Author's response to reviews

Title: Prevalence of Diabetic Retinopathy in Tehran Province: A Population-Based Study

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Author's response to reviews: see over
Cover letter

Dear all

At first I do appreciate your concern in evaluation and quality promotion of our manuscript and sending the reviewer’s report. We changed the manuscript following the reviewer’s suggestion based on our competence and data availability. Please find the revised parts highlighted in the article. We did our best to change our manuscript according to BMC Ophthalmology Journal instructions, too.

I should also mention that all tables have been changed. We also changed the outcome variable from NoDR, PDR and NPDR to presence of any retinopathy. These changes were based on one of the reviewer’s right suggestion (Dr. Jill E Keeffe) who was not in agree with the previous classification. Besides; the Ordinal regression analysis was changed to the ordinary Logistic regression in table 3. Tables 1 and 2 were combined together as table 2 which presented in two forms, 2-A & 2-B. The form 2-A shows the findings in more detail which makes it crowded but in form 2-B, the results are summarized. We prefer table 2-B, but the respected referees can choose the one they rather.

First reviewer (Dr Mohsen Janghorbani)

1. Hypertension had been recorded qualitatively (absent/present) in our study, and the blood pressure > 140/90 mmHg was considered as the presence of HTN which was as same as some previous studies (references #25-26 and 45 in the manuscript), so we could not perform a new analysis based on HTN=130/85.

2. As mentioned in the text, Patients in the first study (SURFNET) were selected from five age groups by using random cluster sampling and proportional to size of different urban and rural areas in Tehran province. The males and females number were the same in each subgroups. Fasting Blood Sugar was taken in four age groups. All persons diagnosed as diabetic were registered and their identification data were sent to our Ophthalmic Research Center and these patients were all announced and undertaken eye exam. Our information of the first phase
(SURFNET) is limited and we only access to the individual data only about the second phase of the study which was performed in our center.

3. Tables presented for men and women separately except table 3, we could not report the logistic regression analysis results separately for men and women, because of inadequate sample size in the categories and improper estimates and standard errors.

4. The percentages were revised according to the reviewer's opinion.

5. Table 3 was changed as dear referee’s comment.

6. Complete list of covariate entered in logistic regression was stated in statistical analysis section.

7. In table 1 & 2 P-values for comparing men and women patients were added.

8. The age and sex specific prevalence was presented in table 1.

9. The mentioned references were added.

10. Considering the Diabetes Mellitus prevalence stated in National survey 2005, Tehran province had the highest DM prevalence in Iran (details are presented in World Health Organization website: http://www.who.int/chp/steps/iran/en/index.html). Besides, in a study (reference #11 in manuscript) which was performed in Tehran Province, even a higher prevalence was reported. In addition, as discussed in ref#12, oral glucose tolerance test (2–h plasma glucose $\geq 11.1$mmol/l (200mg/dl)) was not performed in the original survey, so the prevalence of DM is probably not only less but also more than 9.5% in Tehran population.

11. The language of manuscript was re-checked, however, regarding reviewer's opinion if it is acceptable for being published; we are ready to pay any charge for English edition.
Second reviewer (Dr Jill E Keeffe)

Major compulsory revisions:

We checked the temporal agreement regarding the comment of respected referee, and the kappa coefficient is reported in the manuscript. Besides, we have discussed the limitation of our method for grading of DR in the text (last Paragraph of discussion). In addition, word "grading" changed, because if we want to use "clinical grading" as its complete and standard definition we should use all types of NPDR (mild, moderate, severe) in table 2 which may make it too crowded.

Minor Essential Revisions

In patients with severe degree of NPDR and patients with PDR and CSME, additional therapy including laser and surgical interventions were needed. We called this group as patients with Vision threatening retinopathy in accordance with Eye Diseases Prevalence Research Group definition (Ref#19 in the manuscript).

In table 3, all of the confounders were controlled by using the multiple logistic regression analysis and the effect of each variable on presence of any DR (as the response variable) was reported after adjusting the other variables.

Detailed data related to visual acuity was added to 4th paragraph of results section.

In the discussion section, according to the respected referee’s comment, we should emphasis more on DR grading method. The convenient changes was done done in this revision (first and last paragraphs of discussion section) and the paragraph that stating the low value of imaging in macular edema diagnosis was deleted.

Discretionary Revisions

References in introduction section were updated according to the referee’s comment
Third referee (Dr Rajiv Khandekar):

The age and sex standard prevalence of main outcomes are shown in table (1). It should be mentioned that the cluster size in the national study (SURFNCD), was too low (20 cases in each cluster) and considering the diabetes prevalence, we expected only one or less diabetic case in each cluster.

Most of the similar studies (references#23…29) that reported the DR prevalence in different countries were performed by using the cross sectional method which is cost-effective and acceptable in population base studies.

We do appreciate your precise concern in correcting our manuscript. All the grammatical corrections were done. Since highlighting all of the grammatical corrections made the manuscript too crowded, we ignored some of them.

We explained about type I DM and Type II DM in background section.

Methods

Ethical clearance and consent of participants was stated in the first paragraph.

Similar to other studies in this field (Ref# 23-29), patients were screened in the present study by trained health professional staffs or by a physician

Definition:

Hypertension had been recorded qualitatively (absent/present) in our study, and the blood pressure > 140/90 mmHg was considered as the presence of HTN which was as same as some previous studies (references #25-26 and 45 in the manuscript), so we could not perform a new analysis based on HTN=130/85.

Result:

The numbers less than 10 were written in word.

Median used for age presentation was deleted.
Standard rates were shown in table 1.

The questions related to regularity of visits were close ended and convenient explanation was added to the method and discussion sections.

The rates were corrected in the first paragraph of discussion.

According to the referrer’s comment, the second paragraph of discussion was deleted.

The discussion related to maculopathy and visual impairment was added (5th Paragraph of discussion).

Since of these 639 patients only 5 were deleted due to hazy media, this issue has a very low impact on total prevalence, so we did not include this as a limitation.

The requested extrapolating was performed and reported in the last paragraph of discussion section.

The grammatical correction in the acknowledgment was performed.

Since the WHO reference mentioned by the referee and the Buraim Study was not accessible for us, we couldn't add it. However, we previously reported two other references of the WHO recent reports in the text (Ref#37 20) and reference# 27 (Oman Study) was added regarding to the respected referee's opinion.

Finally, I do appreciate all the referee's concerns in precise evaluation of our study and I hope the answers would comply with their opinions.

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