Reviewer’s report

Title: Occupational exposure to asphalt mixture during road paving is related to increased mitochondria DNA copy number: a cross-sectional study

Version: 0 Date: 28 Dec 2017

Reviewer: Hans Kromhout

Reviewer’s report:

This study focuses on PAH exposure of asphalt workers in Sweden and tries to relate biomarkers of exposure to two early effect markers. I have several major problems with the design of the study and the statistical analyses used.

First of all, not studying the asphalt pavers and their controls at the same time is a major flaw in the design of the study. Just acknowledging that this is a problem and that it was due to practical reasons is insufficient.

I would like to suggest that the authors at least perform a sensitivity analysis excluding the controls and pavers who were studied in early autumn and winter. The distribution of the remaining workers for all three groups is very similar. Since PAH exposure from road workers will also be determined by air pollution and in particular diesel motor exhausts from nearby traffic and in case of the asphalt workers their machines it is important to control for these exposures if the aim is to see whether CRM asphalt leads to higher PAH exposure that conventional asphalt paving.

Second I do not understand why the statistical models used differ when comparing PAH metabolites between the groups (controls versus the two asphalt workers groups) and when comparing pre- and post-working levels. Using a non-parametric test for the latter is not needed and too coarse. A linear mixed model with pre and post-working biomonitoring values as dependent variable and a pre/post dummy as explanatory value would be better.

Third, given that apparently also air monitoring has taken place (reference 14) I do not understand why these levels were not taken into account when trying to explain the biomonitoring levels. Just using three groups is very coarse when one actually has air concentrations levels during the survey.

Fourth, the authors do not explicitly address the difference between conventional and CRM asphalt workers in terms of the measured biomarkers. This also holds for the conclusion and in the abstract.

Fifth, the discussion can be considerably shortened. The paragraph on seasonal effects lines 305-313 does not make sense given that biomonitoring levels are compared to air samples without knowing what the air concentrations were during the biomonitoring collection.
Sixth, the discussion on the effect markers (lines 314-359 on page 14-15) is very speculative and in my opinion it makes no sense to compare coke-oven workers with asphalt (bitumen) pavers. The exposure of the latter will be at least one order of magnitude lower. I would like to suggest to shorten this section drastically and not compare asphalt pavers to coke-oven workers.

Some more minor issues

Table 1

Please add the (unadjusted) exposure variables (1-OH-PYR and 2-OH-PH) to Table 1

Table 2

This is very hard to interpret.

- what is the adjusted mean and how is it estimated. It is totally unclear where the adjusted mean stands for. For which age, BMI, smoking and snus status is it estimated?

- the last column should be taken out (see comment on the Wilcoxon test earlier.

Table 3

It is unclear what is presented in the fourth and eighth column. If it is the median difference it is very strange that in both cases the difference is larger than the both the median pre- and post-working. Was the difference not log-transformed? Why are you presenting medians in Table 3 and adjusted means in Table 2? It is very confusing.

Table 5

Why is the difference analysis restricted to the nested self-controlled analysis? Why not also for the total group?

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