

Comments on Prevalence of Non-alcoholic Fatty Liver Disease and Its Related Factors in Iran

DEAR EDITOR

We read with great interest a recently published meta-analysis in the *IJOTM*. Moghaddasifar, *et al.*, estimated the prevalence of non-alcoholic fatty liver disease (NAFLD) and related factors in Iranian population as 33.95% [1]. The prevalence of NAFLD ranges from 2.04% to 82.9% in different studies.

They correctly identified different study populations as the main source of this heterogeneity and tried to correct it by using meta-regression analysis and coding the different populations. Our study, one of the included studies in the meta-analysis, seems not suitable to pooling with other included studies [2]. We evaluated 114 morbidly obese patients, candidates for sleeve surgery—a type of bariatric surgery. Patients with good physical conditions are usually selected for this specific procedure to decrease the rate of complications. Therefore, our study is not a good sample for adults, especially those with morbid obesity, and may underestimate the prevalence of NAFLD in this population.

Another issue is the main calculations that seem to have an error due to ignoring the size of populations in different provinces. For better estimation of the prevalence of NAFLD in Iran, authors should have considered the size of provinces and age-gender distribution in each region [3, 4].

We also think the authors might have “double-publication” bias. The 952 children and adolescents out of 1107 subjects of Tazhibi, *et al.*, study seems to be the same subjects of Adibi, *et al.*, study [5, 6]. One study compared the prevalence of NAFLD in obese and non-obese children [5], while another evaluated the effect of life-style on NAFLD in these children [6]—both studies reported a prevalence of

16.9%.

For lack of enough data homogeneity about the prevalence of NAFLD in Iran, this topic seems not suitable for a meta-analysis study. Good quality original studies evaluating prevalence of NAFLD and correlated factors in Iran are strongly needed. Large cohort studies may resolve this important issue.

CONFLICTS OF INTEREST: None declared.

FINANCIAL SUPPORT: None.

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Authors' Reply

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We deeply appreciate the comments on our paper [1]. Regarding the paper of Karimi-Sari, *et al.* [2], which was included into our systematic review, the study population, obese patients undergoing sleeve gastrectomy, has been considered the adult population group. As the matter of fact, it was not ignored by the authors; they emphasized that the structure of the primary studies suffered from a great heterogeneity and mentioned this fact in detail within the text and Tables. Moreover, results of meta-regression models revealed the population as one of the main sources of the heterogeneity. In addition, the study population was one of the issues that was considered for subgroup analysis, though due to the limited number of primary studies in each subpopulation, only systematic review was carried out for such subgroup.

We agree with other comments on entering

two studies with different titles conducted in the same population [3, 4]. Although we considered the site the study was conducted, different sample sizes reported for each study, *i.e.*, the sample size of the study of Tazhibi, *et al.* [4], was reported to be 1107, while the sample size of Adibi, *et al.* [3], study was 952. That was why the researchers extracting the information made such a mistake. In total, the available evidence of primary studies is very low and such limitation is expected to be settled by future studies.

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