Reviewer's report

**Title:** Effects of Greek Orthodox Christian Church fasting on serum lipids and obesity.

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**Reviewer:** Shraga Shany

**Level of interest:** A paper of considerable general medical or scientific interest

**Advice on publication:** Other (see below)

Comments:

1. The Greek Orthodox Christian fasting periods represent changes in diet rather than a real fast. Besides the general declaration that meat was replaced by fruits, vegetables and legumes, or that fish were replaced by other sea fruits, there are no quantitative details concerning these changes. Moreover, the possible changes in total calories intake is neither described and nor discussed. The authors have added the required information concerning diet changes during the fasting periods on the new Introduction chapter. Although this description is a qualitative rather than a quantitative one, it may be accepted. Similarly, data concerning changes in calories consumption in fasters and control groups, were added on page 13 of the discussion.

2. There is no description concerning physical activity during the fasting periods. Reduction in physical activity during these periods might explain the slight, although not significant, reduction in HDL-C levels. The authors claim on page 14 of the discussion that there were no changes in physical activity during the fasting periods neither in the fasters nor in the controls.

3. Serum triglycerides levels are not presented, although these values were taken into account in the LDL-C calculations. The use of the Friedewald equation for the LDL-C calculation, as was carried out in this study, is suitable only in the case of normal or in limited increase in serum triglycerides levels. Therefore, it is important to present these data. Triglycerides levels were added to Tables 2, 4 and 5. at least from the mean values, it seems that the Triglycerides levels are in the normal range. The use of the Friedewald equation, in this case, seems to be legitimate.

4. There is a gender difference between the groups. The control group is composed of 36 women while the fasting group contains only 29. Although no age distribution of the women is noted, it may be concluded from the average age of each group that the women are in their reproductive age. In this age their total cholesterol as well as their LDL-C serum levels tend to be lower than in men. The unequal number of women in the tested groups may alter the results. The authors claim that gender differences in this study were found to be not significant.
5. A significant difference in the number of smokers exist in the comparison between the tested groups. The authors confirm the significant statistical difference in smokers number between the tested groups (much higher in controls).

6. I am not certain that calculating the means of the lipoproteins data before and following the three annual fasting periods is a good idea. The fasts are different in length, season and possibly in diet. I believe that more accurate results could be obtained by comparing separately the pre and post fasting data. The author's response to this issue is expressed by the new data on Table 2, to my satisfaction.

7. From the footnote of Table 2 it is understood that only 71 subjects out of the 120 participants had all 6 measurements. If this is true, it should be emphasized that the regression analysis was carried out only on this number. I also believe that BMI should be gender analyzed. I have found the description in the new Results chapter mentioning the fact that all comparisons were done only on subjects that have all 6 measurements, however I failed to find this comment below Table 2. As far as the gender BMI is concern, I understand from the author's response, that this point was analyzed and no significant regression could be detected.

8. Both fasters and non-fasters are in the overweight range. BMI of 27 for fasters and 27.6 for non-fasters. This fact limits of course the significance of the results for the general population. The author's answer to this issue should be included in the discussion.

9. While no significant difference in T.Cho/HDL-C ratios were determined in the comparison between the fasting and non-fasting groups, a significant decrease in LDL-C/HDL-C ratios in the fasting group was reported (Table 3). This discrepancy may be explained by the fact that LDL-C values in this study, are calculated rather than directly measured. As a result, all laboratory mistakes are concentrated on this value. Moreover, a bias may be caused by including data with high levels of triglycerides in the calculation of LDL-C levels (See comment 3). This may put a question mark on the validity of the LDL-C results in this study. The author's response is well accepted.

10. There are differences between tables 3 and 4 in T.Cho, BMI, T_CHO/HDL-C and LDL-C/HDL-C values for the fasting group on the end of the fast. I assume that these differences are a result of including different participants numbers in each table. This fact is not mentioned in the article and the reason for it is not explained. The author's explanation should be added to the text.

11. In the discussion chapter it is mentioned that T_CHO/HDL-C and LDL-C/HDL-C ratios in Barnard et al study remained unchanged during the diet changes. The authors of the present study claim that these results contradict theirs. The results on Table 4 are in full agreement with Barnard et al data and the same is true for the T_CHO/HDL-C ratio presented on Table 3. The authors have changed this point in the text, in the spirit of the above mentioned comment.

12. Not all comparisons with the literature made in the discussion chapter are valid, since the authors of the present article failed to provide information concerning the "vegetarian" diet during the fasting periods. I agree with the authors that now, after the addition of the dietary restrictions during the fasting periods, to the introduction chapter, the comparisons made on the discussion are more valid than before. However, it should be mentioned that the comparison has only a partial value, due to the above mentioned limitations.

In general, it seems that the authors have answered most of the questions I raised. Hence, I recommend to publish this article in BioMed.
Competing interests:

None declared.