

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	A population-based study comparing patterns of care delivery on the quality of care for persons living with HIV in Ontario
AUTHORS	Kendall, Claire; Taljaard, Monica; Younger, Jaime; Hogg, William; Glazier, Richard; Manuel, Douglas

VERSION 1 - REVIEW

REVIEWER	Stephen Berry Johns Hopkins University United States of America
REVIEW RETURNED	23-Feb-2015

GENERAL COMMENTS	<p>The investigators apply an innovative and previously-validated care-typology approach to one of the most important questions in HIV healthcare: whether HIV care coordinated primarily by a primary care doctor or an HIV-specialist will promote better overall health outcomes. Despite the growing importance of this topic, it is a difficult one to study. This work represents a potentially valuable contribution to the literature because it is able to provide comparative data on this topic. An important strength of this study is that the investigators are able include almost all persons engaged in HIV care in the entire province of Ontario, capturing both major urban and rural settings.</p> <p>Major Critiques:</p> <p>1. Abstract Conclusions / Page 9 Text Box 1 / Discussion paras 1, 3, and final para: The investigators seem to underestimate the likely impact of HIV disease stage to contribute to ART prescription outcomes, hospitalizations, and cancer prevention outcomes. It is highly likely that patients with advanced HIV stage (low CD4, active OI's, history of previous treatment failures and resistance) will be referred by primary doctors to specialists. Once prescribed ART by specialists and once engaged in treatment for OI's, they will likely return to the specialists for therapeutic monitoring. This would have the effect of raising the ratio of specialist to primary care visits, which is the defining point for primary care provider vs. specialist dominant care. Hence, ART prescription is likely to cause specialist dominant care rather than the opposite. Having a low CD4 is also likely to drive hospitalizations. Multiple studies of risk factors for hospitalizations have shown that low CD4 is the most important independent risk factor for both ADI and non-ADI hospitalizations [JAIDS 2012;59(4):368; AIDS 2008, 22:1345; JAIDS 2010;54:248]. Finally, having a lower CD4, and especially having active opportunistic infection(s), decreases the clinical urgency for cancer prevention. It is hard to send someone for a screening colonoscopy if he or she is battling disseminated MAI. There are several places in</p>
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which the discussion of findings might be modified to account for this large limitation. First, it should be placed in the abstract rather than relegated to the 3rd bullet of the article summary. I would also suggest being more specific about it in the abstract and not simply referring to “residual confounding”. Second, the strength of conclusions in places like Abstract Conclusions and Discussion para 1, should be decreased. Phrasing such as “had a strong influence” (Abstract) and “confirm on a population level...” (Pg 20 / line 36 and pg 23 / line 46) are too strong. Because of this limitation, the results are at best suggestive and hypothesis-forming. Conclusions should be phrased with this in mind and the 3rd para of Discussion (or another paragraph) should discuss how potentially important this limitation is and that low CD4 is likely to be a contributor to the observed outcomes of not just ART prescription but hospitalizations and cancer prevention as well. It would also be important to point out the potential reverse causality of ART prescription on specialist-dominant care.

2. Methods / Results / Discussion: The data on diabetic eye disease screening are important in that they represent a contrast to the finding for cancer-prevention outcomes. The Methods section should describe this outcome. This outcome should probably be characterized as a primary-care type outcome. The text in Page 20 / line 26 lumps it with specialty-care, but since HIV specialists are not diabetes specialists, it seems better-placed with primary care outcomes. Since the findings on diabetic eye disease prevention contrast with those of cancer prevention, Discussion 1st para (page 20 / line19) and Discussion 2nd para should refer specifically to “cancer prevention” as being better performed under primary care dominant types of care and better highlight the fact there was no difference for diabetic eye screening.

3. Figure 2 / Discussion 1st para (pg 20 / line 14): It would be helpful to present the full multivariate regression results which underlie Figure 2 as a Supplemental Table (or series of Supplemental Tables) and to describe the main relevant points in the text. Without all multivariate OR’s presented, there is no support for the statement on page 20 / line 14 that how care is shared between primary and specialist is “by far the most important” factor on the outcomes studied. How much more important was it that number of visits, than comorbidity scores, than demographics, etc...?

