

infiltrate clinically, the safe approach is to always culture the lesions and treat them as potentially infective cases, until proven otherwise. Topical corticosteroids under the coverage of broad-spectrum antibiotics can be given in patients with severe disease and negative cultures to hasten resolution.

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Sir,
Highlighting the methodology

We read with interest the article of Tsang CSL *et al*¹ in which they compared the visual field and optic nerve head changes in normotensive patients with obstructive sleep apnoea (OSA) to age- and sex-matched controls without OSA. Unfortunately, we feel that some aspects of their analysis could have led the authors to invalid conclusions.

Comparing eyes in the two groups, the authors found statistically significant differences ($P < 0.01$) in both visual field (VF) indices using unpaired *t*-tests. However, as most patients have contributed both eyes to the analysis, eye-specific outcomes cannot be considered independent. Failure to account for this dependence results in standard errors that are too small and *P*-values

that are too significant.² For this reason alone, the author's conclusions must be considered questionable. Further, the authors despite having identified deviations from normality in the distribution of both VF indices in the OSA patients have used a test that assumes normality. One approach that addresses both the non-normality and the dependence in the data would be to construct bootstrap confidence intervals (CIs)^{3,4} for the difference in means allowing for clustering at the subject level. The authors have repeatedly used the term CI but none have been reported. From the figures shown, there does not appear to be a large difference in either VF index between the two groups and no estimates of this difference have been provided. Reporting the differences in means with CIs would have demonstrated whether any observed differences are clinically relevant.

There are also inaccuracies in the description of the study design methodology. The term 'incidence' of suspicious disc change is referred to in the title and in the text, whereas in fact the study investigates prevalence. This distinction in terminology is important and should not be confused. Furthermore, the term case-control study should be reserved for studies where subjects are selected on the basis of outcome and not exposure (as in this case, ie, presence/absence of OSA).

In summary, this paper demonstrates that collaborations between statisticians and other researchers are vital in order to avoid preventable mistakes that can render the results of otherwise well-conducted studies uninterpretable.

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Sir,
Reply to Shah *et al*

We thank Drs Shah *et al* for their interest in our paper on obstructive sleep apnoea (OSA) and optic neuropathy.¹ Here, we attempt to elaborate on a few points mentioned by their letter to the editor.

In our study, we analysed each eye independently because normal tension glaucoma and various forms of optic neuropathies like nonarteritic ischaemic optic neuropathy, which may be related to OSA, often present unilaterally and asymmetrically, especially in the early stage.^{2–4} Anyway, as suggested by Dr Shah *et al*, we re-analysed the data by collapsing the data into individuals and analysed using Wilcoxon's test (a nonparametric test). The differences of visual field indexes between OSA and control arms were still found to be statistically significant ($P=0.021$ for the difference between mean deviation in the two arms, and $P=0.001$ for the difference between pattern standard deviation in the two arms, respectively).

Different statistical tools help researchers to analyse data in different aspects and present them at different angles. Shah *et al* suggested that we should present the difference of visual function indices in means with confidence interval (CI). However, in our series, only a small proportion of OSA patients developed optic neuropathies. Comparing their mean may not yield conclusive information. Therefore, as an alternative, we chose to present the visual field index in a graphical method showing the distribution of data, which gave a clear overall picture demonstrating that a small peak of suboptimal visual field indices was found in the OSA group. Concerning the values of CI, we did present the data in the footnotes of all tables and figures.¹

Finally, we would like to thank Dr Shah *et al* for mentioning the importance of study design methodology and terminology. We share their view that these are important aspects in any research.

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Sir,
**Reversible structural disruption of the outer retina
in acute welding maculopathy**

Exposure to welding arcs might result in maculopathy if appropriate protective eyewear is not used. The damage is thought to stem from photochemical injury to the photoreceptors.^{1,2} We report the correlation of clinical and optical coherence tomography (OCT) findings in a case of acute welding maculopathy.

Case report

A 19-year-old male presented with blurred vision in his right eye after staring at a welder earlier that day. The patient was aphakic in both eyes after lensectomy at the age of 10 years for subluxated microspherophakic lenses. Corrected visual acuity 3 months before presenting to our clinic was 6/7.5 in the right eye. Visual acuity in the left eye was light perception as a result of recurrent rhegmatogenous retinal detachment after trauma and