

Correspondence

Evaluation of the effect of Islamic fasting on lung volumes and capacities in healthy persons

To the Editor

I have read with interest the article by Moosavi et al.¹ A few clarifications are needed: in the introduction the authors have mentioned that “people fasting generally experience significant improvement in their respiratory function”. Apart from their own study, can they support this statement from the literature? The second and third test dates are ambiguous. For example, a test could have been conducted on the 14th of Ramadan (in the first half of Ramadan), and then in the same person again on the 16th of Ramadan (in the second half of Ramadan). If so, what would be the scientific merit of this study design? Therefore, it would have been advisable to either present the exact dates when these tests were actually conducted or conduct the tests in a narrower range of dates. Notwithstanding the outcome of the study, which of the predicted values were used, and how many tests were performed on each subject? The level of probability taken as significance was 5% ($p < 0.05$). However, whenever multiple variables are compared at a time, it is likely by chance alone that some of them will be statistically significant. Therefore, stricter criteria should be applied than the usual 5% probability, as seen in previous work.² For example, if 4 variables are compared, for any one variable to reach a statistical significance, variables must have a P -value equal to $0.05/(\text{number of variables})=0.0125$. Should this approach been used, perhaps the outcome, and conclusion would have differed. Apparently, there are discrepancies between the proportion of males, and the mean age of all subjects in the abstract, and the main results section. Which are correct? In the discussion, Moosavi et al stated a previous study was conducted on women,³ yet it was carried out on men only. The results presented showed a significant decrease in mean body weight after Ramadan, however, in the discussion the authors say “In our study, the mean weight of subjects was significantly higher after Ramadan”. I would like to thank the authors for their interesting work, especially in presenting the baseline lung function data, which is relevant for research scientists, and clinicians, whether primary care or specialist.

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Reply from the Author

I would like to express my special thanks to Dr. Mirza Subhan for his sincere attention to our paper. In response to the first question, I should mention that the exact sentence in our last revision (before portable document format [PDF] approval) was “people fasting generally feel improvement in their respiratory function” and not “experience significant improvement”. The editor of the Saudi Medical Journal (SMJ) has changed it in PDF version, and unfortunately I did not pay enough attention to this small change, since changes are not usually highlighted. Anyhow, it was according to our pilot study on quality of life of healthy persons in Ramadan [unpublished data]. Although, some studies have shown improvement in pulmonary function following calorie restriction in asthmatic patients^{4,5} and obese persons,⁶ others usually were unable to show significant changes of respiratory function as we discussed in our paper.¹ Moreover, I should add that according to our methods, “quantitative variables were described as mean (95% confidence interval [CI])”. So, all parentheses in the results show 95% CI. The editor of SMJ has added the word “range” inside each parenthesis of results by mistake, which unfortunately was overlooked by me during the final approval of the PDF version. The exact time of second spirometry was 7th-10th and third spirometry was 22nd-25th day of Ramadan. Our spirometer, (and any other spirometers) estimates the volumes, and capacities of each person’s lung according to their age, gender, weight, height, room temperature, and so forth, which we call expected value. We also take a pulmonary function test (PFT) by asking the person to blow into the spirometer which gives us another value. Then the spirometer comes up with a percentage by dividing this second value by the expected value. We used these percentages in our study. The predicted values in our study¹ is the spirometer’s feedback after receiving the data on the characteristics of each case, and the room temperature at the moment of PFT. We determined their height only once before the study, and specified the weight, and PFT in each visit for all cases. We have used one way ANOVA (and post hoc), instead of repeating multiple pairwise comparisons. In this case, you do not need the Bonferroni adjustment. Our ANOVA test was carried out 10 times, and mix model analysis was carried out 9 times, and both were used only according to pre-established main hypotheses of the study. We believe that these do not need the Bonferroni adjustment in this situation.⁷ It is better that the results of comparison of men and women, and also correlation of fasting days with other variables be adjusted by the Holm method (a similar but superior method to

Bonferroni),⁸ as these are sub group analysis, and are not our main goals in this study.⁷ Recent papers also believe that the Bonferroni method is inappropriate, as it will be highly conservative, and may miss real difference.⁹ However, by Holm adjustments, most of significant findings of Tables 2 & 3 remain significant. Since our results are exploratory, we can leave the adjustment, and the future confirmatory studies will elucidate the accuracy of the results.¹⁰ You are completely right. The “proportion of males, and the mean age of all subjects” mentioned in the body of the paper are correct, and not in the abstract. The abstract part is the result of all 145 cases at the beginning of entrance to study, and not 117 studied cases. However, as it is shown in our methods only 117 persons met the eligibility criteria, and completed all 4 visits. In the sentence “Subhan MM et al have carried out a similar study with lower sample size only on women”, the word “women” was printed by mistake instead of “men”. About your last comment, unfortunately, it is a writing error in the discussion section of the article. The correct sentence is “the mean weight of subjects was significantly lower in post-Ramadan period, and they lost weight.” However, at the end of the same paragraph we have correctly expressed: “It seems that losing body weight is not the major mechanism explaining volume changes.” The rest of the results, and discussions are mentioned correctly on the matter of weight loss. I want to emphasize that this error did not affect the discussion on the valuable finding in our study, which claims most of the desirable effects of fasting (except increase in MMEF, MEF_{50%} and FEV1/FVC%) were not only due to weight loss. For more explanation I should say that the last part of our results (“Weight loss had only a significant effect ... and FEV1 [$p=0.037$]”) was gained through a mix model analysis. None of the previous studies in this field have carried out this analysis, and we recommend it for future

studies with a similar confounding effect of quantitative variables, which are repeated during measurement.

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Editors note: The numbers in the original article were written as such and the confidence interval (CI) was not mentioned in the result section. The word range was inserted by mistake, which means as a range of numbers because CI was not written.