Swedish snuff and incidence of cardiovascular disease
A population-based cohort study

Reviewer's report

Title: Swedish snuff and incidence of cardiovascular disease. A population-based cohort study

Version: 2 Date: 6 November 2008

Reviewer: Peter N Lee

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While the question of whether Swedish snuff affects risk of cardiovascular disease is of scientific interest, the study is valid, the analysis seems sound enough and the conclusions clearly stated, the major problem with it is that it is extremely sloppily prepared and is more in the nature of an unchecked draft. Below I list 16 points prepared on going through the draft. The main general issues are:

a) Numerous evident errors in the data presented, particularly in Table 1 (see point 7 and also 10),
b) Inappropriate and incorrect citation of the available literature (see points 2, 11, 12, 13, 14, 16).
c) Poor English and presentation (see points 1, 3, 5, 9, 15).

While some of these are individually minor, the net effect is of a paper of a quality far too low for publication. Overall, these points would be considered major compulsory revisions.

The other points raised (4, 6 and 8) are scientific issues which can be classed as discretionary revisions.

1. In the Methods section, the numbers of women and men are given as 17206 and 11206. It would be better to give the numbers actually included in the analyses, 16754 and 10473, so that the numbers and percentages given in the results section are consistent.

2. The introduction cites seven other Swedish studies, as refs 2-8. There are a number more, 1-4 all cited in Lee’s recent review (ref 28).

3. Relative risks and 95% CI are expressed sometimes to 1 and sometimes to 2 decimal places. It would be better to be consistent, preferably at 2 decimal places.
4. In the methods section, is it true that smoking habits were recorded only as never, ex- or current smokers, without any information collected on amount smoked?

5. The third section on “information on socio-economic circumstances” is extremely long (despite starting “in short”), convoluted and unclear. What is meant by a “high” occupation? Is “high social class” meant? The sentence needs rewriting.

6. Why was stroke restricted to ICD 430, 431, 434 and 436 rather than covering the range 430-438?

7. In Table 1 the comparison is between percentages in non users and snuff users, with numbers providing additional detail. Therefore the results should be given as %\(n\) not \(n\%)\. It is not made clear in their table that the numbers for occupation add up to less than the \(N\) values given near the top of the table. This should be made clear in a footnote. Why are there two risk factors with the same name “occupation,” but different results? This should be clarified in the table. I cross-checked \(N\), \(n\) and \(\%\) and found numerous errors – all the entries in this table should be carefully rechecked. Some of the mistakes are:

   - Hypertension men non users 11799 should probably be 1799
   - Hypertension men snuff users 110/737 = 14.9% not 14.2%
   - Never smokers women non users 404 should probably be 7404
   - Occupation Low (2) women non users 3130 should probably be 8130
   - Occupation Others (2) men non users No bracket round 22.2
   - Civil status non snuff users 2412 should probably be 240

8. If there are data on numbers smoked, it would be very interesting to compare the average number smoked among current smokers between snuff users and non-users.

9. The first few sentences in “incidence of cardiovascular disease” need rewording. “Rather unchanged” means little. I suggest a version in the attached word file (see also comment 15).

10. In the same paragraph, the numbers of MI and strokes in never smokers are given as 5 and 4, but in Table 2 as 4 and 2.

11. In the first sentence of the discussion, three references are given to the statement. All these are to one author, and one cites a paper in 1950, long before effects of smoking were “accepted.” Why not cite major recent reviews, e.g.5,6?

12. In the next sentence, as support for the statement “lately more studies have been published,” five references are given, one in 1984 and relating to chewing tobacco in India, which seems fairly irrelevant.

13. In the second paragraph, reference is made to the recent smokeless tobacco review by Lee (ref 28), but only as regards blood pressure. Given that it was a recent review of stroke and MI also, why not cite that elsewhere? That reference is incorrect anyway, it is Lee, PN and the correct journal abbreviation is Int J Epidemiol.
14. In the last paragraph but one of the discussion, reference 24 is totally inappropriate, being nothing to do with snuff.

15. There are a number of errors of English or poor wording. These are indicated, with suggested corrections in the red-lined attached copy.

16. A very quick glance at the references lists reveals numerous errors also. I have not attempted to indicate these, but the authors should check them all against the source paper and also check the journal abbreviations.

References


What next?

Unable to decide on acceptance or rejection until the authors have rewritten the paper, though probably accept if my points have been taken into account.

Level of interest

As noted above, not suitable for publication unless extensively edited.

Statistical review

The errors noted are probably due to failure to properly check for typographical errors. The methods are OK – I am a statistician.

Declaration of competing interests

I am a long-term consultant to various branches of the tobacco industry and have
published widely on smokeless tobacco, though my reviews always present my own views of the literature. I have no other financial interests.