Reviewer's report

Title: A yoga intervention for diabetes risk reduction: A pilot randomized controlled trial

Version: 2 Date: 2 December 2013

Reviewer: Ramaprabhu Vempati

Reviewer's report:

Major Compulsory Revisions:

• Why authors excluded some subjects in the final analysis? Was this because of some outliers or did authors had already defined Intention-To-Treat (ITT) analysis before the study started? Under methods, this part was not clear, after the randomization and study completion, why some subjects were excluded from the final analysis based on screening FBG? Please provide data for these subjects in the manuscript to avoid potential bias.

• At some places in the manuscript, the authors referred insulin resistance as HOMA, which is actually a method of assessment. It would be highly recommended to use Insulin Resistance wording instead of HOMA. Though authors did not actually measure the “insulin sensitivity”, but referred under results section in Page #2. How authors did actually measured insulin sensitivity (e.g. quantitative insulin sensitivity check index), if measured please provide the data on this.

• The first visit evaluated FBG using a glucometer and finger stick capillary whole blood, but authors did not provide further details about this method as a standard technique (specificity and sensitivity at each cut-off level). Because some previous studies concluded that this finger stick method may not useful in determining the prevalence of diabetes mellitus. Also the authors have used Roche Diagnostic’s insulin kits, but did not provide any further details on intra-assay and inter-assay coefficients of variation for measuring serum insulin.

• What was the basis for selecting certain yoga postures in this study as a yoga intervention for diabetes risk reduction, please explain.

• Variety of physical exercises is known to improve the insulin resistance condition and increase B cell sensitivity, but in the current study the yoga group’s decrease in insulin resistance was larger than the control group’s decrease. Also in control group, psychological measures were significantly changed but not in yoga group, however quantitative measures such as physical and metabolic measures did not change significantly. Was this because of unmatched durations of both these interventions (75 min vs 30 min)? If so, please comment this point under the discussion.

Minor Essential Revisions:

• Authors are requested to present the results of primary and secondary
endpoints in graphs for better understanding the effect or trend, if any.

• It is highly advisable to publish results pre-post study rather than the difference between pre-post studies, so that we can better understand the actual difference and group variation. Also please provide more details or data separately on pre and post study OGTT glucose, Insulin levels and Insulin Resistance levels to better understand effect or trend.

• Since this study was on diabetes risk measures, which may get influenced by dietary pattern and lifestyle - smoking/alcohol consumption etc, but authors did not provide such subject details in this manuscript. However authors did mention in the manuscript that the yoga group attended a day-long (8-hour) group counseling session on healthy lifestyle changes (i.e., diet, physical activity, and smoking cessation). It would be appreciable if authors could provide some details about subject’s pre study diet and lifestyle habits, if collected.

Discretionary Revisions:

• This study was a good study and it looks like an extension to a previous study published in Indian J Physiol Pharmacol 2005; 49 (3) : 319–324 (http://www.ijpp.com/IJPP%20archives/2005_49_3/319-324.pdf), however there are some study design level lacunae, which could have been limited the external validity of the current study.

• Control intervention was selected as walking for 30 min, which is not comparable with 75 min of yoga (unknown about how much physical component of yoga practiced by yoga group)

• Inclusion criteria (wide age group range 30-70 yr) was not appropriate for a study involved with diabetes risk parameters, because these parameters would change differently for any intervention in elderly group subjects than younger age group (but authors did not provide the details under different age groups). Please provide more details for better understanding.

• There was no correlation reported between metabolic and psychological measures or did authors identify any differences (in metabolic measures) between the subjects who had highest and lowest psychological scores?

• Decreased weight and waist circumference is a better measure for individuals who are moderately or severely obese, but the data was analyzed amongst all subjects across the study.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Not suitable for publication unless extensively edited

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.
Declaration of competing interests:

I declare that I have no competing interests.