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## Letter to the Editor

### Comment on: Prevalence of Diabetes in people aged >30 years: The results of screening program of Yazd province, Iran in 2012

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#### Dear Editor-in-Chief

We read with great interest the article published in JRHS journal by Lotfi et al.<sup>1</sup> They discussed on the prevalence of diabetes in people aged  $\geq 30$  years using results of screening program in Yazd Province. Authors found that prevalence of diabetes in women (25.3%) was more than men (9.2%). It seems that high prevalence of type 2 diabetes in women due to the large portion (more than 90 percent) of women who have participated in the study were 50 years and above. In other words, the mean age of women was higher than men (Table 2<sup>1</sup>; diabetic women = 1235, total number of diabetic patients  $\geq 50$  years = 1206). If this is confirmed by the authors, the high prevalence of diabetes among women in present study is due to selection bias and not volunteer bias that has cited in the discussion. In fact, it was better that sampling was done better, or analysis should be standardized based on province or national population. Authors only in this way could claim that obtained measurements in the study are related to people aged  $\geq 30$  yr. Thus, the prevalence of diabetes in the general population and in subgroups (gender) will differ from the reported facts and figures.

Authors have mentioned that total of 10 variables that had significant *P*-value in univariate analysis were entered into the logistic regression models. These 10 variables are not clear and with regard to the results, residence place was not significant in the univariate model ( $P=0.36$ ). However this variable was entered into the multiple models and also was significant. So there is a contradiction. Authors reported that the variables were significant in the multiple models and one of these variables was gestational diabetes. As authors know, this factor is limited to subgroups of women, but the reported results and analysis was performed for the total population including men and women. Is it possible to enter variable in the final model while it is limited to one subgroup?

In the methodology section they reported that 3966 people at risk were not screened (lost). The inference is that 2790 people were female. In the second paragraph of results, the authors have analyzed data on 7676 women. This means that also missed women were included in the analysis. Is this

inference correct? If correct, is it possible to evaluate the association between diabetes and risk factors related to women?

Furthermore authors reported that the final analysis was performed on a total of 11,027 people and total of 4886 women were studied (Table 1). But findings of 7676 screened women have been reported in the second paragraph of results.

It is important that the term "prevalence" in place of "prevalence rate" would be replaced in the text. As the authors are well aware of the fact that prevalence is a proportion rather than rate.<sup>2</sup>

Using the term of "case – control" in the study that is actually comparative cross sectional is not correct. It is important to know groups as defined in Table 2, as an alternative to case and control groups, diabetic and non-diabetic groups should have been written.<sup>3</sup>

Finally, in Table 2, it was desirable that findings related to crude odds ratios and adjusted odds ratios were reported in the presence of all variables (both significant and non-significant variables). So audiences could have a proper perception and interpretation of the presented results.<sup>4</sup>

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## Reply

I would like to thank authors for showing interest in our article. With regard to the stated comments, some explanations are provided as follows.

In respect to being high prevalence of diabetes amongst women compared to men; our study was a cross-sectional type that was mainly based on the data already collected. Cross-sectional studies are often prone to various bias and the comment about the high prevalence of diabetes in women due to selection bias, may not be ignored but since that women with a previous history of diabetes compared to known diabetic men had higher welcoming to a diabetes screening program (24.1 versus 7.4), the higher prevalence of diabetes in women can be attributed to emphasizing their health, concluded due to volunteer bias.

As for residence place variable, entered into the logistic regression model. To find the predictive and effective factors on diabetes, the variables that have significant *P*-values and also those that have no significant *P*-values but were clinically important such as residence place, they also were entered into the regression model.

Regarding gestational diabetes variable in the multiple model; the given comment can be correct, but we performed a separate analysis for the men and women. However, it could have been mentioned at the footnote of the regression table.

In relation to the comment said in the second paragraph of results, findings of 7676 screened women were related to the descriptive analysis, but the final analysis was performed on a total 4886 women. (Data on 2790 missed women were not included in the analysis).

The term "prevalence" in place of "prevalence rate"; the suggested comment can be used, so there is no conflict and term of prevalence has to be replaced instead of prevalence rate.

In the case of using the term of "diabetic and non-diabetic groups" instead of "case – control", we agree to this comment.

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