TO THE EDITOR: We congratulate Keel et al1 for their interesting paper on estimating the prevalence of diabetic retinopathy (DR) in non-Indigenous Australians and Indigenous Australians with self-reported diabetes. Collectively, these findings will be greatly useful in the future planning of healthcare resource allocation.

We are intrigued by a few interesting areas stated in this study. First, the authors revealed that the odds ratio (OR) for age was 0.97 (95% confidence interval [CI], 0.94–0.99) for any DR among non-Indigenous Australian participants. There is still a debate regarding age as a risk factor in this aspect. Similarly, the Singapore Malay Eye Study reported that older age (OR, 0.73; 95% CI, 0.57–0.93) was protective of any DR.2 However, another cross-sectional survey in Gegharkunik, Armenia, found that older age (OR, 1.05; 95% CI, 1.02–1.08) was an independent risk factor for DR.3 Could this be owing to differences in population characteristics and even study methodology? What could be the plausible reasons to explain this observation? We would greatly appreciate if the authors could further comment on this.

Second, in this article, higher educational attainment was found to be association with vision-threatening DR in non-Indigenous Australian participants. We are wondering whether this finding could be potentially confounded by income status that was not accounted for in the model presented by the authors. If income information is available in this study, perhaps the authors can further evaluate this aspect?

In Table 4, among indigenous participants, it was observed that longer duration of diabetes was a significant risk factor for any DR (OR, 1.69 per year). In addition, the interaction term of age × duration of diabetes was also significantly associated with any DR but with the effect estimate pointing at the protective direction (OR, 0.99; P = 0.005). In view of the conflicting directions, how should this be interpreted? We would greatly appreciate the authors’ clarification on this.

Last, as acknowledged by the authors in the Discussion section, other relevant confounders and indicator of diabetic control (i.e., hemoglobin A1c, body mass index) were not accounted for in this study. Therefore, we humbly opine that the current risk factor-related findings should be evaluated with caution.

Once again, we congratulate the authors for the wonderful work and thank you for the kind attention.

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