

-----Original Message-----

From: XXX YYY<xxx@yyy.zzz>

Sent: Mon, Aug 19, 2013 at 1:27 PM

To: Managing Editor

Subject: RE: Request for final decision for manuscript number 2013_BJMMR_4845

I have examined in detail the original manuscript by Damor *et al* entitled "**Effect of progressive resistance exercise training on hepatic fat in Asian Indians with Non-alcoholic Fatty Liver Disease**". I have also examined the comments from the three reviewers, and the revised manuscript. One of the reviewers still questions the lack of novelty of the data, and the authors have failed in my opinion to come up with sufficient grounds to warrant publication of the data in its present form.

Of far greater concern is the statistical analysis of the data in Table 1, which shows means and standard deviation data on a number biological of variables, before and after resistance exercise training. Paired t test was used to establish whether the means were statistically significantly different from each other. The sample size was 24 in each case. The authors indicated that they found statistically significant differences in the circumference of the waist, hip, mid-arm, mid-thigh. They also indicated statistically significant differences in skin fold thickness of the biceps, triceps, subscapular and siphilic. The authors also claimed that exercise training significantly reduced the deposition of fat in the liver. And this was what they based their entire manuscript upon, ie, the conclusion is that that exercise improves hepatic fat and truncal subcutaneous fat. In point of fact, hepatic fat was 22.2 +/- 3.9 % before exercise (mean +/- standard deviation, n=24 subjects); and this was reduced to 21.3 +/- 4.0% after exercise. The authors claim that the P value was 0.01, however it is very difficult to see how this could be the case. The mean hepatic fat percentage fell by less than one percent which is very little indeed, and the confidence intervals were 0.18 to 1.6%. How did the authors find this tiny reduction to be so statistically significant? Similar assertions that the circumference data and the skin fold thickness data is highly significant (P less than 0.001 in all cases) is also difficult to reconcile with the data presented in table 1. The mean waist circumference fell from 87.0 +/- 8.7cm pre-exercise, to 86.3 +/- 8.7cm post-exercise. That is a reduction of less than one centimeter, with a error margin of +/- 8.7cm. How can this possibly be highly significant (P less than 0.001) as claimed by the authors? Similar tiny reductions in mean hip, mid-arm and thigh have been reported as highly significant in Table 1 and in the abstract.

Figure 2 shows mean K-value before and after exercise, however the graph and numerical values presented do not appear to be different from each other. The error margins are large and overlapping between treatments, indicating no significant

difference ($k = 0.84 \pm 0.8$ before exercise compared to post-exercise 1.3 ± 0.63). How can this be significantly different? Therefore and in view of the reviewer's twice-stated comments concerning the lack of novelty of the presented findings, this data should be re-examined by a statistician to verify that the data is indeed statistically significant as claimed, before it can be considered acceptable for publication.

Editor's Details:

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