problems, will not be discovered at the neonatal examination and are rarely noticed by parents of preschool children. They are some of the most important *raison d'être* of screening programmes. They are rarely diagnosed under 2 years of age,¹⁻³ although the yield between 3 years and school age is high.^{3,4}

If you are a member of a family with an inheritable visual disorder it is likely that you will worry about the visual acuity of your offspring. Furthermore, having one handicap doubles the likelihood of another. This explains the high percentage of children in the study whose initial diagnosis was made by their parents or a paediatrician examining a second major handicap. There is a difference in "ophthalmic health" in metropolitan areas and inner city and rural areas of the world⁵ and thus a need for vigilance even if "families of low socioeconomic state are perfectly capable of making relevant observations about defective vision."

It is wrong to conclude that because most of the children studied were not diagnosed at clinics for developmental screening such clinics make little contribution to the diagnosis of visual problems. After all, maternal suspicion of deafness is an important pointer to early diagnosis,⁶ but no one has suggested that screening for hearing loss should be abandoned.

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- 2 Gardiner PA. The development of vision. In: Pollak, ed. *Studies in developmental paediatrics*. Vol 3. Lancaster: MTP Press, 1982.
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- 4 Smith VH. Birmingham orthoptic screening service. In: Smith V, Keen J, eds. *Visual handicap in children*. London: Spastics International, 1979.
- 5 Jones BR. Social responsibilities. *J R Soc Med* 1985;78:358-66.
- 6 Martin JAM, Moore WJ. Childhood deafness in the European community. Brussels: Council of the European Community, 1979. (EUR 6413.)

AUTHORS' REPLY.—The early detection of severe visual defects should be one of the aims of a surveillance programme. In our study most of these were diagnosed by the age of 6 months. We are not, however, making a plea for more accurate screening tests for disabling defects since they are detected by other means.

Suitable tests do exist for screening 3 year old children, but we do not agree that myopia and other refractive errors are "the most important targets." The discovery of a refractive error uncomplicated by amblyopia at the age of 3 years instead of at school entry is of uncertain, but probably marginal, benefit. Detection of amblyopia is more important. We cannot prevent it, so detection and treatment are the best we can offer.

The existence of a suitable test does not guarantee that the screening programme is effective.¹ Screening 3 year olds produces many referrals, but the true yield (the number of new cases requiring treatment) is much smaller. Elementary errors of technique are frequent. For instance, the single letter test is still used, although it has been known for years that it misses or underestimates amblyopia.

The analogy with screening for deafness is unfortunate. The distraction test for deafness at 7-8 months is hardly a success story. Unlike visual screening tests for infants, it is potentially sensitive and specific, but the quality of testing in many places is so low that some authorities are suggesting that it should be discontinued. A badly performed screening test does more harm than good because it

provides false reassurance to worried parents. The traditional skills of taking an accurate history and listening carefully to what parents have to say will contribute more to the early diagnosis of visual defects and other developmental problems than the continued use of unsatisfactory screening tests.

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- 1 Stewart-Brown SL, Haslum MN, Howlett B. Pre-school vision screening; a service in need of rationalisation. *Arch Dis Child* 1988;63:356-9.

Editorial freedom

SIR,—There are few editors who would quarrel with the cogent points put forward by Dr Stephen Lock (12 March, p 733). From a South African viewpoint there was also a certain wry pleasure in reading that "outsiders who interfere in the internal affairs of others risk rebuke for their presumption and for exacerbating difficulties."

However, his article contains one important error, where he states, "The editor of the *South African Medical Journal* is forbidden to publish letters or articles from members of the National Medical and Dental Association." As the current editor I wish to put the record straight.

The executive committee of the Medical Association of South Africa did pass a resolution advising the editor of the *South African Medical Journal* not to accept any material from the National Medical and Dental Association. The rationale was that as the National Medical and Dental Association appeared to be losing no opportunity to try to discredit the Medical Association of South Africa, both in South Africa and overseas, it made little sense to give free publicity to it in the Medical Association of South Africa's own journal.

