Author’s response to reviews

Title: Prevalence and Factors associated with Diabetes Mellitus and Impaired Fasting Glucose Level among Members of Federal Police Commission Residing in Addis Ababa, Ethiopia

Authors:

Tariku Tesfaye (tarf9929@gmail.com)
Bilal Shikur (lebiluka@yahoo.com)
Tariku Shimels (tarphar2008@gmail.com)
Naod Firdu (naodfirdu@gmail.com)

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Response for Reviewers’ comments

Reviewer #1;

Comment 1. Based on the comment given for not merging the factors associated with DM and IFG, we have placed them separately.

Comment 2. Because it was commented to change the term ‘risk factor’ by ‘factors associated with”, we have done it accordingly throughout the manuscript.

Comment 3. Why precession level [acceptable error] is 0.02?

It is evident that, in order to obtain a narrower confidence interval of a given parameter estimate [ex. Proportion P], a smaller precision (d) is warranted. For example in our case, the [expected] population prevalence of diabetes mellitus from the nearby town was 5%. If we were to calculate the 95 % CI based on this figure and the stated precision of 0.02, we get a 3-7 % [5%±2%] of the prevalence 95% times out of 100. If we had increased the d value, however, the confidence interval would have been widened. For example if we use d=0.05, the 95% CI could be 0-10% (5%±5%) which is a bit meaningless. Due to the above reason and the fact that studies recommend to use a nearly half P if P is less than 10%, we preferred to use 0.02. This will eventually make an appropriate [larger] sample size and avoid a 95% CI of an irrelevant negative lower bound values as well as larger than 1 upper boundary.

Comment 4. As the last comment of reviewer 1 was to include interquartile range for ‘age’, we have included it in the result part as IQR of 23-34 years range to imply as the age group for the middle 50% of the subjects. [the median age is 27 years which also is the 50th percentile].
Reviewer #2;

Comment 1. Definition of variables; we have included a list of variables with their respective definition under the methods section.

Comment 2. Under the results section, the reviewer had stated this; “In 1st paragraph, author wrote “sixty (6%) subjects were not available due to work related problems and 7 (0.7%) were on annual leave during the study period”. Author should correct the typing error.” However, we left it as it was since we were not clear with the correction needed in this part. The first figure (60) is written in words as writing a new sentence and the latter (7) was written numerically. Percentages are also checked \[\frac{60}{1003} \times 100\% = 6\% \text{ and } \frac{7}{1003} \times 100\% = 0.7\%\].

Comment 3. Why is the wider gap between the crude and adjusted odds ratios?

As can be viewed from all variables (except ‘family history’ for DM) tested for and showed a significant association with either DM or IFG, the variation between the crude and adjusted OR is unidirectional i.e from a higher bivariate [COR] to a lower multivariate [AOR]. This implies that the observed association between the given explanatory variable (any of rank, family history, hypertension, WHR, age or BMI) and the outcome measure [either presence of DM or IFG] was positively confounded when considered in the bivariate analysis which resulted for the higher crude odds ratio (COR). That is, the prevalence of the subjects with the confounder [any or all of the variables other than the one being tested], is much higher among the subjects exposed to the given variable [the one being tested] than the subjects not exposed to the variable. This higher COR can, then, be modified to the true association of lower OR levels [though in the same direction] just after adjustment.

The reverse is true for the exceptional direction of confounding in the ‘family history vs. DM’ association in table 3[5.6 vs. 6.9]. In here, the crude odds ratio is slightly lower than the adjusted OR. Presence of a confounder that masks the true association might have resulted for this effect. If the proportion of subjects with a certain confounder [ex. Hypertension] was lower among the subjects with DM than subjects without DM, the association might be weaker and seemingly lower as compared to the one done after adjustment.

Comment 4. Under the discussion part, the reviewer has commented to correct the stated association between IGH and BMI. Because there was no significant association observed between DM and BMI, we admit the correction and have presented it as only IFG vs. BMI.

Comment 5. Finally, the reviewer had pointed out that there needed much modification to references. Accordingly, we have thoroughly looked in to the references part and made a lot of corrections. Broadly, the following types of modifications were made.
Extension of authors’ name; in some of the citations, there was a situation where names of only few authors’ [<6] mentioned and followed by et al. that was corrected to only >6 authors after consulting original articles.

Order of family and first names; in cases where the [abbreviated] first name preceded the family name, appropriate corrections were made.

Italicized journal names were avoided as per the guideline for authors.

Formats were followed as recommended by the BMC endocrine disorders throughout the references as much as possible.

Corrections are also made in cases where wrong spelling of author names had been included.

Overall, there have been a great deal of important comments and critics raised by the reviewers and the authors have considered the concerns with caution. We hope, you will be satisfied by the responses we have given, and consider our original work for publication in your journal. We are also very pleased to hear any issue concerning our manuscript.

Sincerely,

The authors,