Minor Critiques:

1. Intro. Page 5 / Line 54: References should be given for the statement that people with chronic conditions who don’t have a family physician are more likely to have emergency department visits and hospital admissions”.

2. Methods. Page 9 / Line 26: How were patients assigned who were contractually rostered to a family physician but never actually kept an appointment with the family physician while instead keeping appointments with a specialist?

3. Methods. Page 12 / Line 36: The N for those eligible for Ontario drug benefits might also be written here (it is appropriately given in Table 2) to help readers.

4. Table 1: Number of kept visits to both primary care and specialists should be included in Table 1 since it is a determining factor for the

	<p>care typologies and since it may be an important contributor to outcomes.</p> <p>5. Table 2 / Methods: How were HIV-specific hospital admissions determined?</p>
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REVIEWER	William Mkanta Western Kentucky University, United States.
REVIEW RETURNED	26-Feb-2015

GENERAL COMMENTS	<p>The outcomes may not be relevant to all patients included in the study, immigrants may exhibit different patterns compared to Canadian-born patients. Presentation of the logistic regression results Fig 2 could be better if converted into a table and shown in numerical values to provide a better understanding of the OR magnitudes and differences.</p> <p>Overview: The authors present an important study that is relevant in the current era of HIV/AIDS. Medical advancements have made this disease to be widely managed as a chronic condition especially in the western world. Although this is encouraging, people living with HIV/AIDS are now faced with challenges associated with longevity of their lives with the disease. I think the information obtained from this study would be helpful to health care providers and systems as they prepare to deal with patients who live longer with HIV.</p> <p>Abstract: The Abstract is good. It gives the whole picture and what exactly was needed to be conveyed through this paper. All the objectives of the paper were properly included in the abstract, and it describes clearly what the article is set out to accomplish. However strengths and limitations of this study were included in the article summary. I don't know if this is the requirement of the journal. Otherwise, I would prefer to have this information placed elsewhere at the end of methods or in the discussion.</p> <p>Introduction: The introduction is simple and relevant. It gives a brief overview of issues leading to the objectives of the study. Relevant literature review was made regarding patients with chronic conditions who don't have a regular family physician. Authors used administrative data to measure care for several populations in Ontario; they presented models that included exclusively primary care, shared care with the family physician being the dominant HIV physician, shared care with the HIV specialist being the dominant HIV physician, exclusively specialist care, and low engagement. Overall, introduction helps you understand the key issue with proper background information. As this study is engaged with the experience of HIV patients who live longer with the disease. Some background information on the changes in longevity of life among these patients should have been included to give the reader an idea of the extent of longevity attained by patients living with HIV/AIDS.</p> <p>Methods: The data sources that were used in this cross sectional study were taken from the administrative databases at the Institute for Clinical Evaluative Sciences. Most relevant data has been gathered. But the authors have used many pieces of data to come up with their study</p>
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	<p>group. I hope they were able to do an adequate job of tying together the patients without incurring any losses in the information used for analyses.</p> <p>Outcome measures and Analysis: The outcome measures were relevant for the study; they reflect the groups of conditions that have contributed to the volume of chronic disease in North America. Methods of analysis were also in line with the type of data and objectives of the study.</p> <p>The only major issue I see here is the silence on the trends of care for immigrants. The authors indicated that this group may be challenged differently with the disease compared to Canadian-born patients. Other points to consider would be (1) these patients are coming from different healthcare systems; their pattern of use and healthcare seeking behaviors may not be clearly understood if assessed together with the natives; (2) these patients may be coming from environments where different types of chronic conditions other than those considered in this study are important. Given their relative small proportion in this data, combined with the above facts, it might be beneficial if immigrants are studied on a separate investigation or authors seek a way of adding content about this group of patients in their discussions. Lastly, it is not clear what the distribution of the patients who died is. It would be interesting if majority are immigrants—this will be another reason why this group should be examined in much more detail.</p> <p>Overall It is true that there is increase in need for both specialist and primary care expertise to cover the broad range of care needs for this increasingly complex population. The study seems to have generated important addition to the literature. The authors need to address the issues about immigrants in the study as well as walk through the manuscript to work on minor errors/omissions to correct the grammar whole paper seem to have good evidence based material. It would have been much better if the limitations or weakness mentioned here as that would add much support to the errors if any in this study.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer 1:

