Reviewer’s report

**Title:** Association between Physical Activity and Cardiovascular Risk in Hong Kong Chinese Children and Adolescents

**Version:** 1  **Date:** 2 March 2010

**Reviewer:** Geir K. Resaland

**Reviewer’s report:**

The paper by Kong et al. identifies an interesting and relevant area of children’s health and the question raised is clear and of importance. However, there are some concerns. Still, I would very much like to see this paper published if the authors can clarify the issues I have listed below.

The validity of the questionnaire: Major Compulsory Revisions

Could the authors comment on the validity of the questionnaire, and please describe how they estimated physical activity intensity (low-moderate and high) based on the questionnaires. It is difficult for children to recall their physical activity levels, especially over such a long period (12 months). This fact could also be further stressed in the limitation part.

CVD risk factors and the clustered risk score: Major Compulsory Revisions

The authors have included both HDL-C and LDL-C in their summed risk score and by doing so making lipoproteins count twice. This is not common, and the authors should carefully consider including only one of the lipoproteins. LDL-c is not included in the traditional definitions of metabolic syndrome, and is not measured per se, but calculated. Therefore, a suggestion is to include only HDL-C or do as Andersen et al. (2006) (ref nr 18 in your paper), who use the total cholesterol/HDL ratio.

The authors include either systolic or diastolic blood pressure. I would like to see a rationale for doing so. Why not do like Andersen et al. (2006) (ref nr 18 in your paper), who include only systolic blood pressure?

Insulin resistance: In Andersen et al. (2006), insulin resistance was estimated according to the homoeostasis model assessment (HOMA) as the product of fasting glucose and insulin. You use fasting plasma glucose, a method that is not the best since glucose levels in children do not vary much (not even obese children do necessary have high glucose values). Therefore, your insulin resistance is of concern. Please clarify.

I am concerned about the adjustment for BMI in the regression analyses as an obesity measure (WC) is already in the risk score. It does not make sense to adjust for a measure that is already in the risk score. Please clarify.

Why did the authors not use Coles cut point (Cole et al. 2000) for assessing overweight and obesity? At least as an extra calculation, and thereby allowing international readers to make comparisons to their own work.
Subjects: Major Compulsory Revisions
An impressive number of subjects are included. However, the authors need to be clear on the subjects’ age. In the ABSTRACT the authors write: “children aged 12-20 years”. The authors do not mention age in the METHOD section in the paper. In Table 1 (page 16) the authors write: “children aged 6-20 years…” The authors need to clear up what the correct age span is and include this in the METHOD section.

# The authors write in the RESULT section: “Among 2102 eligible subjects, 1882 completed the questionnaires”. My question is: was there a significant difference in body mass, height and so on, between the included 1882 children and the other children?

Pubertal stage: Minor Essential Revisions
#Please comment on the use of subjective assessment of pubertal stage,- is this a valid method? I have some concern about the high proportion of children in prepubertal stage. One should assume that at the age of 12 (if 12 is the correct age) almost every child should at least have started puberty.

Language: Minor Essential Revisions
In the ABSTRACT and in the INTRODUCTION the authors write: “Children and adolescents are critical period of…”. Children is not a period, childhood is. Adolescents is also wrong; the correct word would be adolescence. This is just an example, and I recommend careful language editing from a professional text-editor.

Finally, a few recommendations that may improve the paper: Minor Essential Revisions

Introduction section:
# Introduction, paragraph 2: "...are critical period of habit formation.." please rephrase the sentence.

Methods section:
# Could the authors please comment on what they mean by "no major illness"
# The authors write: “Only adolescents of Chinese ethnicity with no major illness were recruited for assessment". My question is: as there were also children included in the study sample, were they also of Chinese ethnicity with no major illness. If so, the authors could re-write the sentence.
# The authors write: “Body weight (measured to the nearest 0.1 kg by Tanita physician digital scale, Tanita Corp., Tokyo, Japan)”. My question is: What type of Tanita weight?
# Regarding blood pressure:
(1) The authors write: “blood pressure (BP) were measured after at least 5 minutes of rest using a validated electronic device (Omron, Omron Healthcare Inc., Tokyo, Japan)”. My question is: What type of Omron device?
(2) How many measures of BP was performed on each subject? For instance, if you measured 4 times and used an average of the last two measurements you could write: “The BP monitor was programmed to take four measurements with a two-minute break between each measurement. For all tables and analyses, the mean value of the last two measurements was used”.

Results

# How many subjects had valid blood samples?
# 9% and 5.6% of the children were obese, how many were overweight?

Discussion

# In general, the discussion part has maybe too much emphasis on guidelines and maybe too much emphasis on studies in adults, and I miss more discussion pertaining your own results.

# Regarding the reference in the DISCUSSION part, it would be more natural this way: [9,19] rather than [19,9] (as it stands now).

# Regarding the statement: “there are amassing evidence”, I would like to see another adjective than amassing.

# Regarding the statement “In this study, we have found that more boys report high level of physical activity compared to girls. This finding is in keeping with the observations in Swedish high school students [27], which involved 993 high school students aged 16-19 years surveyed by questionnaires”. Your finding is not only in keeping with the observations in Swedish high school students [27], but in fact the most striking conclusion in the existing physical activity literature in children/adolescents is that boys are significantly more physically active than girls. I suggest that you re-write this sentence and include a reference from e.g. Riddoch et al. (2004) who applied accelerometers to assess activity levels in a group of more than 2,000 children from four European countries (EYHS). Riddoch CJ, Andersen LB, Wedderkopp N, Harro M, Klasson-Heggebo L, Sardinha LB, Cooper AR, Ekelund U. Physical activity levels and patterns of 9- and 15-yr-old European children. Med Sci Sports Exerc. 2004: 36: 86–92.

I also think that there needs to be more focus on the limitations in this study.

1.

The cross-sectional design does not allow conclusions based on causal inferences. This should be emphasized. As an example: the authors write in the conclusion part in the ABSTRACT: “Physical activity level is a potentially important modifiable lifestyle factor to reduce cardiovascular risk in children and adolescents”. Does your cross-sectional data allow this type of conclusion?

2.

The authors write: “… due to budget restrict, we used subjective measures of physical activity level by questionnaire rather than objective assessment tools like accelerometer. Nonetheless, using questionnaires are widely adopted in
many large-scale population studies with reasonably reliable data collected for analysis”. There are many different questionnaires used in different studies, and the limitations are often mentioned. The authors should do the same. Also, many large scale studies in fact measure physical activity levels subjectively (as the EYHS study (Riddoch et al. 2004) and the Avon Longitudinal Study of Parents and Children (ALSPAC) (Riddoch CJ, Mattocks C, Deere K, Saunders J, Kirkby J, Tilling K, Leary SD, Blair SN and Ness AR. Objective measurement of levels and patterns of physical activity. Arch Dis Child. 2007; 92:963-969).

3. Regarding the statement: “Third, the effect of regular exercise on bone health in these young individuals had not been addressed”: The authors could seriously consider to delete this sentence.

4. The study sample includes only children from Hong Kong, and overweight and obesity were defined by BMI greater than Hong Kong local age and gender specific 85th and 95th percentile respectively, raising questions about the generalizability of the results.

**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, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests' below