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Title	Relationship between primary care physician supply and diabetes care and outcomes: a cross-sectional study
Authors	Tara Kiran MD MSc, Richard H. Glazier MD MPH, Michael A. Campitelli, MPH, Andrew Calzavara MSc, Therese A. Stukel PhD
Reviewer 1	Dr. Lee Green
Institution	University of Alberta, Edmonton, Alta.
General comments and author response	Reviewer 1, Comment 1 The basic literature is addressed adequately. The evidence of the tenuous relationship between some of the guideline-recommended DM care (e.g., A1c control, lipid measurement and management to targets vs "fire and forget") and patient outcomes is not noted, however. Response We thank the reviewer for raising this interesting issue. The CDA 2013 guidelines state that “glycated hemoglobin (A1C) levels >7.0% are associated with a significantly increased risk of both microvascular and macrovascular complications, regardless of underlying treatment”, citing three landmark studies. The guidelines also cite literature demonstrating the benefits of treating dyslipidemia in patients with diabetes. However, we agree with the reviewer that there is a questionable relationship between monitoring of hemoglobin A1C and achievement of hemoglobin A1C targets and have added this caveat to our limitations section. Reviewer 1, Comment 2 The statement that the networks include all physicians who contribute to the care of the majority of the population immediately prompts the question of the minority of the population, and what impact that group (almost certainly different in significant ways) would have on the outcome measures. Later, in Methods, it turns out that only 1.02% of patients were not attributed to a network. This might be clarified earlier. Response: We thank the reviewer for this insightful comment. We have revised this sentence to read “[networks] include the primary care physicians who contribute most of the care for the assigned population, regardless of geographic borders, and were highly self-contained. ” This statement is more accurate. As the reviewer notes, the methods section provides further details about the physicians and patients in the networks. We address the issue of loyalty as noted in the response to comment 3. Reviewer 1, Comment 3 According to the referenced article, network loyalty for primary care is 81%, so even in network a significant share of care was received outside. The potential impact of that gap is not discussed. Response We thank the reviewer for pointing out this potential limitation in our study. While median network PC loyalty was 81%, the 90th percentile loyalty was 91% and loyalty was generally higher for non-urban networks, so that most primary care was received from primary care physicians within the network. Furthermore, we captured all care received by the network population regardless of where it was received. The effect of receiving a small proportion of care outside the network would serve to slightly dilute the effect of network supply and we have noted this limitation in the discussion section. Reviewer 1, Comment 4: The exclusion of patients (almost 5% of the sample) not surviving the April 2009-March 2011 interval excludes an important outcome of diabetes. No justification is presented for this exclusion. Response: We thank the reviewer for highlighting that we were not clear enough about our methodology in the manuscript. We excluded patients who died between April 2009 and March 2011 only when examining the outcome of optimal monitoring as testing would not be appropriate for those who died in the study timeframe. However, we did not exclude patients who died when assessing hospitalizations and emergency department

	visit outcomes. We have revised the relevant paragraph in the methods to make this distinction clearer. Reviewer 1, Comment 5: The definition of the model variables involves quite a few judgment calls and assumptions. These appear to be well considered, and the choices as good as can be made within the limitations of the data available. Response We appreciate the reviewer's positive feedback Reviewer 1, Comment 6: As is noted in comments about the Introduction, there is doubt about the 2008 guidelines used. 5 of the 6 optimal measures (4 A1c and 1 lipid) may not actually have much to do with outcomes. The paper would be markedly stronger if the authors were able to include prescribing patterns, particularly prescriptions for metformin and statins. As blood pressure control is the single most important (lowest NNT) process measure for DM, the paper would be stronger if the authors had access to at least some proxy measure for HTN control. These may be very difficult to obtain, however. Response: We agree with the reviewer that it would have been ideal to include prescribing patterns or a measure related to blood pressure control. Unfortunately we are limited by what data is available in our administrative databases. Our databases do not include complete data on prescribing or any measure of blood pressure. We have expanded on these limitations in the discussion. Reviewer 1, Comment 7: The use of neighborhood income introduces a degree of ecological fallacy but is a reasonable choice for this type of research. Response: As the reviewer suggests, we do not have access to individual income data but studies have shown that neighborhood income levels are a reasonable proxy for individual income. We are glad that the reviewer finds our choice of neighbourhood income reasonable. Reviewer 1, Comment 8: Poisson regression as implemented here is a good choice, except for hospital admissions. Those have a significant zero-incidence issue, and a two-stage model (e.g., a probit analysis for any admission first, and the probit estimate rather than actual admissions used in the regression) might be more appropriate. Response We thank the reviewer for the thoughtful critique of our statistical approach. We realize we might not have been clear enough in the description of our methods. Although we used Poisson regression, we did not model count data. We modeled binary data (e.g. one or more ED visits vs. none). We used Poisson regression with a robust variance estimator to calculate the relative risk of outcome rather than the odds ratio of outcome since odds ratios only approximate relative risks when the outcome is rare (<5%). In our cohort study, we knew the optimal monitoring outcome would be very common (~35-40%) so we knew any odds ratios calculated would grossly overestimate the risk of the outcome leading to problems with interpretation. The reviewer suggests a two-stage model. Our hypotheses were not focused on the implications of such a two-stage model, i.e. characteristics of patients who were ever vs never hospitalized, and characteristics of patients who had multiple hospitalizations among those who had at least one admission. We have revised our analysis section to clarify our rationale for using Poisson regression. Reviewer 1, Comment 9: There is not a great difference in physician FTE between the low and high supply networks. That may result in a range compression problem, and hence lead to a small observed correlation. Indeed, the effect size is quite small, but larger among the rural networks where the range of supply is greater.

