Reviewer's report

Title: Methodologies used to estimate tobacco-attributable mortality. A review.

Version: 1 Date: 4 April 2007

Reviewer: Tony Blakely

Reviewer's report:

General

It is probably useful at the outset that I declare my expertise in this area. First, tobacco-attributable mortality is not a major expertise of mine. However, I have recently had to grapple with understanding the Peto method and reconciling NZ findings that (in my view) were incorrectly estimated by the Peto method. I will list this work at the end of review, as I believe it contains useful information for this paper that (whilst published post 2005) needs including - even if just in Discussion. My comments, by necessity, are in part a reflection of this NZ-specific work.

To my knowledge, this would be the first review of all methods used to estimate tobacco-attributable mortality. This is welcomed.

To the best of my knowledge, all methods are listed. And they appear to be presented correctly - although I have not thoroughly checked the formulas and such like.

My over-riding general comment is editorial - the paper does not read easily, probably a function of the authors not having English as their first language? Examples on inefficient language include: "Under this heading one method was in evidence..." "The paper constitutes our knowledge ...

I think it is necessary for the journal, or possibly a colleague of authors, to give the paper a through editing to improve style and flow. This also applies to table captions and text, and so on.

-------------------------------------------------------------------------------

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

-------------------------------------------------------------------------------

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1st paragraph. I assume only 3 studies used 'individual analysis', ie those referenced? Might pay to be explicit.

-------------------------------------------------------------------------------

Discretionary Revisions (which the author can choose to ignore)

I concur with the authors' recommendation for simulation studies. It is a shame, however, that such a simulation study was not the focus here.

The last paragraph of Discussion is distracting - nowhere previously in paper was genetics mentioned. It reads as an afterthought at the moment.

Returning to my specific NZ experience, we have evidence of different smoking-mortality rate ratios by ethnicity in NZ, and different rates of lung cancer among never smokers compared to those in CPS. This, in my view, invlladed the Peto method applied in NZ using CPS data. And we are fortunate enough to have a full population cohort study with smoking data allowing direct estimation. To my knowledge, this is one of the few examples of a direct 'check' of many of the assumptions in the methods described in this paper. I therefore draw the authors attention to the following papers:

What next?: Accept after minor essential 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