1. The investigators apply an innovative and previously-validated care-typology approach to one of the most important questions in HIV healthcare: whether HIV care coordinated primarily by a primary care doctor or an HIV-specialist will promote better overall health outcomes. Despite the growing importance of this topic, it is a difficult one to study. This work represents a potentially valuable contribution to the literature because it is able to provide comparative data on this topic. An important strength of this study is that the investigators are able include almost all persons engaged in HIV care in the entire province of Ontario, capturing both major urban and rural settings.

We are pleased this reviewer views this as an important question for investigation.

Major Critiques:

2. Abstract Conclusions / Page 9 Text Box 1 / Discussion paras 1, 3, and final para:

The investigators seem to underestimate the likely impact of HIV disease stage to contribute to ART prescription outcomes, hospitalizations, and cancer prevention outcomes. It is highly likely that patients with advanced HIV stage (low CD4, active OI's, history of previous treatment failures and resistance) will be referred by primary doctors to specialists. Once prescribed ART by specialists and once engaged in treatment for OI's, they will likely return to the specialists for therapeutic monitoring. This would have the effect of raising the ratio of specialist to primary care visits, which is the defining point for primary care provider vs. specialist dominant care. Hence, ART prescription is likely to cause specialist dominant care rather than the opposite. Having a low CD4 is also likely to drive hospitalizations. Multiple studies of risk factors for hospitalizations have shown that low CD4 is the most important independent risk factor for both ADI and non-ADI hospitalizations [JAIDS 2012;59(4):368; AIDS 2008, 22:1345; JAIDS 2010;54:248]. Finally, having a lower CD4, and especially having active opportunistic infection(s), decreases the clinical urgency for cancer prevention. It is hard to send someone for a screening colonoscopy if he or she is battling disseminated MAI. There are several places in which the discussion of findings might be modified to account for this large limitation. First, it should be placed in the abstract rather than relegated to the 3rd bullet of the article summary. I would also suggest being more specific about it in the abstract and not simply referring to "residual confounding".

This reviewer certainly highlights the most important limitation of our study, and one we hope to mitigate by linking Public Health Laboratory data and clinical records in the future. While we fully agree with the reviewer's perspective on the potential for ART prescribing to arise in assignment to specialist care typology, we also envision a shift in HIV care in recent years in which people who are now stable on ART could transition to primary care, and those less stable on ART (due to social inequity or other) may bounce in and out of low threshold primary care sites. This probably adds an additional layer of complexity, one that might be easier to tease out once we can link to more rich clinical and social data, including means of HIV acquisition, that predispose an individual to one care model or another. Nevertheless, given the importance of his comment, we have:

a) included the following sentence in the conclusions section of the abstract:

"Our key limitation is the potential for confounding due to absence of HIV stage measures."

b) extended the third bullet point of the strengths and limitations on page 3 to read "Limited ability to capture HIV-specific morbidity may result in residual confounding arising from unmeasured disease stage indicators"

c) in the limitations section on page 22-23 (last para) we have changed:

"Increased HIV severity may have led to less focus on preventative care and resultant higher hospitalizations among those in exclusively specialist care."

to:

"While we used validated measures of comorbidity burden, there are measures of HIV-specific morbidity, such as CD4 counts and time since diagnosis, that are unmeasured in this study. Most importantly, increased HIV severity or advanced disease stage may have led to less focus on preventative care and resultant higher hospitalizations among those in exclusively specialist care (52)."

and have included the reference provided by the reviewer.

3. Second, the strength of conclusions in places like Abstract Conclusions and Discussion para 1,

should be decreased. Phrasing such as “had a strong influence” (Abstract) and “confirm on a population level...” (Pg 20 / line 36 and pg 23 / line 46) are too strong. Because of this limitation, the results are at best suggestive and hypothesis-forming. Conclusions should be phrased with this in mind and the 3rd para of Discussion (or another paragraph) should discuss how potentially important this limitation is and that low CD4 is likely to be a contributor to the observed outcomes of not just ART prescription but hospitalizations and cancer prevention as well. It would also be important to point out the potential reverse causality of ART prescription on specialist-dominant care.

