Reviewer's report

Title: Leisure time physical activity in middle age predicted the metabolic syndrome and diabetes in old age: A 28-year follow-up of men in the Oslo Study.

Version: 1 Date: 10 January 2007

Reviewer: David Laaksonen

Reviewer's report:

General
The authors studied the association of leisure-time physical activity with the incidence of the metabolic syndrome and diabetes during a 28-year follow up in 6,410 men who were middle-aged at baseline. The main finding was that leisure-time physical activity was a strong determinant of the development of the metabolic syndrome and diabetes.

The findings are consistent with other prospective cohort studies. This study suggests that physical activity predicts the metabolic syndrome even after 28 years follow up. The large size and population-based sampling are strengths. There are several serious problems with the study, however.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

The follow-up is a composite of 5 studies. Most of the men were from a single study. The authors should perform subgroup analyses excluding the men with data added from other studies. This is especially important for men with data taking from fasting blood samples instead of the non-fasting samples taken at baseline and during follow-up for the rest of the men.

The definition of the metabolic syndrome is very problematic. The authors claim to use a definition based on the NCEP criteria, but the definition is modified so extensively that that can be questioned. It is reasonable to substitute BMI for waist circumference, but a waist circumference of 102 cm does not correspond to a BMI of 25 in any population. The 102 cm cut-off has its origins in papers by Michael Lean, in which a waist circumference of 102 cm corresponded to a BMI of 30. The authors raise the cut-off for elevated blood pressure. This may be reasonable, but is yet another change in the definition. HDL cholesterol was not measured. Thus there are only 4 features of the metabolic syndrome. Defining the metabolic syndrome as 2 of 4 features is a major departure from the NCEP criteria.

Use of non-fasting samples is problematic. The authors use statistical modelling to take into account the time since the last meal, but they don’t seem to have much confidence in the models, because they chose to raise the cut-off for plasma glucose from 6.0 to 7.0 mmol/l. Rather than define diabetes as an adjusted plasma glucose of 7.0 mmol/l or greater or diabetes medication, they define it as a non-fasting glucose greater than 13 mmol/l or diabetes medication. Impaired fasting glycemias should be included in the definition of the metabolic syndrome, and diabetes (using a cut-off of 7.0 mmol/l) should be excluded at baseline. When defining diabetes, they should use adjusted plasma levels of 7.0 mmol/l. Also men with self-reported diabetes should be considered to have diabetes, not just those stating that they use diabetes medications or insulin.

I also find it difficult to accept that the authors use different definitions of the metabolic syndrome at baseline and during the follow up. At the very least analyses should be repeated using the same definition to confirm if the results remain the same. It is also unclear as to whether diabetes was defined in the same way, because the authors state only the definition used for the 2000 follow up.

The quantification of physical activity is rather crude. The category of light physical activity probably includes large numbers of men who meet current physical activity recommendations. These men seem to be very active compared to some other Western countries.

Specific comments.

1. Methods. The authors confuse definitions and categorization of variables with the statistical methods. It is
confusing and should be rewritten. Age should be included in the models even if it did not significantly predict the outcomes. Age is nonetheless a powerful determinant of both the metabolic syndrome and diabetes, and a 10-year age span is not all that narrow.

2. From what I see in Table 2, the adjusted association of smoking status with the metabolic syndrome and diabetes was not significant. The results and discussion devoted to smoking can be shortened considerably.

3. The focus of the paper is leisure-time physical activity and the metabolic syndrome, but the discussion of the main findings related to this is rather superficial.

4. A table giving the baseline characteristics of the cohort should be provided.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

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

Declaration of competing interests:

I declare that I have no competing interests.