Screening for visual defects in preschoolchildren

Sir, I would like to comment briefly on the paper by Ingram et al., 'Screening for visual defects in preschool children.' Dr Ingram and his associates continue to raise important questions and make excellent empirical contributions to the often difficult-to-study area of child vision screening. They also have been careful in the present report to qualify their findings in several respects. Nonetheless, in view of the strong statement of there being 'no evidence' (p. 16) to support the need for preschool screening, some further comment seems appropriate.

More than a quarter of the population studied (26%, Table 1) had a condition of abnormal vision detected prior to the study's screening. Since this group presumably presented with the most serious and hence most readily detected abnormalities, they would represent the most important target group in an unselected population screening. In effect removing them from the screening sample of the present study thus limited the study population to the less serious, and hence less important to detect and treat, segment of the abnormal vision subpopulation. This limitation is an important one, because whether this prescreening detected group would have been detected without screening in other socioeconomic or eye care delivery conditions as applied to this study is an open question. In one large-scale survey of 12–17 year olds in the USA, for instance, less than a third of the individuals detected with strabismus indicated any history of treatment.

Of those who presented with visual problems after the screening at age 3 1/2, 43% apparently passed the screening and are therefore classified as 'false negatives' of vision screening (p. 18). Under-referral is, of course, a serious matter for a screening programme for both credibility and medico-legal reasons. It thus seems important to emphasise that this false negative rate was derived, not from vision screening generally, but from the particular screening methodology used in this study, which comprised cover test, acuity, and retinoscopy. The shortcomings of all three at detecting both amblyopia and strabismus have been discussed elsewhere, and Dr Ingram and his associates in their own findings have pointed out the shortcomings of refraction in detecting strabismus.

Broader conclusions about screening utility would have been justified if a reliable measure of binocular visual function had been included, in view of other evidence of such measures' effectiveness in screening. An attempt was made to use the RDE random dot stereotest, but this was abandoned since the test was found to be 'unreliable' (p. 18), and a large proportion—22%—of the children tested were reported as not attempting the test or guessing. Unfortunately, no details are reported on details of the test administration methodology or the nature of this unreliability, which makes detailed comment impossible. In view of my own involvement in the development of this particular test I do not wish to appear partisan. However, it does seem fair to point out that studies by others as well as myself have found the RDE to be quite reliable and easy to use with young children. Guessing certainly should not have been a problem if there were adequate repetitions and the test plates not marred so as to provide a nonstereoscopic cue to children. Furthermore, it is not clear why another stereotest such as the TNO14–15 or right hand plate of the Randot II16 was not substituted if the RDE was not found to be satisfactory.

Finally, the rationale for screening for amblyopia is challenged on the basis that only 3/16 of the detected patients achieved 6/6 vision from occlusion (Fig. 1), an outcome termed 'not very encouraging' (p. 20). Exactly the opposite point of view could be supported, however—that the outcome was in fact encouraging and the screening thus justified—on the basis of the fact that 15/16 patients did show some improvement from occlusion (Fig. 1). Indeed, if only a 6/6 result is considered adequate, much of the standard clinical treatment of amblyopia must be questioned.

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References
13 Gruber J, Dickey P, Rosner J. Comparison of a modified (two-item) Frisby with the standard Frisby and random-dot E stereo-

Sir, Thank you for allowing me the opportunity to reply to Dr Simons’s letter. Perhaps I may answer, one by one, the points he raises.

Firstly, we cannot say that those in our sample who presented before 31/2, almost inevitably with a cosmetically noticeable squint, would necessarily have done so in other socioeconomic conditions. The particular set-up we have in that part of our district could have predisposed us to the findings we reported.

Secondly, I accept that our ‘false negative’ rate was derived from the screening methods we used. 26% of those ultimately found to have abnormal vision had a ‘false normal’ visual acuity and/or cover test when screened at 31/2. Likewise, 20% of those ultimately found to have abnormal vision, had a ’normal’ refraction at 31/2 and would have been ‘missed’ by this test.

The question of ‘false positives’ is equally important, because they cause increased and unnecessary parental concern, work and financial expense. In our series 56–7% of those who recorded normal or uncertain vision at 31/2, and 51–2% of those who had an abnormal refraction (using our criteria) at 31/2, actually had normal vision without any treatment. The majority of the ‘false abnormal’ refractions were hypermetropia without astigmatism or anisometropia. I have since realised that, if we had chosen only astigmatism and/or anisometropia as the criterion for abnormal refraction, the percentage of false positives using refraction would have been reduced to 29–5%. If we had combined this with a cover test, I calculate we would have identified 85% of those with abnormal vision (excluding myopia). It might be possible to simultaneously test for an abnormal refraction and squint using the more modern developments of photo refraction. Nevertheless, some false positives and false negatives are both inevitable whatever method is used.

Thirdly, Dr Simons justifiably questions our method for stereocuity testing. Maybe guessing and inadequate cooperation should not have been a problem, but they were. We found interpretation of the response to be too unreliable for use as a one-off, non-repeated screening test in the population sample we screened. This does not mean to say that others would also find this so.

I will admit to being slightly ‘partisan’ against testing for stereocuity, but no more than I am against any other ‘subjective’ screening test. This is because in most cases defective visual acuity is associated with an abnormal refraction and/or a squint, and therefore I think that an objective test should be more accurate than a subjective one, particularly in preschool children. Having said that, we have not recommended screening by refraction—we merely advised against screening at 31/2 by any method.

Finally, our views on the results of occlusion in these children are questioned again. I do not think that improvement to 6/6 is necessary before one can conclude that occlusion has been valuable. However, I wonder whether one or two lines improvement in the range 6/18 to 6/9 is worthwhile, and I do believe that the standard treatment of amblyopia should be questioned.1

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Reference

Adverse reactions to guanethidine eye drops

Sir, I write to express my regret that guanethidine 5% with adrenaline 1% eye drops have been voluntarily withdrawn by the company responsible for their manufacture.

I have for some years been concerned about the rare effects of antiglaucoma medication and other topical eye therapy on the conjunctiva and cornea.12 There is no question that these adverse effects are rare, unpredictable, and not normally dose or time related.

All topical therapy, in common with other medicines, carries both benefit and risk, and it is the job of the prescriber to calculate this risk/benefit ratio for any patient receiving treatment. I understand that at least 1000 patients whose glaucoma was currently controlled with guanethidine 5% plus adrenaline 1% have now been deprived of useful therapy. I believe that it would have been more appropriate for the company to insert a warning in the package advising that the combination had been associated with conjunctival cicatrization and advising that prescribers should monitor the outer eyes of patients receiving drops. In addition to depriving a large number of patients of useful treatment the decision to withdraw the drops also creates an unreal impression of safety. Any topical therapy can produce an unacceptable adverse reaction, and all prescribers will need to continue to watch for signs of these adverse effects, which are not limited to guanethidine and may even be associated with common constituents such as preservatives.

Responsibility for monitoring adverse effects as well as benefits will always be the prime duties of a prescribing doctor. PETER WRIGHT
Moorfields Eye Hospital, London EC1.

References

Book reviews


This book was first published in 1978 and the revised edition includes chapters by a number of authorities to augment the