To address these concerns, we have:

a) changed the indicated line in the abstract conclusion to read:

“We found how care is provided and shared between specialist and primary care physicians had an important influence on the quality of care delivered.” We elected to use the term ‘important’ rather than simply deleting the adjective because of the reviewers first statements – we do believe that the associations we found are important to care delivery, despite the limitations of our data.

b) changed the last sentence of the first paragraph of the discussion (page 20)from:

“However, it does confirm on a population level the need for both specialist and primary care expertise to cover the broad range of care needs for this increasingly complex population.”

to

“However, it does demonstrate on a population level the need for both specialist and primary care expertise to cover the broad range of care needs for this increasingly complex population.”

4. Methods / Results / Discussion: The data on diabetic eye disease screening are important in that they represent a contrast to the finding for cancer-prevention outcomes. The Methods section should describe this outcome. This outcome should probably be characterized as a primary-care type outcome. The text in Page 20 / line 26 lumps it with specialty-care, but since HIV specialists are not diabetes specialists, it seems better-placed with primary care outcomes. Since the findings on diabetic eye disease prevention contrast with those of cancer prevention, Discussion 1st para (page 20 / line19) and Discussion 2nd para should refer specifically to “cancer prevention” as being better performed under primary care dominant types of care and better highlight the fact there was no difference for diabetic eye screening.

The authors considered the inclusion of diabetes eye exams in advance of publication. While a primary care type outcome, we feel it would best be presented in a separate analysis under management of chronic conditions. First, this indicator relates less to the care provided specifically by the family physicians and HIV specialists in the typology models, which is the thrust of this particular manuscript. Second, it relies on ascertainment of diabetes, which relies in part on billing codes by these physicians, thus diabetes ascertainment itself may differ across models. To contextualize these issues, including diabetes eye exams as an outcome would substantially, but not substantively, extend the length of the methods, results and discussion sections. Our preference would be to remove this outcome and focus on clear primary care prevention outcomes, HIV-specific ART, and health care utilization, and leave comorbid condition care to another, more thorough analysis. We have modified Figure 2 accordingly. We hope the reviewer is comfortable with this approach.

3. Figure 2 / Discussion 1st para (pg 20 / line 14): It would be helpful to present the full multivariate regression results which underlie Figure 2 as a Supplemental Table (or series of Supplemental Tables) and to describe the main relevant points in the text. Without all multivariate OR's presented, there is no support for the statement on page 20 / line 14 that how care is shared between primary and specialist is “by far the most important” factor on the outcomes studied. How much more

important was it that number of visits, than comorbidity scores, than demographics, etc...?

This is a good suggestion. Rather than having an entire table per outcome, we have created a summary Table 3 (which could also be an appendix) of the adjusted results for each outcome. On page 19 of the results section, after reference to Figure 2 (adjusted model results for all outcomes are shown in Table 3 (or Appendix). The table can be found at the end of the document.

Minor Critiques:

1. Intro. Page 5 / Line 54: References should be given for the statement that people with chronic conditions who don't have a family physician are more likely to have emergency department visits and hospital admissions".

The reviewer is correct, and we have provided the following important reference for this statement: Glazier, R. H., Moineddin, R., Agha, M. M., Zagorski, B., Hall, R., Manuel, D. G., ... Kopp, A. (2008). The impact of not having a primary care physician among people with chronic conditions. ICES Investigative Report (pp. 1–30). Toronto: Institute for Clinical Evaluative Sciences.

2. Methods. Page 9 / Line 26: How were patients assigned who were contractually rostered to a family physician but never actually kept an appointment with the family physician while instead keeping appointments with a specialist?

Thanks for this question. Such patients would be assigned to Specialist dominant co-management. We thought it was important to identify that they were in fact "linked" to a primary care physician even if they had no visits to them, as this is important information to understand how patients actually use the healthcare system. They would have been assigned to Specialist dominant co-management rather than Family physician dominant co-management because 50+% of visits were conducted by the specialist physician rather than the family physician, thus appropriately reflecting that the HIV specialist is the main HIV care provider. We hope this is clear from our assignment algorithm in Figure 1.

