Author's response to reviews

Title: Personal factors influence use of cervical cancer screening services: epidemiological survey and linked administrative data address the limitations of previous research

Authors:

Sarah C Olesen (sarah.olesen@anu.edu.au)
Peter Butterworth (Peter.Butterworth@anu.edu.au)
Patricia Jacomb (Trish.Jacomb@anu.edu.au)
Robert J Tait (Robert.Tait@anu.edu.au)

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Author's response to reviews: see over
Dear Editor,

Please find attached the revised manuscript entitled: *Personal factors influence use of cervical cancer screening services: epidemiological survey and linked administrative data address the limitations of previous research.*

We thank the editorial board for the opportunity to address referees’ comments and resubmit this paper for consideration for publication in *BMC Health Services Research*. We thank the referees for their learned comments and suggestions, which we believe have resulted in further improvements to the manuscript.

We have addressed each comment in detail in the order they appear in the referees’ reports. Referees’ comments are quoted in italics.

All authors once again declare that they have no conflict of interest and approve this resubmission. The manuscript is not currently submitted or published elsewhere.

Yours sincerely,

Sarah Olesen and Co-authors
Response to reviewers

Referee 1: Marc Arbyn

INTRODUCTION


This replacement has been made (see Reference [1]).

2. The authors are right in stating that self-reports often yield biased (overestimated) results compared to more reliable sources (medical files, administrative records from health insurers). We also found a substantial discrepancy between estimates in cervical cancer screening coverage in Belgium (% of women aged 25-64 having had at least one Pap over the last 3 years) according to the source of information: estimated from telephone survey > estimates from health interview surveys (home visits): 70%, > 59% from reimbursement claims. See Arbyn, Quataert et al, Eur J Cancer Prev 1997; Arbyn, Simoens et al, Prev Med 2009.

We thank the referee for pointing us towards these additional references and have now included them in the manuscript (References [23, 24]).

RESULTS

3. Add coverage estimates assessed from the self-reports.

PATH participants were not asked to self report cervical cancer screenings as this information was (as the referee notes) more reliably obtained from administrative records. We have made this clearer in the Measures section to address this comment (lines 116, 125).

4. Table 1. Add “and non-imputed” after unweighted in the column headers.

Referee 2 requested that non-imputed data be used for all analyses. This has been done and thus the above point need not be addressed.

5. Under missingness: add “proportion”

The column heading for missing data in Table 1 has been changed to “% missing data” to address this comment and Referee 2’s point 1.4.
6. A 95% CI around the PAR should be reported.

This has now been included (see lines 246, 251-2)

DISCUSSION

7. It would be interesting that the authors contrast coverage from the self-reports with coverage from the administrative records as discussed in the introduction.

Please refer to point 3 above.

8. The computation of PAR is correct but its interpretation that non-screening will reduce non-screening with 74% is too absolute and naive. It assumes a 100% effective action in all the women with cited risk factors. One needs evidence that by an intervention tackling the inequality factors one can increase screening coverage with a certain fraction. The authors could recommend research on effectiveness of such interventions.

We have qualified our interpretation of this (PAR) finding and recommendations in response to this comment (lines 242-51, 262-274). We thank the referee for their suggestion.
1. MAJOR COMPULSORY REVISIONS

1.1. Please tone down some of the claims regarding previous approaches and what this paper contributes. It is not appropriate to describe previous approaches as “flawed” and to say that your paper addresses (all) previous methodological limitations, and is “unique” and “optimal”. All epidemiological approaches have strengths and weaknesses; I suggest saying that this research addresses some of the methodological limitations of previous approaches and represents a useful approach, for the following reasons....

We have revised the language throughout the manuscript to address this comment, including the specific examples provided by the referee (lines 32-4, 48, 77-79, 98-104, 277-81, 296, 317-9).

1.2. Remove the multiple imputation of missing values from the main results. Although it seems to be accepted methodology to impute missing values in the main results in certain parts of the social sciences and, I think, psychology, it is not generally accepted practice in epidemiology. None of the other papers published in this area have, to my knowledge, used this in their main results. It is occasionally done as a sensitivity analysis where missing values are a problem, but not for the main results. This illustrates a general issue with this paper, in that it does not seem to be modelled on previous epidemiological papers (see my comments on the tables, below). I suggest restricting the data to those individuals with non-missing values on variables considered important and introducing a “missing” category for other less important covariates. Alternatively, you could restrict the data to only those with non-missing data. A sensitivity analysis using imputed values could be introduced separate from the main results.

We thank the referee for their advice regarding consistency between our paper and others in the area. We are aware of recent epidemiological papers that do impute missing values in their main analysis within the cancer [1, 2] and health services literature, including those from BMC Health Services Research [3-5], and methodological papers that recommend this method in epidemiology [6]. We had thus chosen to model our method on this recommendation and the style of the present journal. As noted in the manuscript (line 240), sensitivity analyses revealed the same pattern of results for imputed and unimputed datasets.

However, we greatly value the expertise of the referee and thus modify the manuscript to include analyses from the unimputed dataset (i.e., complete cases) as recommended. We have changed our comment in line 240 to reflect that imputation was considered and produced the same results as the presented, unimputed analyses. Sample characteristics from the imputed dataset have also been removed from Table 1.

The referee also suggests the possibility of “introducing a “missing” category for other less important covariates”. We feel that this is not appropriate due to the very small amounts of missing data on covariates (see Table 1). We thus loose few respondents by restricting the sample to complete cases...
and follow the referee’s second suggestion of “restrict the data to only those with non-missing data”.

