Reviewer's report

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

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Reviewer: Anthony J Hedley

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1) General: This is an interesting and potentially very useful review of epidemiological and statistical methods used to estimate tobacco attributable mortality.

2) Major compulsory revisions: The paper needs extensive revision to improve layout and clarity. In particular the present English syntax makes it difficult to read and, in places, confusing and difficult to understand.

Page 3: Choose another form of words rather than “Having roughly accurate estimates …”. The use of ACCURACY and reliability as scientific terms is probably inappropriate in this paper. The focus should be on VALIDITY of the statistical and epidemiological methods. There is no gold standard. Where different methods have been applied in the same population then reliability, as repeatability, may be appropriate.

The Methods Section is far too sparse. The authors need to be much more explicit and detailed about the search strategy and the criteria used to discard the majority of the 372 papers retrieved.

They mention the influence of the tobacco industry but they need to categorise papers into their sources eg academic; other NGOs and regulatory bodies; and TOBACCO INDUSTRY SPONSORED. The latter are from sources which were paid in tobacco dollars to discredit the papers. I accept that they may make potentially valid statistical points but these should be discussed in that context. PN Lee is one such author whose consultancy works for the tobacco industry but the provenance of all papers should be known. Incidentally PN Lee is incorrectly listed as “Lee D” in the references.

Page 5: How were the papers which dealt with “mortality attributable to different causes” accessed; there is no mention of this.

The introductory description of the seven methods identified needs improving with sub-heads and clearer use of terminology. For example “hybrid” doesn’t mean much to the uninitiated. I suggest they rewrite this section as a more structured and formulaic summary of the methods. While some methods are represented in equations, others are discussed only using text. Standardization would greatly improve this section and facilitate the authors’ critique of the methods.

Page 6: The introductory comment on PAF (population attributable fraction) is confusing given that the discussion of estimation of PAF doesn’t appear until pages 7 and 8. It isn’t clear that if you haven’t obtained and used RR and exposure prevalence for a particular population then you have to use a PAF from another, usually overseas, population.

As pointed out elsewhere by the authors the PAF is a function of RR, exposure prevalence (at a given point/period) and of the maturity of the epidemic. So for example PAF for lung cancer is 90% and in Asia about 70% at present.

Page 6: SAMMEC is not necessarily different from the Lam (77)/McGhee (80) “hybrid” model because McGhee (80) also applied this to secondhand smoke.

Incidentally although McGhee calculated PAF these were not quoted in reference 80 (only RR were presented) but rather in a further analysis in which the community costs of tobacco were estimated (McGhee et al Tobacco Control 2006; 15:125-30).

I would suggest that the Doll study of the British doctors cohort could be quoted as a method because it was a well-defined cohort/population at-risk study with rigorous internal validity. The criticism of lack of “representativeness” (ie external validity) is not very substantial. It is a highly valid approach to assessing mortality from tobacco.
Page 9: Edit out abbreviations.

Page 10: English syntax a big problem here. The text also goes from specific (Peto, Lam, McGhee) to GENERAL (Garfinkel) to VAGUE (Rogers). The text could be made much clearer and more focussed and applied.

Page 15: I am not sure what they mean with the statement about “the dose-response gradient”. There is relatively little variation in amount consumed among regular smokers and even one cigarette/day is associated with significant mortality risks. Average daily consumption among regular adult smokers is usually high but may vary between countries. Of course data structure can always potentially be refined, but this is a different issue from a critique of the basic methodologies.

Page 16: One of the important points about the Lam/McGhee methodology is that it was based on ascertainment of active smoking/passive smoking 10 years before death and so avoided bias which may arise because sick people quit in the years closer to death. This is also relevant to the comment on PAGE 5 about studying tobacco use “in the studied populations prior to the start of the estimations”. However I think re-classifying quitters as non-smokers is invalid in view of the rate at which risk is shed and the residual risks associated with long term smoking.

Page 19: I do not accept that “Mendelian randomisation” has anything practically useful to offer the public health assessment of the impact of tobacco on mortality. Where smoking prevalence declines, so do population age-specific death rates from lung and other cancers and other tobacco-induced diseases. While accepting that individuals are more or less susceptible because of their genetic make up, in the bigger picture these populations have not changed their genes, only their exposures. For tobacco-induced disease, the biggest single preventable cause of mortality in most countries, valid estimates of tobacco mortality are important drivers of public health policy and rapid achievement of the goals of the WHO Framework Convention on Tobacco control.

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

**Level of interest:** An article of importance in its field

**Quality of written English:** Not suitable for publication unless extensively edited

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

No competing interests.