3. Methods. Page 12 / Line 36: The N for those eligible for Ontario drug benefits might also be written here (it is appropriately given in Table 2) to help readers.

We have revised this sentence on page 12 to read: "Finally, one HIV-specific outcome, the receipt of any ART prescription among those eligible for Ontario drug benefits (n=8,302), was included."

4. Table 1: Number of kept visits to both primary care and specialists should be included in Table 1 since it is a determining factor for the care typologies and since it may be an important contributor to outcomes.

Thanks to the reviewer for this comment. We agree this information is important to contextualize the results. We have included the number of primary care visits to the usual family physician and the number of HIV specialist visits in the revised table.

5. Table 2 / Methods: How were HIV-specific hospital admissions determined?

HIV hospitalizations were defined as any hospitalization for which HIV was the primary diagnosis. We have clarified this in the methods section on page 12.

Reviewer 2:

6. The outcomes may not be relevant to all patients included in the study, immigrants may exhibit different patterns compared to Canadian-born patients. Presentation of the logistic regression results Fig 2 could be better if converted into a table and shown in numerical values to provide a better understanding of the OR magnitudes and differences.

Thanks to the reviewer for this comment. As per the suggestion of reviewer 2, we have provided all of the logistic regression results in the appendices, and have retained the summary Figure 2 for ease of reading.

7. Overview:

The authors present an important study that is relevant in the current era of HIV/AIDS. Medical advancements have made this disease to be widely managed as a chronic condition especially in the western world. Although this is encouraging, people living with HIV/AIDS are now faced with challenges associated with longevity of their lives with the disease. I think the information obtained from this study would be helpful to health care providers and systems as they prepare to deal with patients who live longer with HIV.

Thanks for this positive comment.

8. Abstract:

The Abstract is good. It gives the whole picture and what exactly was needed to be conveyed through this paper. All the objectives of the paper were properly included in the abstract, and it describes clearly what the article is set out to accomplish. However strengths and limitations of this study were included in the article summary. I don't know if this is the requirement of the journal. Otherwise, I would prefer to have this information placed elsewhere at the end of methods or in the discussion.

Thanks to the reviewer for these positive comments. As the strengths and limitations section is requested by the journal, I will leave this as is.

9. Introduction:

The introduction is simple and relevant. It gives a brief overview of issues leading to the objectives of the study. Relevant literature review was made regarding patients with chronic conditions who don't have a regular family physician. Authors used administrative data to measure care for several populations in Ontario; they presented models that included exclusively primary care, shared care with the family physician being the dominant HIV physician, shared care with the HIV specialist being the dominant HIV physician, exclusively specialist care, and low engagement. Overall, introduction helps you understand the key issue with proper background information. As this study is engaged with the experience of HIV patients who live longer with the disease. Some background information on the changes in longevity of life among these patients should have been included to give the reader an idea of the extent of longevity attained by patients living with HIV/AIDS.

To clarify the reviewer's point regarding longevity, we have added new reference 10 and revised the first sentence of the second paragraph of the introduction (page 5) to read:

“As people with HIV on combination antiretroviral therapy (ART) have life spans that approach those of HIV uninfected individuals(10), they are likely to acquire comorbidities related to aging as well as from the effects of HIV and its treatment (11–13).”

Reference 10 is: Wada N, Jacobson LP, Cohen M, French A, Phair J, Munoz A. Cause-specific mortality among HIV-infected individuals, by CD4+ cell count at HAART initiation, compared with HIV-uninfected individuals. *AIDS*. 2014;28(2):257–65.

10. Methods:

The data sources that were used in this cross sectional study were taken from the administrative databases at the Institute for Clinical Evaluative Sciences. Most relevant data has been gathered. But the authors have used many pieces of data to come up with their study group. I hope they were able to do an adequate job of tying together the patients without incurring any losses in the information used for analyses.

We are glad the reviewer is aware of the comprehensiveness of the ICES data. We are confident in the dataset linkages and have identified any places where individuals are lost due to attrition or missing data.

