Author's response to reviews

Title: The validity of self-reported cancer screening history and the role of social disadvantage in Ontario, Canada

Authors:

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Author's response to reviews: see over
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Dear Mr. Vargas and Dr. Oksanen,

Thank you for the opportunity to revise and re-submit our manuscript (MS: 4366638631198127 - The validity of self-reported cancer screening history and the role of social disadvantage in Ontario Canada) to BMC Public Health. Please find below a point-by-point response to the concerns raised by the reviewers.

Reviewer 1:

Major Compulsory Revisions

The conclusion that "Future research should focus on education strategies that enable patients to be more aware of their screening status." seems far fetched and infeasible, and distracts the reader from the other main important finding that self-reports should not be relied upon when estimating screening utilization. Please remove instances of these statements in the abstract and discussion.

We appreciate this reviewer's point and have accordingly removed all references to this form of future research in both the abstract and discussion of the manuscript.

Methods: "The CCHS data to which we had access contain responses from approximately 30,000 Ontarians per cycle who agreed to data linkage with administrative health data" Did all CCHS respondents consent to linkage as part of the study generally or was this a separate consent process, and if so, what % did consent to linkage?

The Canadian Community Health Survey (CCHS) is a national biennial survey conducted by Statistics Canada that collects information related to health status and health care utilization. There are approximately 50,000 respondents in Ontario per cycle, with approximately 30,000 Ontarians agreeing to data linkage with administrative health data. Numerous studies (including ours) based at the Institute for Clinical Evaluative Sciences have been conducted using these linked data. We have now added to the manuscript that approximately 60% of Ontario CCHS respondents had their data linked.

Inclusion/Exclusion criteria: "People were included if they were in the linked CCHS database, had answered the questions about cancer screening..." What about people who did not report any screening and therefore may not be in the linked database simply due to lack of an exam to link to? This point needs clarification.

“The linked CCHS database” refers to the 30,000 Ontario respondents per cycle who agreed to have their responses linked to administrative health data. For clarity, we have re-worded this sentence to now read “Survey respondents were included if they had answered the questions about cancer screening and were eligible for
cervical cancer screening (cervix group), breast cancer screening (breast group), or CRC screening respectively (CRC group)

Next page: "Eligibility for CRC screening was defined as ... no history of barium enema or sigmoidoscopy in the preceding five years, no history of colonoscopy in the preceding ten years." By CRC screening do the authors really mean fecal testing only? If so that should be stated more clearly up front, as this reviewer was surprised to see these potential CRC screening procedures used as exclusion criteria. This point needs clarification.

We appreciate the reviewer’s point and have clarified throughout the manuscript that we were specifically examining fecal occult blood testing in this study.

Discussion: "The high false positive rate implies that particular efforts are needed to address issues of over-reporting of FOBT use" How would you intervene on that in such a way to improve population-based recall of FOBT? Please delete this sentence and replace with a conclusion related to the impact of over-reporting on estimates of utilization, or the like.

We have edited this sentence to now read "The high false positive rate implies that estimates of FOBT utilization based on self-report could be particularly erroneous and should be avoided where possible, and our finding of high true negatives is in line with the known low prevalence of CRC screening by FOBT in Ontario".

Discussion: The paragraph that starts "We have also highlighted several subgroups that are more likely to over-report cervical cancer screening in Ontario than their counterparts," could provide more interpretation. In fact over-reporting of cervical cancer screening (defined as 1-Sp) was consistently lower for subgroups typically thought of as more disadvantaged (foreign-born, Non-White, recent immigrants, though really over-reporting was high for all subgroups.

Taking into account that we have added in a regression analysis to the manuscript, we have now edited this paragraph to read, “There was no clear pattern suggesting that Ontarians with social disadvantage were consistently more likely to over-report than their more advantaged peers. For example, in regression analyses, women who were White, more educated, English native speakers and of higher income were less likely to over-report cervical cancer screening in Ontario than their respective counterparts, but this did not hold true for the other screening types. The higher sample size in the cervix group may have driven the higher number of significant results for cervical cancer screening than for the other two forms of screening. However, we have demonstrated that sociodemographic characteristics do appear to play a role in Ontario populations and should be considered when examining cancer screening gaps based on self-reported data.”

Table 1: It would be much more informative if Table 1 were reorganized to include the number of people, rate of documented screening, and rate of
reported screening, separately for cervical, breast and CRC.

We appreciate the reviewer’s suggestion. We believe it is best to reserve Table 1 to describe the baseline demographic characteristics of each group of participants, to provide the readers with some context. However, Table 1 includes the number of people in each screening group, and we have now edited the three figures to include the rate of documented screening and rate of reported screening.