This decision, which was originally intended to cover only advertising, was reversed when the threat to editorial independence was perceived. Last year I stated clearly and unequivocally in an editorial¹ that contributions to the *South African Medical Journal* would be judged and accepted at the editor's discretion solely on the basis of their quality and relevance and not on the source from which they emanated. I would like to reassure Dr Lock and the readers of the *BMJ* that that policy has not changed.

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- 1 Anonymous. Serving the Truth [Editorial]. *S Afr Med J* 1987;72:371.

**This letter is substantially the same as one published in the *Lancet* of 23 April (p 941). We publish it, with the *Lancet's* permission, to give Dr Lee the chance to reply to a criticism made in the *BMJ*.—ED, *BMJ*.

Risk of upper respiratory tract infection and malaria prophylaxis

SIR,—Drs Pheng Soon Lee and Edwin Y L Lau (26 March, p 893) conclude that differences in the risk of upper respiratory tract infection in the military recruits described was due mainly to the malaria prophylaxis given to one group. This is based on the false presumption that other factors have been compensated for adequately.

Despite the geographical similarity and proximity of the two camps there remains a sufficiently great environmental difference between them for malaria prophylaxis to be required at only one. Surely risks for other infections might also differ.

Other variables which are unaccounted for may be important. Psychological stress is a well recognised cause of immune suppression.¹ Examination students under stress have been shown to have impaired cellular immunity² and reduced production of IgA.³ In the latter study there was a higher incidence of respiratory disease during the period of stress. The impact of personality factors was also assessed. Students exhibiting a "high inhibited need for power" had concentrations of IgA that continued to fall even when the amount of stress decreased. Officer recruits undergoing greater physical and psychological stress than other recruits might be affected similarly.

Greater physical activity has not been excluded as the cause for a higher risk in the officer cadet group. The reasoning that if it were related the increased risk of sprains and of upper respiratory tract infections should be identical is flawed. This is no more logical than saying that jogging should increase the risk of ankle injuries by exactly the same percentage as it increases the risks of being run over by a car. I would be reluctant to draw any conclusion about the relevance of dapsone-pyrimethamine as a cause of immunosuppression on the basis of the authors' findings, and I hope that prescribing practices are not altered.

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- 1 Anonymous. Emotion and immunity [Editorial]. *Lancet* 1985;ii: 133-4.
- 2 Dorian B, Garfinkel P, Brown G, et al. Aberrations in lymphocyte subpopulations and function during psychological stress. *Clin Exp Immunol* 1982;50:132-8.
- 3 Jemmott JB, Borysenko M, Chapman R, et al. Academic stress, power motivation, and decrease in secretion rate of salivary secretory IgA. *Lancet* 1983;ii:1400-2.

Postviral fatigue syndrome

SIR,—Dr A V Mark Hughson (9 April, p 1067) suggests that our arguments have been overtaken by a report which was published after our paper was accepted.¹

In that paper enteroviral infection was found in 17 of the 76 (22%) subjects with postviral fatigue syndrome, and it persisted in only five (7%) at one year. In a second sample of 87 people with postviral fatigue syndrome an enteroviral antigen was detected in 44 (51%), and this remained in 39 (45%) at four months. Despite the detailed virological studies, we think that this investigation exemplifies some of the shortcomings in "the old approach" to this condition. There are scant details on diagnostic criteria and no information on case selection or why there were two samples. Using volunteer control subjects, enlisted by the cases, may have led to biases that could account for the differences. The importance of these results will emerge only with precise information about the population from which cases were drawn and the methods by which they were selected.

One of the main aims of our paper was to encourage discussion of the definition of cases. Dr Hughson points out that we risk underdiagnosing the disorder. This caution is appropriate for research purposes but it does not mean that patients with some features suggesting the syndrome need be denied the best care in clinical settings. This may include prescribing rest for all suspected cases, but such instructions, which have anecdotal support, must be weighed against the potentially severe complications of prolonged inactivity.² The role of controlled exercise in the