Reviewer's report

Title: Factors associated with nonattendance, spontaneous attendance and reminded attendance to cervical screening in an organized screening program: a cross-sectional study of 12,058 Norwegian women

Version: 2 Date: 24 December 2010

Reviewer: Stephen Morrell

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This is a clearly written paper and does not need too many major revisions.

Methods section:

Data sources: Why were women no older than 45 years surveyed?

Attendance definitions: Why weren't never attendees distinguished from non-attendees (no Pap smear registered within the last 4 years)? This group could be biasing results in the late attenders. Have the authors considered a sub-analysis of those who have never attended screening?

Results section:

Some numerical results along with text would help the reader. In numerous instances significant differences are reported but not their direction (ie, ORs<>1); eg: p.8: “Marital status also influenced attendance in the fully adjusted model, but here only the single women differed significantly from the married/cohabiting women.” Use of the words ‘negative’/’lower likelihood’ or ‘positive’/’higher likelihood’ as appropriate to indicate the direction of differences is always useful.

The statement, “In the age- adjusted model, attendance increased with increasing frequency of wine drinking. The same pattern was evident in the estimates from the fully adjusted model, though some of the individual contrasts did not reach statistical significance.” is not quite accurate. The pattern is not obviously the same in the fully adjusted model: the difference between the most frequent wine-drinking categories in the fully adjusted models is not significantly different from 1. However, a trend test probably would show it, if for example an ordinal quantity for wine-drinking frequency were modeled as a continuous variable (ie, with 1df) and if statistically significant then this would be evidence of a dose-response.

Table 1: Sample representativeness of the population has higher proportions of non-spontaneous attendees and lower proportions of non-attendees. This might be addressed.

Tables 2 & 3: The results for a number of covariates not significant with age-adjustment only are not reported in these tables from the fully adjusted model (eg, beer drinking, liquor drinking, number of new coital partners, etc).
Were these covariates excluded from the ‘full’ models and not reported? Or were they retained in the full models and not reported? If numbers were too small for including them, then this should be stated, or some other reason justifying not reporting them (e.g., the full model was parsimonious and based on stepwise elimination). If it is stated that statistical significance was not reached in the full model but no evidence is shown for this (e.g., no. of recent coital partners, p.9), then the authors could say ‘results not shown’, but why not just put them in the table and be done with it?

Use of the word ‘crude’ should really refer to unadjusted (univariate) estimates. All the ‘crude’ estimates reported in the paper are actually partially adjusted (for age). It would be preferable to use the term ‘age-only adjusted’, even if it is somewhat clumsy.

[Interesting: women believed Pap necessary every 3rd year for them were more likely to not attend]

Discussion section:
If there is possible ‘residual confounding’ between smoking and lower education, then why wouldn’t there also be between lower education and never-condom use in predicting non-attendance?

The statement, “There also seems to be consensus that women of low socio-economic status have a lower attendance to cervical screening [2, 22, 23]. Note, however, that studies confirming this notion have largely been performed in opportunistic screening settings.” (p.14) is not true. There have been numerous observational studies, using both population-based surveys and screening registry data, that show the SES-screening gradient to be more than a ‘notion’ in organised screening settings (e.g., in Australia).

The discussion of the increasing age gradient with reminded versus spontaneous screening (p.15) omits the important and well-known age-related explanation of ‘telescoping’, where periods of recall are more likely to be under-estimated with increasing age. An older survey respondent is more likely to recall something that actually happened 4 years ago as happening 3 years ago.

The statement, “Moreover, the data on the women’s lifestyle was based on self-report which may be erroneous and prone to recall bias.” (p.17) is not technically correct. Recall bias has a meaning specific to case-control studies, where case interviewees are likely to recall putative exposures or factors related to their condition differently than controls. There are no ‘cases’ in this study. Reporting on lifestyle choices is affected more by ‘social desirability bias’, where the respondent is more likely to offer a response they perceive the interviewer to judge as favourable, especially with lifestyle choices that might be frowned upon socially.

The authors need to justify why the upper age limit of the sample was 45 years. The representative of the sample needs to be discussed more in relation to differences in proportions between the population and sample, as shown in Table
1.

Finally, many studies of screening and health behaviours show strong ethnic differences. Not analysing ethnicity in regard to Norway’s cervical screening behaviours should be justified (ie, perhaps the relative homogeneity of ethnicity in Norway, etc)

**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