Sir,

Regarding the UK guidelines for screening for Retinopathy of Prematurity (ROP),[1] Fleck *et al.*[2] stated 'The need to include infants of birthweight >1250 grams should be reviewed at a future date'. This followed their finding that no babies heavier than this developed threshold ROP in their 4-year study. Also, Goble *et al.*[3] have recently questioned the need to screen larger/older neonates for ROP. On the basis of a large population study they suggest reducing the upper limits for screening to 1250 g weight and 29 weeks gestational age at birth, with the possible inclusion of older babies in screening programmes if certain sickness criteria are met (e.g. severe neurological insult or severe blood loss). We should like to present a case of severe ROP in a baby considerably outwith these birthweight and gestational age criteria who did not meet either of these sickness criteria.

A 1440 g Caucasian baby girl was born in the UK in 1996, at 30 weeks and 4 days gestation. Respiratory distress developed 7 h post-partum, requiring ventilation for 2 weeks. During this she developed a left pneumothorax which was drained. For the first 4 days she was monitored with a transcutaneous pO_2 monitor with readings about a mode of approximately 9.4 kPa with a maximum of 11.6 kPa. Subsequently a saturation monitor was used with measurements between 94% and 99%; some supplemental oxygen was given but whenever saturations went above 96% room air was used.

Routine ophthalmic screening at 6 weeks of age found 5–6 clock-hours of stage II ROP at the zone 2/3 junction bilaterally, which progressed over 2 weeks to stage III plus in the left eye and stage IVa in the right. The staging was agreed by a second consultant to meet the accepted definition of 'threshold disease'.[4] Peripheral retinal cryotherapy was administered to both eyes with good regression of the ROP. There was no family history to suggest any inherited cause for retinopathy, and ophthalmic examination of the parents revealed that they had normal optic fundi with no evidence of familial vitreoretinopathy.

Goble *et al.* advise caution in the tightening of screening criteria and suggest inclusion of larger babies with a turbulent clinical course. The occurrence of threshold ROP in our case emphasises the importance of this. We suggest that the definition of 'sickness criteria' requires further study as our baby did not meet those criteria suggested by Goble *et al.*

Bagdoniene and Surtautiene[5] have recently presented data showing that ROP remains a significant problem in babies weighing considerably over 1500 g in Lithuania, indicating that ROP incidence has significant geographical variations. Even within the UK such variations are seen; Goble *et al.* compare their data with earlier studies including one by the same observer in a different part of the UK[6] where the incidence of severe disease was higher. They suggest that this variation may relate not only to neonatal survival but also to standard of care and ethnic mix. Published data for the Northern Region of the UK[7] indicate an incidence of threshold ROP of at least 3.6% in infants born at less than 32 weeks' gestation, compared with 2.2% found by Goble *et al.* A study of sickness criteria would therefore have to be conducted within a geographically defined population, and its findings would be applicable only to that population. The British Ophthalmic Surveillance Unit (BOSU) is currently acquiring data on all UK babies developing stage III ROP; it is hoped this will help to define screening criteria appropriate to the UK.

Until sickness criteria are clearly established we believe it would be dangerous to alter the current Royal College of Ophthalmologists screening guidelines.

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Sir,

I read with interest the article 'The association between the oculocardiac reflex and post-operative vomiting in children undergoing strabismus surgery' by Allen *et al.*[1] We have used a sub-Tenon's local anaesthetic block with 2 ml 0.5% plain bupivacaine, after surgery but prior to awakening, in all our patients for squint surgery for the past 2 years. Forty-five of these patients were prospectively audited for a 24 h period. A zero incidence of vomiting was achieved.[2] All patients received a standardised anaesthetic avoiding opioids, relaxants and reversal agents, and spontaneously breathing gas, oxygen, isoflurane or sevoflurane on a laryngeal mask. In addition all received non-steroidal pain relief, paracetamol and topical amethocaine intraoperatively, plus two anti-emetics (ondansetron and either metoclopramide or perchlorperazine). We have also assumed that the marked reduction in vomiting is the result of a blockade of the oculocardiac/

oculoemetic reflex, despite previous reports including those by Cousins and Bridenbach[3] of these reflexes occurring up to 1–11 2 h after retrobulbar blockade. We have, however, previously recorded a zero incidence of vomiting in a group of patients in whom sub-Tenon's blocks were not routinely used.[4] We are therefore currently commencing a formal double-masked study in paediatric patients to investigate the importance of the anti-emetics in our anaesthetic protocol in producing our low incidence of vomiting to date. We look forward to the results from Dr Allen's study with great interest.

