Reviewer's report

Title: An observational study to assess changes in social inequality in smoking-attributable upper aero digestive tract cancer mortality among Canadian males between 1986 and 2001

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Reviewer: Silvano Gallus

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The present study aims to estimate smoking attributable UADT cancer deaths by social class among Canadian men over the period 1986-2001, indirectly, using lung cancer mortality rates. The study parallels the methodology used in Jha et al., 2006 (Ref 19). The manuscript is clear, concise and well-written. However, more details are needed about data and methods used. Moreover, there are several limitations in the study design and in the methodology used (only partially addressed in the Discussion Section), that do not allow to provide reliable estimates, and, more importantly, to derive part of the conclusions.

I must admit that the only reason why I decided not to reject the present manuscript (but to require the resubmission after major revision) is the presence among the list of authors of Jha, who is a renowned expert on this issue.

MAJOR COMPULSORY REVISIONS

More details are needed about the data used:

1) Which source of data were used to obtain age and sex specific lung and UADT cancer death rates?

2) Did authors obtain the “5-year age, … disease-specific mortality data for… different social strata” (as reported in Jha et al., 2006; in this case, how social class was assessed for various deaths should be clarified) or did they obtain “age and sex specific death counts to lung cancer and UADT cancers” and then calculated the age-specific rates of each social stratum (as it is reported in the methods section; in this case the implemented computation should be clearly stated)?

3) Are mortality data satisfactorily complete for the urban Canada?

To avoid possible misunderstandings, mortality data and population data should be described separately. It is not clear how authors obtain the number of deaths by socio-economic level.

The Peto method (Peto et al., 1992), is one of the most used and reliable methodologies to derive overall smoking attributable mortality. However, as all the other methodologies (including SAMMEC), it provides reliable estimates under certain assumptions. My impression is that, once lung cancer mortality rates (by social class) are used to derive rates for one single (and relatively
uncommon) mortality cause, by social class and in different periods, the number of assumptions and the uncertainty level of the estimates provided dramatically increase. This methodological aspect should be discussed in the Discussion section.

Besides the possible methodological problems, the present study has several (other) limitations, including the relatively low number of UADT deaths: in fact, the main finding of the present report (i.e., the stronger decrease in UADT death rates for the lowest compared with the highest social class between 1986 and 2001) is due to an excess of only 20 to 30 deaths attributable to smoking among the lowest social class in 1986. In fact, decreasing the number of UADT deaths attributable to smoking by 26 deaths, only, we would obtain a comparable UATD death rates among low social class males, in 1986 (32.6/100,000) and in 1991 (32.5/100,000), and the strength of the main study finding would be totally or partially reduced.

Other limitations include the lack of a national (Canadian) mortality rate to derive RRs, although the use of US-CPS II cohort is acceptable. The use of aggregate data which does not allow to assess individual exposure and to provide allowance (standardization) for covariates is another limitation. Also, the study is relatively old, since it is based on data of more than 10 years ago. More importantly, the latency period associated with lung cancer is estimated in 20 to 30 years; thus, lung cancer deaths in 1986-2001 reflect smoking exposure of 1965-1980. Thus, when authors conclude that “tobacco control efforts in Canada have reached the men of poor or lower social strata” are authors referring to the tobacco control strategies implemented in 1965-1980 or in 1986-2001? Which tobacco control strategies?

It is clear that due to all the above-mentioned weaknesses of the present study, limitations cannot be considered “arguably minor” and “only subject to attenuation bias” (page 8; 2nd paragraph). Moreover, Peto’s method is a reliable methodology to derive overall smoking attributable mortality and possibly smoking exposure, but less reliable to derive current “smoking prevalence”. Despite reporting bias, well-conducted representative health surveys likely give more reliable findings. Therefore, the sentence “As for strengths of this analysis, prevalence of smoking in the study population was estimated indirectly… rather than considering prevalence rates from various health surveys…” is in my opinion incorrect. The main problem is that the major finding of this study is in contrast (opposite direction) with repeated, representative and large surveys conducted in Canada between 1999 and 2006 (Reid et al., 2010; Ref 16). This study should be discussed in the Discussion section, study limitations should be highlighted and the implication on tobacco control efforts should be deleted, at least from the conclusion sections of the Abstract and of the main text.

Figure 1 and Table 1 apparently show the same estimates, but the results are different. Please, double check!

**Level of interest:** An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable

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

Declaration of competing interests:
I declare that I have no competing interests