1.3. Consider removing the adjustment for use of Medicare services from the main analysis. As the authors have noted, cervical cancer screening generally takes place at a primary care consultation, so it seems strange to adjust for use of primary care services. Although only one of these services is likely to be the one where the screening took place, use of primary health care services is causally related to cervical cancer screening and there is a risk of over-adjustment in adjusting for use of Medicare services.

In addition, there is also a risk of answering a different question from the one outlined in the aims (“verify the characteristics thought to be associated with cervical cancer screening”), i.e. the authors are essentially saying “given a particular level of primary health care service use, which factors are associated with cervical cancer screening?”.

It is, however, an important finding that the main predictor of cervical cancer screening uptake in this population was overall health services use. It would also be reasonable to explore how much of any association between a given characteristic and cervical cancer screening was explained by use of Medicare services, by showing the findings without and then with such an adjustment. This has sort of been done for the PAR.

A potential approach would be to examine the association between overall health services use and cervical screening uptake but not to adjust for it in the main analyses. A subsequent analysis could explore the effect on the other OR of adjusting for it.

We share the referee’s concern that adjustment for overall health service use may confound the model for cervical screening service use as the behaviours are obviously related. However, we were equally concerned that our model identified determinants of cervical screening rather than health-service utilisation more generally. To our minds, this latter concern outweighed the former and led us to the more conservative approach presented in the manuscript (i.e., including/adjusting for overall service use in the multivariate model for cervical screening uptake; Table 3). And as the referee points out it is “an important finding that the main predictor of cervical cancer screening uptake in this population was overall health services use”.

The referee’s suggestion of showing the multivariate model with and without adjustment for overall health service use ameliorates both of the above concerns. We have thus followed this recommendation in the revised manuscript. In line with the presented PAR results, we first present this multivariate model with adjustment for overall service use, then without to demonstrate the impact of this covariate on other variables in the model (see the new ‘Model C’ in Table 3). The minor differences between Models B and C in Table 3 are reported in lines 231-240.

1.4. Model the tables for the paper on established epidemiological conventions (e.g. Moser et al [1]). Tables should include:

- the “n” for each level of the variable
- **intuitive values for the proportion each of these represents (e.g. %)**
- **numbers and OR for all categories, including the reference group**
- **enough information so that the tables stand alone, including indicating what has been adjusted for.**

Each of the above four changes has been made. In Table 1 this includes: the “n” for each category of the categorical variables. In Table 3 this includes: ORs for all categories (including reference categories) and more detailed information about the variables included in each model.

*In addition:*

- it is usual to present the odds ratio adjusted only for age, then a multivariable model; the totally crude OR is difficult to interpret and can be reconstructed from the “n” if the reader really wants to know it.

Our sample includes a narrow cohort design with two age cohorts, each ranging only 4 years. There is no standard/precedent for presenting age-adjusted ORs for this design. If we adjusted for age cohort (i.e., a dichotomous variable), the ORs would refer to associations between the covariate in question and cervical screening for the **reference category** of this variable (i.e., the younger cohort). If we adjusted for continuous age (as is often done), the ORs would refer to associations between the covariate and cervical screening for women of the **average age of these cohorts** (56 years) – an age that is not actually represented in the sample. Neither of these options is easily interpretable or meaningful and was thus not carried out. To ameliorate concerns about the role of age in the associations between covariates and cervical screening we thus performed a series of sensitivity analyses that explored interactions between age cohort and each covariate (i.e., whether the associations between covariates and screening differed across cohorts). This is reported in lines 184-9.

- it is unclear what the item called “missingness” refers to

This term has been replaced with “missing data” (lines 173-4, 201, Table 1).

- it seems strange to have two separate covariates for “did not finish high school” and “tertiary educated”. These would usually be included in a single educational variable, with multiple levels.

This has now been done (see Tables 1 and 3).

2. **DISCRETIONARY REVISIONS**

2.1. The paper could benefit from a more extensive literature review, as there appeared to be a number of relevant papers missing.

Several additional references have been included – References 1, 15, 16, 17, 23, 24.
2.2. Although it is customary to weight samples when estimating and attempting to generalise from prevalences, it is not usually done for internal comparisons in cohort and cross-sectional epidemiological studies.

Our reading of the literature on the epidemiology of cervical screening indicates that whilst some studies have not applied weights, many recent studies have [7-9]. We elected to apply weights across all analyses for consistency, as we present both prevalence rates and ‘internal comparisons’ (i.e., logistic regression models to examine the correlates of cervical screening in the PATH sample). As reported, we conducted sensitivity analyses and found no appreciable differences in the pattern of results for weighted and unweighted samples (line 240).

2.3. The authors may want to consider toning down their policy recommendations. The authors state that “programs should expressly target women who are not working, reliant on social welfare, currently smoke, do not have children, have poorer physical functioning, high levels of anxiety, a history of sexual abuse and low overall levels of health service use.” Obviously, the policy agencies need to maximise screening overall, with limited resources, so they may not be in a position to expressly target these groups. Also, we don’t know what would actually happen if we did target these groups.

A similar suggestion was made by Referee 1 (point 8.) We have amended our recommendations to reflect these suggestions in lines 266-273, and 322-325.

2.4. The authors state that cervical cancer screening is usually done by a woman’s GP. While screening is usually done in general practice, it is often done by a practice nurse; it would be helpful if this was clarified.

This has been clarified in line 127.
References


