Reviewer's report

Title: Metabolic syndrome among a middle-aged population in the Red River Delta region of Vietnam

Version: 2
Date: 15 July 2014

Reviewer: Stavros Liatis

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Major issues

• Although the data on the prevalence of MS and its components are clearly reported, the part of the manuscript describing the relation of MS with several “risk factors” is somehow confusing. Since this is a cross-sectional study it is not supposed to capture risk factors and/or causal relationships but only associations. Therefore, terms as “risk factors”, “at risk of developing...” and “predictors”, should be avoided.

• The paper tends to be too long. I recommend to omit tables 3 and 4. Furthermore, I do not see why the components of the definition of MS are listed (in tables 3 and 4) as “predictors” of the syndrome. If the authors wish to explore whether certain socioeconomic or other parameters are associated with the presence of MS, they should perform a multivariable logistic regression analysis WITHOUT including the 5 variables that are used to define the syndrome. Such an analysis could be presented in place of tables 3 and 4. In addition, the use of ROC curve is strongly discouraged. It is not advisable to test the ability of identifying MS (a diagnosis based on the presence of 3/5 criteria) by using a different set of variables.

• It is not clear which of the definitions of MS has been applied in the paper. The authors refer both to the ATP III and IDF definitions but none is identical to the criteria they have used. It seems to me like they have used modified ATP III criteria, adjusted for waist circumference in Asian population, as suggested by the IDF definition. This issue should be clarified in the methods section.

Minor issues

• The patient selection method is described as “simple random selection method”. Please clarify.

• Please clarify why “mentally disordered subjects were excluded” and how a “mental disorder” was defined.

• Results obtained from OGTT are presented but the performance of OGTT is not described in the methods. I do not see why an OGTT is needed for the purpose of this paper (?).

• The method of measuring body fat is not reported.

• Was the structured questionnaire, used for data collection, validated in this population? If not, this should be stated as a limitation.
• How are alcohol drinkers defined?
• Was physical activity assessed? If not, this should be acknowledged as an important limitation, since PA is strongly related to MS.
• Is heart attack (described in background section) identical to myocardial infarction?
• According to the methods section, plasma glucose was measured by glucose oxidase method. Please report consistently on plasma glucose (and not blood glucose) in the whole paper and in the tables.
• The reference for prevention of MS [7] is a review. Please replace by the relevant clinical trial.

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

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

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
None to declare