Correspondence

Injection-site lamp for fluorescein angiography

SIR, Fluorescein angiography is a common and valuable examination in ophthalmic departments. We describe here an addition fitted to several fundus cameras in the departments we serve which has been found to make easier the performance of the test.

It is clearly desirable that the clinician should be able to see the patient's arm during the intravenous injection of the sodium fluorescein solution to be sure that it is given correctly, and yet the room should be dark from the start of the procedure so that extraneous light does not degrade the first frames of the sequential photography. The fitting of a small light to the fundus camera to illuminate only the injection site allows both these requirements to be attained.

The light is shown in Fig. 1. It was adapted from the type

sold as map-reading lamps for motor cars. Depending on the type of fundus camera, in some cases we have powered the lamp from the supply to the fixation light, while on other instruments it has been more convenient to fit a separate battery supply for the new light. In either case the lamp unit was modified to use a twin conductor supply rather than the metal casing as the current return path. The lamp was fastened to the forehead rest of the fundus camera but with freedom to swivel as required. A switch for the lamp was placed for the convenience of the camera operator.

To our knowledge such illumination as this is not provided by any fundus camera manufacturer, but we have found this adaptation to be valued in the clinic and simple and cheap to implement.

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Fig. 1 Injection-site lamp.

'Visual Optics and Refraction'

SIR, In reviewing the literature for the 3rd edition of my text *Visual Optics and Refraction* I have just now come upon Robert Weale's review of the 2nd edition in the September 1981 issue of the *BJO*. If Professor Weale did not like the book, it is his duty to say so. But this duty also implies a reviewer will read the preface and look at the pictures and thus not confuse a textbook with a manual. No one, Weale writes, is going to convince him 'refraction needs over 700 pages.' By what gastric intuition is this deduced? He might have consulted such precedents as Donders, Landolt, or Emsley. Or Helmholtz. Or Duke-Elder vol V. Alas, Weale thinks physiologic optics no more relevant to refraction than the pharmacology of cycloplegia.

In the only substantive comment Professor Weale says he is 'all in favour of the abandonment of 100% contrast test targets' and I fail to make this clear. Had he read more critically he might have noted that I favour just the opposite. The text is further derided for 'an attempt to supplement Adler without his catholicity.' No doubt this sentence means something—if one could but fathom it.

To compare my book to those of Adler and Davson, both excellent physiology texts, neither of which cover optics or refraction, is perplexing. To censure the lack of references in one sentence and complain of an excess in the next is disconcerting. To debate writing style with such old clichés as 'Scylla/Charybdis' and 'suffering paper' (apparently a favoured phrase since it is repeated in other reviews) is neither graphic nor elegant. To quote me, inserting his own comments within the quote, is merely poor taste. But to accuse one of padding a book with extraneous material to jack up the price is insulting. To review a book is one thing, to question the author's integrity is another. Besides,

Correspondence

Professor Weale must be fully aware that no writer is ever consulted about publication price. Weale admits his opinion may be 'a little on the churlish side.' Agreed. Of course, I would have phrased it differently.

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Contrast sensitivity testing

SIR, A recent article¹ reports an important methodological improvement for contrast sensitivity testing with printed gratings derived from the Arden plates.² Vaegan and Halliday¹ used a 4-alternative forced-choice procedure to reduce the influence of response bias, and presented results obtained in glaucoma. However, in discussing these results they inaccurately characterised our previous findings on contrast sensitivity defects in glaucoma.³⁻⁵ The inaccuracies have implications which concern not only testing methodology but also the pathophysiology of vision changes in glaucoma.

Vaegan and Halliday¹ found that 'glaucoma patients . . . have a contrast sensitivity deficit of about 6 db in relation to aged matched normal persons at all spatial frequencies when tested with a conventional oscilloscope display of static gratings' (p. 481).¹ They erroneously conclude (p. 481)¹ that their results 'disagree with the findings of Atkin *et al.*^{3,5} and speculate (p. 490) that 'It is possible that the sample was too small or unrepresentative in many studies which do not find significant differences' (citing Atkin *et al.*^{3,5}). There actually is no disagreement. Our results with static patterns (unpublished) were similar to Vaegan and Halliday's.¹ The static stimuli, which were similar to those used in conventional contrast sensitivity methods,^{1,2} had in fact yielded significant differences between *group* means but had not shown a high enough ability to discriminate *individual* patients from normals to offer promise of a clinically useful test. Therefore in our original abstract we had said that 'sensitivity to the (non-flickering) grating . . . did not distinguish between patients and controls',³ meaning that the groups showed such a large amount of overlap that the static-target method provided little useful information about *individual* patients. Consequently our subsequent papers^{4,5} reported only the results with flickering targets.

With targets flickering at 8 Hz (a method not used by Vaegan and Halliday¹), our studies revealed dynamic components of early glaucomatous vision changes^{4,5} which appeared to be more clinically useful than the static response data. Vaegan and Halliday¹ seemed in fact to recognise this possibility, for they suggested (on p. 490) that 'Atkin *et al.*^{4,5} may . . . have had greater success with phase-alternating stimuli because responses to them are more reliable . . .' Exactly so! With dynamic testing there had been nearly perfect discrimination between glaucoma patients and age-matched control subjects.⁴ We had therefore concluded that the dynamic method might be useful to characterise individuals, not just groups.

It is important to call attention to this methodological difference—the use of *dynamic* rather than static contrast

sensitivity measures. Since our original publications³⁻⁵ there have been several studies corroborating our emphasis on the importance of temporal modulation for discerning early effects in glaucoma, all suggesting that an abnormality in glaucomatous visual responses is apparent with particular clarity at moderate frequencies, e.g. 8 Hz,⁶⁻¹¹ as well as at higher frequencies.⁶

Disparities in test sensitivity between dynamic and static methods may have pathophysiological implications. The higher sensitivity found for tests using flickering targets tends to support 2 of the several pathophysiological hypotheses that have been proposed to account for vision changes in glaucoma. (1) The first hypothesis was that there might be an early differential vulnerability of one component of retinal organisation (perhaps a subclass of retinal ganglion cells) which is especially sensitive when stimulated with fast temporal frequencies and less sensitive when stimulated with static gratings.^{2,8} (2) A subsequent suggestion was that vision changes might result in part from reduced synaptic efficacy within the CNS, due to depression of the rapid component of axoplasmic flow in optic nerve axons.¹² The resulting neurotransmitter depletion could lead to a generalised reduction of visual sensitivity¹² that would be exacerbated by temporal modulation, therefore showing greater effects with dynamic than with static testing.

We offer the foregoing comments to set the record straight about statements in the Vaegan and Halliday paper¹ which could cause confusion.

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