11. Outcome measures and Analysis:

The outcome measures were relevant for the study; they reflect the groups of conditions that have contributed to the volume of chronic disease in North America. Methods of analysis were also in line with the type of data and objectives of the study.

The only major issue I see here is the silence on the trends of care for immigrants. The authors indicated that this group may be challenged differently with the disease compared to Canadian-born patients. Other points to consider would be (1) these patients are coming from different healthcare systems; their pattern of use and healthcare seeking behaviors may not be clearly understood if assessed together with the natives; (2) these patients may be coming from environments where different types of chronic conditions other than those considered in this study are important. Given their relative small proportion in this data, combined with the above facts, it might be beneficial if immigrants are studied on a separate investigation or authors seek a way of adding content about this group of patients in their discussions. Lastly, it is not clear what the distribution of the patients who died is. It would be interesting if majority are immigrants—this will be another reason why this group should be examined in much more detail.

This reviewer is absolutely correct – immigrant populations living with HIV are an important subset of patients requiring focused attention, given their health and health seeking behavior differ from other populations. The data used in this study contains administratively-defined information on immigrants to Canada. We are collaborating with other researchers who can link clinical cohort data to these administrative data that will yield more rich information on self-defined ethnicity and immigration status. We also hope that our added Table 3 (or Appendix) will allow the reviewer to identify where we found associations between immigrant status and outcomes that warrant further investigation.

12. Overall

It is true that there is increase in need for both specialist and primary care expertise to cover the broad range of care needs for this increasingly complex population. The study seems to have generated important addition to the literature. The authors need to address the issues about immigrants in the study as well as walk through the manuscript to work on minor errors/omissions to correct the grammar whole paper seem to have good evidence based material. It would have been much better if the limitations or weakness mentioned here as that would add much support to the errors if any in this study.

Thank you again to this reviewer for their helpful feedback, especially in relation to further work required to delineate the care needs of immigrant populations.

VERSION 2 – REVIEW

REVIEWER	Stephen Berry Johns Hopkins University USA
REVIEW RETURNED	31-Mar-2015

GENERAL COMMENTS	<p>This study remains a laudable approach to an important question. The authors have understood and addressed each of my previous comments, and have improved the overall presentation of findings. I do feel however, that the presentation is still lacking in response to three previous concerns and could benefit from further changes.</p> <p>1. Appropriately characterizing the conclusions as suggestive rather than demonstrative or confirmatory. The potential impact of HIV disease stage as a confounder, and observational nature of this study make the latter two terms too strong. A) Abstract Conclusions, sentence which now reads, “We found how care is provided and shared between specialist and primary care physicians had an important influence on the quality of care delivered.” I would place “potentially” in front of “important”. In this context “important” refers to how strong is the effect of care type on the various outcomes. The effect might be quite small if the confounding from missing disease stage is large. B) First para of Discussion, sentence which now reads, “However, it does demonstrate on a population level the need for both specialist and primary care expertise to cover the broad range of care needs for this increasingly complex population.” In the version I downloaded, this sentence still includes “confirm” instead of the intended “demonstrate”. “Suggest” would be more appropriate than either confirm or demonstrate. The same phrase is repeated in the final paragraph of Discussion, and this should also be “suggest”.</p> <p>2. Diabetic eye screening data. The findings on diabetic eye screening from the initial submission were intriguing. Rather than space limitations, I think the compelling reason to omit these findings would be the second reason offered – inability to ensure accurate ascertainment of diabetes. As such, omitting the diabetes findings is reasonable. However, effort should be made to describe the cancer screening outcomes specifically as “cancer screening” throughout, rather than general “prevention” or “primary care” measures. This is important in order to be specific about this study’s actual scope and because even if diabetic eye screening data are not presented, the authors have reason (from them) to question whether the trend of effect of care type on cancer screening outcomes does, in fact, hold true for all or most types of primary-care related prevention. Following are some examples which stood out to me and some suggestions for re-phrasings. a) Abstract / Outcome Measures first word might be changed from “Prevention” to “Cancer screening”. b) Introduction last para, “primary care indicators” changed to “cancer screening measures as markers of primary care quality”, c) Table 2 row title, “Technical quality of primary care outcomes” to “Cancer screening outcomes”, c) Results, paragraph beginning “Figure 2...”, eliminate “prevention care maneuvers” and the parentheses surrounding the three actual cancer screening outcomes, d) Discussion first paragraph, “received better prevention” change to “received better cancer prevention”, e) Discussion second paragraph, “improved prevention” to “improved cancer prevention”.</p> <p>3. Potential for confounding by lack of data on HIV disease stage. I</p>
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	<p>think it should be plain in the manuscript – even to readers who skim it – that CD4 data (generally the most useful form of disease stage data) are lacking. In the third bullet point of the strengths and limitations on page 3, which now reads “Limited ability to capture HIV-specific morbidity may result in residual confounding arising from unmeasured disease stage indicators”. “Such as CD4 cell count” should be added after “indicators”.</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer comments:

1. This study remains a laudable approach to an important question. The authors have understood and addressed each of my previous comments, and have improved the overall presentation of findings. I do feel however, that the presentation is still lacking in response to three previous concerns and could benefit from further changes.

We thank this reviewer for his genuine interest in and attention to our study.

2. Appropriately characterizing the conclusions as suggestive rather than demonstrative or confirmatory. The potential impact of HIV disease stage as a confounder, and observational nature of this study make the latter two terms too strong.

2A) Abstract Conclusions, sentence which now reads, “We found how care is provided and shared between specialist and primary care physicians had an important influence on the quality of care delivered.” I would place “potentially” in front of “important”. In this context “important” refers to how strong is the effect of care type on the various outcomes. The effect might be quite small if the confounding from missing disease stage is large.

We have added the word “potentially” in the specified location as suggested.

2B) First para of Discussion, sentence which now reads, “However, it does demonstrate on a population level the need for both specialist and primary care expertise to cover the broad range of care needs for this increasingly complex population.” In the version I downloaded, this sentence still includes “confirm” instead of the intended “demonstrate”. “Suggest” would be more appropriate than either confirm or demonstrate. The same phrase is repeated in the final paragraph of Discussion, and this should also be “suggest”.

We have changed “demonstrate” or “confirm” to “suggest” in both places within the manuscript as suggested above.

3. Diabetic eye screening data. The findings on diabetic eye screening from the initial submission were intriguing. Rather than space limitations, I think the compelling reason to omit these findings would be the second reason offered – inability to ensure accurate ascertainment of diabetes. As such, omitting the diabetes findings is reasonable. However, effort should be made to describe the cancer screening outcomes specifically as “cancer screening” throughout, rather than general “prevention” or “primary care” measures. This is important in order to be specific about this study’s actual scope and because even if diabetic eye screening data are not presented, the authors have reason (from them) to question whether the trend of effect of care type on cancer screening outcomes does, in fact, hold true for all or most types of primary-care related prevention. Following are some examples which stood out to me and some suggestions for re-phrasings. a) Abstract / Outcome Measures first word might be changed from “Prevention” to “Cancer screening”. b) Introduction last para, “primary care

indicators” changed to “cancer screening measures as markers of primary care quality”, c) Table 2 row title, “Technical quality of primary care outcomes” to “Cancer screening outcomes”, c) Results, paragraph beginning “Figure 2...”, eliminate “prevention care maneuvers” and the parentheses surrounding the three actual cancer screening outcomes, d) Discussion first paragraph, “received better prevention” change to “received better cancer prevention”, e) Discussion second paragraph, “improved prevention” to “improved cancer prevention”.

This is a very helpful suggestion. We have addressed the areas specified, as well as throughout the paper, to reflect the outcomes as “cancer screening” rather than “prevention”.

4. Potential for confounding by lack of data on HIV disease stage. I think it should be plain in the manuscript – even to readers who skim it – that CD4 data (generally the most useful form of disease stage data) are lacking. In the third bullet point of the strengths and limitations on page 3, which now reads “Limited ability to capture HIV-specific morbidity may result in residual confounding arising from unmeasured disease stage indicators”. “Such as CD4 cell count” should be added after “indicators”.

We have made this revision to include CD4 cell counts specifically within the bullet points, and have ensured this is also reflected in the discussion section of the manuscript (no revision required).