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Sir,

We thank Dr Fry for his interest in our paper and congratulate him on the very low incidence of post-operative vomiting following strabismus surgery in his unit. Whilst the use of systemic anti-emetics such as ondansetron and metoclopromide certainly reduces the incidence of this complication, their cost and potential to cause side-effects in children limit their routine use in the UK. Prolonged post-operative nausea can be just as unpleasant for the patient as vomiting, and we feel that it is important to assess its incidence and severity when comparing anti-emetic regimens.

In theory, sub-Tenon's anaesthesia may obviate the need for systemic anti-emetics by the blocking of the oculocardiac reflex. Our current study is a formal, double-masked controlled trial in which the incidence and severity of post-operative nausea and vomiting in children receiving sub-Tenon's anaesthesia is compared with that in patients receiving our standard anaesthetic regimen (which does not include a prophylactic systemic anti-emetic agent). Since Dr Fry's study will compare the efficacy of sub-Tenon's anaesthesia with prophylactic systemic anti-emetic agents, the two studies should complement each other and we look forward to hearing Dr Fry's results.

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Sir,

We read with interest the recent paper by Zambarakji *et al*.[1] The authors claim that silicone implants produce a more pronounced degree of capsulorhexis phymosis following phacoemulsification.

This claim is based firstly on the fact that capsular areas in patients with silicone implants were significantly smaller at 6 weeks and 6 months than in those with PMMA implants. However, from the data it would appear that patients with a silicone implant had a smaller mean capsular diameter *at the outset* (4.52 ± 1.24 mm at day 1) compared with those with a PMMA lens (5.10 ± 1.00 mm). Has this been taken into account? A smaller capsulorhexis may in itself increase fibrosis due to the larger number of remaining anterior capsule epithelial cells, as these cells play a role in capsule fibrosis.[2]

Secondly the authors have compared the *percentage change* in capsule reduction over the two time intervals studied between the two lens types. They appear to have performed a direct statistical comparison of these two percentages (their Table 1) – an analysis which is unorthodox and inconclusive.

In addition it is not stated whether patients were randomised to each of the two surgeons or indeed if the type of lens implanted was randomly allocated. Given that the procedure of capsulorhexis is an idiosyncratic one, the behaviour of the anterior capsule may be affected by the surgeon rather than the IOL type.

Finally it is difficult to directly compare the two lenses, the PMMA lens used being a rigid one-piece lens with a 5 × 6 mm haptic whereas the silicone IOL used was foldable with a larger optic size. Several variables are at play regarding the effect of these lenses on the tensile forces within the capsular bag (Gonvers *et al*.[3] appear to demonstrate an increased constriction of capsulorhexis with a plate rather than loop haptic), so any significant difference between lenses may be due to IOL design rather than composition.

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Sir,

We thank Walsh *et al*. for their comments on our paper.[1] Our study[1] is purely an observational study. We have measured the rate of anterior capsular contraction following uncomplicated phacoemulsification surgery, and observed that this is greatest during the first 6 weeks following surgery even when measured in percentage per 6 weeks. If instead we measure the change in square millimetres, then there would still be a decrease, and the contrast (between the first 6 weeks and the following 41 2 months) would be greater because x% of a large area is more square millimetres than x% of a smaller area.

The authors do not claim that silicone implants produce a greater degree of capsular phymosis; indeed the phenomenon of capsular phymosis is a multifactorial one.[2,3] This may be affected by the surgical technique, as well as factors relating to the patient and intraocular lens (IOL). We have therefore attempted to eliminate patient factors by excluding those with a history of diabetes, glaucoma, pseudoexfoliation, ocular inflammation