Table 2- It would be nice to see the reports to records ratio perhaps in place of the likelihood ratios which are hard to interpret in this context, whereas report to records ratio is a straightforward measure of over-reporting.

The likelihood ratios have now been removed from Table 2. The report-to-record ratios have been reported and displayed separately in Figures 1-3.

Reviewer 2:

Major compulsory revisions
1) the definition of telescoping and its analysis (Table 3) is incorrect. The authors use the PPV as criterion, and conclude that if the PPV increases when they expand the time frame of the gold standard, this implies telescoping. But if the definition of a positive (gold standard) is broadened, the PPV must increase. This is inevitable, not specific to telescoping. To show telescoping, the authors should look at specificity: specificity should improve as the definition of a positive is broadened, because telescoped events are now removed from the "negative" column.

We appreciate the reviewer’s suggestion. We have accordingly removed the telescoping analysis and its associated Table 3 from the manuscript. We have instead provided a new Table 3, outlining the results of the newly added regression analysis as suggested below.

2) In the tables the authors show lots of stratified analyses, but do not signal significant differences between strata. Add interaction tests, and then comment on the significant differences.

We have now added a multivariable regression analysis to highlight which variables are independently associated with screening and to highlight differences between strata. The new Table 3 describes this regression analysis and it is discussed throughout the manuscript.

3) Describe in Methods how the report/record ratios were obtained. McNemar tests?

Report-to-record ratios were calculated as the ratio of the proportion of women who reported a screening test during the relevant time period to the proportion of
women who had a record of a screening test during that time period. Confidence intervals around the report/record ratios were obtained using the method of Smith (1966). This reference has now been added to the manuscript.

4) revise the abstract so it addresses the issue of deprivation that is announced in the title and the intro of the abstract. The results are uninformative about this.

The abstract has been edited to address the role of sociodemographics, specifically by discussing the multivariable regression analyses.

5) In the conclusion (abstract and paper), the authors call for educational strategies to help people remember their screening status. This is totally beyond the available data. Why would education be the answer here? There are many other options: ask better questions, rely on records instead of reports, give everyone a smart card with actual screening records, etc

We have removed the call for educational strategies from both the abstract and discussion sections of the manuscript.

6) describe the study population and the study design at the beginning of Methods, then the study variables, and then only the datasets.

We appreciate the reviewer’s suggestion. However, we have chosen to begin this section with a description of the datasets as this sets the stage and provides context for describing the study population and study design. For example, the Canadian Community Health Survey was one of our datasets and we believe it is important to describe the CCHS before defining the study population. We describe the datasets, then the study population, study design and variables.

7) In the Discussion, answer the research question - what do you think about the effect of deprivation on recall?

In the Discussion, we have included the following paragraph: “There was no clear pattern suggesting that Ontarians with social disadvantage were consistently more likely to over-report than their more advantaged peers. For example, in regression analyses, women who were White, more educated, English native speakers and of higher income were less likely to over-report cervical cancer screening in Ontario than their respective counterparts, but this did not hold true for the other screening types. The higher sample size in the cervix group may have driven the higher number of significant results for cervical cancer screening than for the other two forms of screening. However, we have demonstrated that sociodemographic characteristics do appear to play a role in Ontario populations and should be considered when examining cancer screening gaps based on self-reported data.”

Minor essential revisions
1) ratios should not be given to the 1000th
Figures 1-3 have now been edited so that report-to-record ratios are no longer given to the 1000th.

2) The first paragraph of Results should describe the 3 populations

We have edited the first paragraph of the Results section to provide more description of the three populations.

3) The authors talk about "cohorts", but this is a cross-sectional study

We have removed all language referring to “cohorts” and now say either “group” or “screening group”.

4) The tables are data-heavy. Consider removing the LRs. If you keep them, note in the discussion that LRs of about 2 are generally considered useless

The likelihood ratios have now been removed from Table 2.

5) I would think that the tables should be within the main paper, not additional material.

Due to BMC requirements, the length of the tables makes it necessary to include them as additional material.

Discretionary revision

1) I believe that the paper would be more interesting if the authors examined the predictors of overreporting of screening. I would use only the "negative" groups (by gold standard), and look for predictors of a positive report, using logistic regression

As described above, we have now added the suggested regression analysis to the manuscript.

We thank you again for the opportunity to revise and re-submit our manuscript and look forward to your response.

Sincerely,

Dr. Aisha Lofters