answered, even in the presence of better tests that we now possess. It will still reduce yield.

However, it may also introduce another problem. The PHP research group excluded one-third of enrolled cases. There will always be a group of patients ineligible for PHP. The best way to help this population would surely be direct referral to a hospital retinal specialist for flourescein angiography. Again, these patients would gain little from a further prehospital consultation. Indeed, this would delay potentially eligible patients accessing treatment at the earliest stages of the disease. Some lesions may even become too advanced for treatment by the time of hospital attendance.

The cost of case-finding, mentioned by Wilson and Jungner, is more than merely the cost of the test itself, however. It includes all the costs incurred along the patient journey. Making this journey longer will of necessity make it more expensive per case detected.

Dr Verma's letter does raise one final interesting point, however, namely the issue of compromising interests in journal publication. He suggests, no doubt amicably of course, that we have such a compromising interest, namely a 'resistance to change' and even an imputed motive; a less than professionally courteous relationship with optometry. We are all familiar, of course, with the disavowal of compromising interests when they threaten the intellectual neutrality of original work. Are the same rigours inapplicable to letters and editorials? Of course not. Indeed, such is the human tendency to accept a*rgumentum ad verecundiam* (appeal to authority) that they should be at least equally transparent. I accept that there are problems with disclosure,[6] but of course the risks of nondisclosure are significantly greater. For example, I am sure that the fact that Dr Verma is the founding director of an independent profitable concern that markets detailed screening tests for macular degeneration will not compromise his neutrality in this debate, but it would seem that readers should be aware of the facts and allowed to draw their own conclusions.

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Sir,

The prevalence of low vision and blindness in an inner city in Canada

The article by Maberley *et al*[1] adds to our understanding of ophthalmic and concomitant general medical conditions experienced by inner city residents in Vancouver's downtown eastside. Although this report is useful in its own rights, it is also important to highlight some of the methodological limitations of this study that will have a bearing on the interpretation of the findings. Firstly, the way study participants were recruited raises questions as to how representative this group is to the general community. It is problematic when community prevalence of a disease/condition has to be generated from attendees of health care, as these are unlikely to represent fairly the general population from which they come from. So from the study, it can be said that the prevalence estimates obtained would represent better the population of patients who attend this health facility for eye care. Even when we do that, the participants in this study were not randomly recruited, as they were consecutive patients on special days when recruitment occurred. How representative these special days and times (2 h) are raise further questions on the representativeness of the 'sample'.

The authors also report that ocular examinations were conducted by a single ophthalmologist and this can be a source of systematic error as compared to when more than one person makes a diagnosis on the same patient. The comparison of the prevalence obtained in this study to an earlier study aimed to study general community prevalence[2] needs to be made with the differences in the study designs in mind. In the 2005 study, all patients attending a health facility over a 5-year period were

eligible. Diagnoses were verified through use of more than a single ophthalmologist. Owing to the large sample size ($N = 962$), these results would be more representative of the study population than in the current (inner city) study.

Finally, the authors need to be commended for presenting not just 'point prevalence values' but also confidence intervals (CI) for this parameter. This obviously helps the reader to see that virtually all of the values have wide CI raising questions on the precision of these estimates. For example, nonstandardized prevalence of visual disability was 500 per 10 000 (95% CI, 242–900 per 10 000), and nonstandardized prevalence of low vision and blindness 400 per 100 000 (95% CI, 174–770).

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Sir,

Reply to Dr Muula

We would like to thank Dr Muula for his comments regarding our recent publication.[1]

The purpose of this paper was not to precisely define the prevalence of disability in a population, but to identify a vision crisis that has not previously been recognized and to put a best estimate on the prevalence of this problem in a very marginalized community.

The first issue Dr Muula raises concerns how representative our study sample was of the general inner-city population in Vancouver's downtown eastside (VDES). We agree that our data may underrepresent individuals who do not attend medical care and have noted this in the second-last paragraph of the paper. However, we must emphasize that our subjects were not attending the Vancouver Native Health Society (VNHS) for eye examinations as Dr Muula suggests. Instead, these individuals were there for general, nonophthalmic care (paragraph 2 of the Methods section). As a result, we believe, there is no selection bias towards eye disease in our sample. Moreover, the dates and times of each intake clinic were varied over the course of the 2-year study period, and were not conducted at the same time of day or on the same day of the week. As such, we believe that we achieved as representative a sample of clinic attendees as possible. We also know that demographic data from the VNHS clinic has been found to correspond quite closely to the larger VDES community.

Dr Muula also has concerns regarding the use of a single ophthalmologist for the eye examinations in our study. We do not believe this is a valid criticism. First, our study did not require specific patient diagnoses, only a simple categorization of the aetiology of vision loss—a routine practice for ophthalmologists. Second, although it would have been interesting to have more than one physician confirm our ocular classification, such an approach was not practical from a physician availability standpoint and would not necessarily have improved our categorizations. Third, contrary to Dr Muula's comments, all of the ophthalmic diagnoses in our prior study of a medium-sized Canadian city (Prince George) were also made by a single ophthalmologist.[2] This latter study was a chart review and, as such, the patients' ophthalmologists were occasionally consulted if there was diagnostic uncertainty for the physician performing the data abstraction.

We agree that there are methodological differences between our VDES and Prince George studies. These differences were unavoidable given the dissimilarities of the medical and social environments in these communities. Our intergroup comparisons are not intended to be unqualified; however, the prevalence figures for our VDES population (even taking into