	Response We agree with the reviewer's astute observation and have added a sentence to this effect in the limitation section of the discussion. Reviewer 1, Comment 10: There is no mention of correction for multiple comparisons (though the p values are strong enough that it is unlikely to make a difference). Response We did not correct for multiple comparisons as we analyzed a small number of hypotheses, all of which were based on clinical intuition. Reviewer 1, Comment 11: In general the discussion is useful. The acknowledgement of limitations should address the weakness of the evidence for the process measures, however, and in particular the lack of access to prescription fills. They also do not address the possible bias introduced by omitting death during the study period as an outcome. Response: We thank the reviewer for these suggestions. We have revised the limitations section to address the weakness of the evidence for the process measures and the lack of access to complete prescription data. As the reviewer mentions, we did not include death as a potential outcome in the examination of hospital visits (but did exclude those who died from assessment of the optimal monitoring outcome). Death was extremely rare in this cohort so it would have been difficult to find differences among groups. If high supply networks delivered better care and had lower death rates, patients in those networks would have had more opportunity for ED visits and hospitalizations, which may have biased our findings towards the null. We have noted this limitation in the discussion.
Reviewer 2	Prof. Rory Tekanoff
Institution	Urban Care Health Group, Director, Community Medical Programs, London, Ont.
General comments and author response	Reviewer 2, Summary Preamble Overall, this paper is well constructed in the reporting of the objectives, methodology, results, conclusions and corresponding interpretation of the results. My first impression as a peer reviewer is that the article is well written and does provide adequate information for readers to conclude that patients with ready access to primary care physician networks results in a more consistent administration of evidence based testing than for diabetes patients who do not have access to health care teams or are located in rural or non-urban areas. The objectives of the study have been met. Other papers related to similar study have been published and serve to validate the conclusions of the paper submitted. Example below. "Performance of Primary Care Physicians and Other Providers on Key Process Measures in the Treatment of Diabetes Fritha Morrison, MPH1, Maria Shubina, SCD1, Saveli I. Goldberg, PHD2 and Alexander Turchin, MD, MS1,3,4Ĩ Diabetes Care, American Diabetes Association, 2015" Objectively, in this study and its report, I applaud the use of a large sample size, a well constructed, well conceived methodology utilizing model count variables such as Poisson regression and GEE models, strict inclusion and exclusion criterium, etc., coupled with the key objective of supporting an accepted theorem amongst the medical community that network/team approaches to disease management results in the betterment of patient monitoring (there is no mention of health outcomes in this paper), it is my conclusion that although this study COULD be of use to the Canadian Healthcare Professional in their practice of managing diabetes, however the pathways to the author's conclusions include gaps which are not well explained in the article and could raise questions which may affect the relevance of the study information as well as the age of the study. Response: We thank the reviewer for these positive comments. We have done our best to address any concerns below.

	Reviewer 1, Comment 1: A major caveat for this article is the study time period and will undoubtably raise questions as to whether in fact that practitioners in general were not working within the 2008 CDA recommendations. The baseline for appropriate monitoring were the 2008 CDA Guidelines. In this study, patients were included from the years 2007-2009. This begs the question then, "would only half of the study group be optimized because of the introduction of the CDA guidelines midway through the patient study period?" The authors do not confront this issue which presents a serious question as to whether or not physicians were using the recommendations for the entire study group. If not, this article has little credibility. Response: We thank the reviewer for raising this important issue. We have confirmed that the 2003 CDA guidelines have similar recommendations for hemoglobin A1C measurement (recommendation to perform approximately every 3 months); lipid measurement (recommendation to perform approximately every 1-3 years); and retinal screening (recommendation to perform approximately every 1-2 years). We have revised our methodology to clarify that the parameters we used to measure optimal monitoring were relevant for both the 2003 and 2008 CDA guidelines. Reviewer 1, Comment 2: It could be construed that optimal testing was less likely to occur in non-urban settings as a result lack of knowledge. This was a result of an inconsistent access to education for practitioners within these geographical areas. Medical education programs are more frequent, have a greater diversity of topic information, and include "bigger name" specialist presenters when held in urban settings with a high concentration of potential attendees. This was the case back in 2008 and still remains an issue today. Pharmaceutical companies in particular, focus on urban settings more frequently than non-urban or rural settings for promotion of products, guideline education, and disease management practices. Simply, it is related to return on investment, and in the case of physicians being educated on the 2008 CDA guidelines, knowledge gaps may have existed in the non-urban physician, consequently leading to the reported result of simply not ordering the optimal number of tests, not due to the fact there are fewer health teams, but due to lack of knowledge. If the authors are using the 2008 CDA guidelines as the yardstick for optimal care as the baseline in their study, education of the guidelines must be taken into consideration and again, this was not broached by the authors in this paper. It has been shown that a direct influence of skill and knowledge acquisition has a direct relationship on the betterment of patient outcomes. Reference below. 