Reviewer’s report

Title: Effects of Lifestyle Modification on Metabolic Syndrome: A Meta-analysis

Version: 1 Date: 6 June 2012

Reviewer: M. Hassan Murad

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MAJOR COMPULSORY REVISIONS

1. Authors did not report the risk of bias in individual studies (methodological quality) or describe the quality of the evidence (strength of inference). This is a mandatory procedure that is always needed to interpret this body of evidence.

2. Authors report following the PRISMA statement but they clearly did not do that (no quality assessment, no measure of consistency (i.e., I2, only p value is reported), and importantly, no description of search strategy.

3. The review is clearly affected by publication bias (by definition, restricting the review to English language). Funnel plot assessment is absolutely useless here (small number of studies with high heterogeneity).

4. In two areas (abstract and results section), author state that Lifestyle Modification reduced HDL (the correct inference is it increased HDL)

5. Pooling risk difference is flawed in this situation (associated with significant heterogeneity due to baseline risk heterogeneity) and is rarely done. The appropriate measure for the effect size in this analysis should be a relative association measure (RR or OR).

6. Sensitivity analysis for several correlation coefficients is needed

7. Clusters RCTs were included but there was no clear description of how the design effect was adjusted for in analysis.

MINOR ESSENTIAL REVISIONS

1. Awkward writing: (for example, repeating “random effects model” 3 times in abstract). Numerous grammatical errors (for example, missing “of” in “proportions of patients”, page 6, line 7

2. Authors use 3 analytical models which is confusing to readers with limited statistical background and takes away from the message of this review. They should choose one model (a priori) and provide justification of their choice (at the least, take that out of the abstract).

3. Life style modification follows a dose related fashion so when they decided include only RCTs they could underestimated the effect size of LSM programs. This should be acknowledged in discussion

Discretionary Revisions
1. We would like to see implications for future research about LSMs in this review. For instance, evaluate LSM with and without weight loss, LSM diet vs anaerobic exercise vs aerobic exercise vs combination, etc.

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I have no competing interests. My colleague: Juan Pablo Domecq Garces, MD. also has no competing interests.