Reviewer's report

Title: Smoking and primary total hip or knee replacement due to osteoarthritis in 54,288 elderly men and women

Version: 1 Date: 11 July 2013

Reviewer: Peter Lee

Reviewer's report:

This is a very well written and generally clear paper, with appropriate statistical analysis.

Major compulsory revisions
The only issue I have is that the sensitivity analyses were not well described.

As regards misclassification of smoking status, all one is told is that adjusted relative risks (RRs) were calculated under a variety of possible sensitivities and specificities, without saying what they were. As generally people do not falsely claim to be smokers, so that the sensitivity can be regarded as essentially zero, it would seem better to simply report how the RRs varied for a range of specificities, or even better (as being more comprehensible to the non-statistician), as to how the RRs varied for different assumed proportions of smokers denying smoking, e.g. 5%, 10% or 20%. As far as I can see, the statement that “the 2.5th and the 97.5th percentiles of the simulated distribution of bias-adjusted RRs were 0.03 and 0.58, and the median estimate was 0.33” will have no meaning to anyone without knowledge of the misclassification rates assumed, and will have little meaning even with knowledge. I also note that the second line of the sensitivity analyses has a statement “assuming trapezoidal distribution between 0.75 and 1” which means nothing to me. Distributions of what?

As regards accounting for unmeasured confounding, neither the methods section nor the results section gives the reader a vague clue as to what has been done. For a start, I do not know which variables the authors are considering that might have been relevant confounders. I also note that Figure 2 is labelled Figure 7. Whilst I applaud the general idea of carrying out tests to show that associations are robust to misclassification of smoking status and unmeasured confounding, one needs to be clear what was done. Given they are only sensitivity analyses, I suggest that the authors use additional files to describe what they have done in more detail, give more results, and cross-refer to these files in the main paper. As I indicated for the misclassification analyses, a simpler approach would be beneficial also.

Minor issues not for publication
Reading the text, I only noticed two minor English points:
p 5 “Study independent variables” line 1 – the word “baseline” appears twice.
Quality of written English: Acceptable

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

Declaration of competing interests:

I am a long-term consultant to the tobacco industry.