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Title	A cross-sectional population-based study of breast cancer screening among women with HIV in Ontario, Canada
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Reviewer 1	Dr. Wilson Pace
Institution	University of Colorado, Family Medicine, Boulder. Colo.
General comments (author response in bold)	<p>Strengths</p> <p>This is an observational study of mammography rates over a 2 year period of time in women with and without HIV living in Ontario Canada. The aim of the study is to describe breast cancer screening activities at the population level in the province, comparing HIV positive women to the HIV negative population. The question has increasing relevance as individuals with HIV continue to live longer lives and reach the age for a greater number of preventive services, including breast cancer screening. The study uses a number of linked databases maintained by the Institute of Clinical Evaluative Sciences (ICES), an agency funded to create linked datasets for observational studies of this type. The data sources and prior linking across the various data bases, which is done by ICE, are a great asset to the study and investigators. Given universal coverage for mammography in the age group studied (50-74) and universal mammography reminders sent by the provincial government to women in the age cohort the study environment provides an excellent back drop to examine the screening decisions of HIV positive women.</p> <p>The methods are reasonably described. The impact of data linkages at the individual level is mentioned in the STROBE statement section but not in the body of the manuscript. While data loss appears to be minimal a flow diagram describing this data loss across the analytical framework should be included, at least for the HIV positive women. The analytical variables were either clearly delineated in the databases of interest (receipt of mammography) or previously validated (identification of HIV positive status.) The outcome data is provided both as absolute differences and relative risks with and without controlling for differences in the underlying populations. Though the sheer volume of information takes time to work through the presented information allows the reader to clearly understand the results.</p> <p>The overall results as represented in the tables are well laid out. The discussion mostly reiterates the findings. The importance of engagement with a regular primary care site/physician is highlighted across both populations based on enrollment versus non-enrollment and total number of visits. The limitations are reasonable discussed.</p> <p>We thank the reviewer for the careful review and thoughtful comments regarding our paper.</p> <p>Concerns</p> <p>1. As mentioned above a flow diagram of data losses across linked datasets should be provided at least for the HIV positive women.</p> <p>The flow chart itself cannot be attached within the online text box provided, but is available in the uploaded text version of this letter.. We would be happy to include this as a figure or appendix if the editors feel it would be helpful for readers.</p> <p>2. The primary results paragraph is terse and difficult to follow. The sentences following the statement of lower rates of breast cancer screening for HIV positive women appear to relate back to the full study population but this is impossible for the casual reader to discern from the way it is written.</p> <p>Thanks to the reviewer to pointing out the challenges in reading this paragraph on page 10. We have made several changes to the results section to delineate more clearly the analyses we are speaking to. In particular, we make sure to indicate the analyses that consider the complete cohort of both women with HIV and women without HIV, and which are conducted among only women with HIV. We have also added subheadings for these analyses in order to improve the flow for the reader: "Analyses of women with HIV compared to women without HIV" and "Analyses restricted to women with HIV."</p> <p>3. The overall outcomes report non-adjusted results. The paragraph describing adjusted results appears to apply only to women who are HIV positive but this is only evident by referring back to the tables and tracking the reported relative risks. A statement or short paragraph concerning the impact of multivariable adjustments for the full cohort should be included. The HIV relevant results would then logically follow this information.</p> <p>We can appreciate the reviewer's concerns about the ease of following our results section. As mentioned above, we have made several changes and added appropriate subheadings to facilitate this flow.</p> <p>4. The discussion or interpretation section initially just restates the results. The initial paragraph adds little. The entire discussion seems to primarily comment on the findings of other studies for non-HIV positive women as opposed to actually examining the data at hand. For instance, a possible explanation for lower rates of mammography screening in HIV positive women focuses on previous work around competing demands in primary care. This is somewhat at odds with the actual reported results which indicate that among the entire population as the number of co-morbidities increase the likelihood of screening increases while among HIV positive women the number of co-morbidities has no impact of the likelihood of receiving mammography. Another interesting finding in the secondary analysis is that immigration status is associated with lower overall mammography rates in the general population but has no impact on screening rates among HIV positive women (though the results are impacted by very low numbers in some subsets of HIV patients.) Finally, while essentially all medical research papers end with more research is warranted, this study finds that across a population of just under 1.5 million women approximately 42 fewer women with HIV were screened compared to the rest of the eligible</p>

	<p>population. Comments concerning provider awareness seem very reasonable but the need for significantly more research may be somewhat overstated given the overall population impact of lower screening rates in the HIV positive population.</p> <p>The reviewer makes important points about the interpretation of our data. In response, we have made several changes to the interpretation section.</p> <p>We intended our analysis to convey two key messages. The first analysis, which included all women (with and without HIV) was intended to compare the prevalence of screening among women with HIV to those without HIV, after adjustment for potential confounder and predictor covariates. As such, aside from HIV status, we were less interested in the predictors of screening among all women as these have been documented previously. The second analysis, which was restricted to only women living with HIV, was intended to unveil predictors of screening among women with HIV. To make this clearer, we have now presented details for both the unadjusted and adjusted models in the results section on page 11. One main limitation with this second analysis is its smaller sample size among women with HIV, and we have ensured this is stated in the limitations paragraph of the interpretation section on page 13.</p> <p>We have restructured the interpretation section to provide more context to our results. As per the editorial request, we have kept the purpose of the first paragraph to be to restate the main findings. In the second paragraph, we have compared our screening rates to other studies among women with HIV, and have described associations between patient characteristics explored in our study with those reported in the literature. In the third paragraph, we focused more succinctly our findings with respect to practice characteristics associated with screening. Finally, we have moved our recommendations to the concluding paragraph, and have tempered these recommendations to reflect this reviewer's concern that we remain circumspect in our conclusions.</p>
Reviewer 2	Dr. Michael Green
Institution	Queen's University, Family Medicine, Kingston, Ont.
General comments (author response in bold)	<p>Nicely written paper on cancer screening in HIV positive women. The study used population level administrative data and was well done with appropriate methods and adjustments for known or postulated confounders. As with any study using this sort of data there are some limitations but these are nicely documented in the paper.</p> <p>We thank Dr. Green for his feedback.</p> <p>1. My only suggestion is that there be more specific detail about the existing literature on this subject. The references to prior work in Utah and here in Ontario were too vague to allow the reader to easily see how these results support or contrast with those from other studies.</p> <p>We thank the reviewer for their feedback and hope the extensive changes to our interpretation section respond to his concerns.</p>