprophylaxis should not be included in notifications of tuberculous disease. . . . They [that is individuals receiving chemoprophylaxis] are reported separately from the cases of tuberculosis."

In the course of a recent survey of tuberculous notifications we found that approximately 3% of all notified cases were in fact individuals being given chemoprophylaxis and could not, therefore, truly be described as patients suffering from tuberculosis.[2] It therefore seems that, despite the efforts of the British Thoracic Association subcommittee report, individuals being given prophylactic chemotherapy are still being counted as cases of tuberculosis. We cannot have it both ways. Either these individuals were suffering from tuberculosis and were therefore receiving treatment not prophylaxis, or they were receiving prophylactic chemotherapy, in which case they were not cases and should not have been reported as such. Until this subtle but important point is appreciated by all physicians treating and notifying tuberculosis, the statistics for tuberculosis will remain inaccurate