

**Conflict of interest**

The authors declare no conflict of interest.

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Sir,

**Comment on 'Idiopathic uveal effusion syndrome causing unilateral acute angle closure in a pseudophakic patient'**

I read with interest the above communication by Bhogal *et al.*<sup>1</sup> published recently in the *Eye* journal.

The authors presented images of B scan ultrasound, correctly demonstrating choroidal effusion as the primary trigger in precipitating angle closure glaucoma in their patient. The anterior segment OCT images however failed to show the anatomical mechanism of angle closure, which, in many cases, is caused by the anterior rotation of ciliary body and most probably associated with annular ciliary body detachment. These findings would have been best illustrated by high frequency ultrasound (HFU). The value of HFU in cases of pseudophakic pupillary block and other post-operative ciliary body abnormalities was demonstrated by us in previous publications.<sup>2,3</sup>

Anterior segment OCT is an 'optical scan' and therefore obeys the simple optical principle of inability to penetrate through opaque media. This is the domain of ultrasound. It is tempting to use anterior segment OCT in many clinical situations, as it is noninvasive and easy to use. Anterior segment OCT produces excellent images of the cornea, anterior iris tissue, trans-pupillary lens and angle configuration. It is however inferior to HFU in imaging of the posterior iris surface, ciliary body,

posterior chamber, zonules, pars plana and periphery of choroid. An excellent prospective observational case series, comparing anterior segment OCT and HFU in the imaging of anterior segment masses, tend to confirm the above assertion and was published by Pavlin *et al.*<sup>4</sup> in 2009.

It is reasonable to recommend to readers that whenever imaging of the ciliary body is desirable, then HFU should remain the technique of choice.

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The author declares no conflict of interest.

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Sir,

**Response to 'Shield or not to shield? Postoperative protection after modern cataract surgery'**

We read with interest the correspondence by Lindfield *et al.*<sup>1</sup> questioning the necessity for the routine use of shields after small incision cataract surgery following a retrospective review of local practice, and feel that it raises an interesting point. We would, however, request clarification of a potential confounding factor that was not included in the reported data. The authors make no comment regarding the proportion of corneal sections that were sutured. If either group is disproportionately weighted to using corneal sutures, this could either further strengthen or weaken the author's argument.

Secondly, a 2003 ASCRS survey<sup>2</sup> showed that 72% of small incision cataract surgery was performed through a clear corneal section with only 28% through a scleral tunnel (no UK data available). The cohort of Lindfield *et al.*<sup>1</sup> had a disproportionately high percentage of scleral tunnel patients compared with likely current standard practice.

Finally, the questionnaire response rate was extremely low, representing only 5% of the sample size, and arguably could represent a selection bias, in which patients most likely to fill in a survey regarding the wearing of the shield may have been those who took issue with it.

Nevertheless, we do feel that assuming there was no difference in suturing section rates, the findings of Lindfield *et al*<sup>1</sup> may strengthen a case for conducting an adequately powered prospective randomised control trial to definitively answer their question. In the meantime, we would advise caution to surgeons considering changing their practice regarding shields, until we have sufficient evidence to dismiss the benefit of their use.

### Conflict of interest

The authors declare no conflict of interest.

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### Sir, Response to Banerjee and Elgohary

Many thanks to Banerjee and Elgohary<sup>1</sup> for their critical appraisal. I agree that the use of corneal sutures would confer a wound advantage. Not including this data was an oversight. No scleral tunnels required suturing in either group. For the shieldless group, clear corneal wound construction consisted of either two-step or three-step technique depending on surgeon. For the shieldless group, one corneal incision was closed with a single 10/0 nylon suture ( $n = 127$ , 0.79%). The shield-wearing group included two wounds secured with 10/0 nylon ( $n = 314$ , 0.64%).

In all, 70% of procedures in our audit were performed through scleral tunnels. This is reflective of our standard practice and because of surgeon preference. Reasons are familiarity and a possible endophthalmitis advantage of scleral tunnel over corneal incision in the ESCRS Endophthalmitis Study.<sup>2</sup>

The patient questionnaire was not given to all the 1407 patients. It was administered to all patients seen over a 1-month period for follow-up in our nurse-led clinic. All the 46 patients who were approached responded, therefore, we feel that selection bias was minimal.

In summary, we agree that a properly powered, prospective study is required. The intention of this audit

was to stimulate questioning of routine postoperative shielding without corroborating evidence.

### Conflict of interest

The author declares no conflict of interest.

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### Sir, Predictive factors and outcomes for posterior segment intraocular foreign bodies

We read with interest the article on ‘Predictive factors and outcomes of posterior segment intraocular foreign bodies’ by Choovuthayakorn *et al.*<sup>1</sup>

In the results section, 4 (4.76%) of the 84 eyes with intraocular foreign bodies (IOFB) underwent primary enucleation. Given the trend to avoid primary enucleation following trauma, it would have been helpful if the authors had described the indications for primary enucleation in these eyes and why enucleation was preferred over evisceration.

One of the key conclusions drawn, and perhaps the most controversial, is that good outcome can be achieved even if there is delay in removal of IOFB. The incidence of endophthalmitis reported by the authors in this series was 9.1% that, though comparable with the literature, was still quite high. In another case series of 1421 eyes with IOFB by Zhang *et al.*,<sup>2</sup> the reported rate of endophthalmitis was much higher at 16.76%, pointing possibly to the influence of IOFB presence. The two referenced papers<sup>3,4</sup> citing low-risk or no risk of endophthalmitis with delayed removal of IOFB were in the context of war injuries, whereby high-velocity explosives and the heat generated before impact could potentially partially account for the relatively lower incidence of endophthalmitis. Furthermore, there was no mention about the use of intravitreal antibiotics in this case series, which has a major role in prevention and treatment of post traumatic endophthalmitis.<sup>5</sup>

In conclusion, we are concerned that the article may create the impression that delayed removal of IOFB can achieve good visual outcome by giving systemic antibiotics and prompt primary repair. On the contrary,