'Continuing medical education-driven skills acquisition and impact on improved patient outcomes in family practice setting. Bellamy N1, Goldstein LD, Tekanoff RA; Support, Non-U.S.Gov't. J Contin Educ Health Prof. 2000 Winter;20(1):52-61 As a result I do see some serious limitations related to publishing this study in a future edition of CMAJ Open and I would caution the editors on some additional important things to note. Response: The reviewer elegantly describes some of the challenges with the delivery of Continuing Medical Education (CME) and its potential impact on quality of chronic disease care. Our study did not set out to compare the rates of optimal monitoring in urban and non-urban areas. However, our findings in this regard are contrary to the reviewer's hypothesis. Crude rates of optimal monitoring were higher overall in non-urban areas compared to urban areas. We acknowledged a priori that there may be some differences in delivery of primary care between urban and non-urban areas including the differences relating to access to CME that the reviewer describes. Because of these potential differences, we stratified our analysis by urban and non-urban networks and compared outcomes in high, medium, and low primary care supply networks within each stratum. Reviewer 2, Comment 3: The study was completed in 2011, almost 4 years ago utilizing an older set of Canadian

	Diabetes guidelines on frequency of monitoring. Since the study was completed, in 2013, the Canadian Diabetes association has issued a revised and more comprehensive set of guidelines; a key change in recommended monitoring of HbA1C testing to in fact be doubled compared to the optimal monitoring measured within this study. The information in this article is dated. Could this information be extrapolated to support a more rigorous monitoring recommendation by the CDA standards of today? Would physicians/HCPs in fact meet the same level of monitoring if the same study was conducted with patients from 2013-15? Only a more timely study can answer that question, using baseline measurements which represent optimal practice at the current time. Response: We thank the reviewer for raising this concern. We acknowledge that our study was completed approximately 4 years ago. However, we believe the overall findings are still relevant and that our process measures are consistent with optimal practice at the current time. Both the 2008 and 2013 CDA guidelines recommend hemoglobin A1C testing every 3 months when glycemic targets are not being met. However, both guidelines state that “testing at 6-month intervals may be considered in situations where glycemic targets are consistently achieved”. In our study, we set a conservative target that all diabetes patients should have a hemoglobin A1C test at least once every 6 months, which translates to a minimum of four tests per year. We believe this process measure and the measures related to cholesterol and retinopathy screening will still resonate with clinicians today. Reviewer 2, Comment 4: The most important gap in this study is the absence of any patient outcome information. As healthcare professionals, one can infer that if intensive monitoring or follow-up is undertaken, patient outcomes should be affected positively. We know that in spite of having significant amounts of information, the translation of such to concretely show that patient outcomes are enhanced is sorely lacking, particularly in patients with diabetes, hypertension and cholesterol issues. In 2013 an article in the Canadian Journal of Diabetes illustrates the glaring problem and dismal proportions that primary care physicians have in getting hyperglycemic/hypertensive/dyslipidemic patients to target. "Type 2 Diabetes Mellitus Management in Canada: Is It Improving? Lawrence A. Leiter MDa,d,e,*, Lori Berard RNf, C. Keith Bowering MDg, Alice Y. Cheng MDa,d, Keith G. Dawson MD h, Jean-Marie Ekoé MD i, Carl Fournier MD j, Lianne Goldin k, Stewart B. Harris MD, MPH l, Peter Lin MD k, Thomas Ransom MD, MSc m, Mary Tan MSc k, Hwee Teoh PhD a, b, Ross T. Tsuyuki PharmD, MSc n, Dana Whitham RD, MSc o, Vincent Woo MD p, Jean-François Yale MDq, Anatoly Langer MD, MScc. Reprinted from Canadian Journal of Diabetes - April 2013 (Vol. 37, Issue 2, Pages 82-89), with permission from Elsevier. http://www.sciencedirect.com/science/article/pii/S1499267113001329 As a result of more timely study into direct influence on patient outcomes, this article submitted although elegant in study design, may not fit the more current need for physicians to understand that patients with diabetes simply are not being treated optimally, and this poses then a greater question; if monitoring is at the high levels in urban areas that the authors report, why is treatment not meeting the patient need? Response: We agree with the reviewer that there is a tenuous link between monitoring and optimal patient outcomes. Unfortunately, our administrative data does not include data on hemoglobin A1C values, blood pressure values or other intermediate outcomes. We have revised our discussion section to expand on these limitations. However, theoretically, monitoring is a necessary pre-requisite to achieving optimal intermediate outcomes. Further, our study does not only assess adherence to evidence-based testing but also looks at emergency department visits and hospitalizations, two important outcomes for patients with diabetes