

Reviewers' comments and author response

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Title	Correlation of primary care bonus payments with patient-reported access in urban Ontario: A cross-sectional study
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Reviewer 1	Dr. N. Nante, Università di Siena
Reviewer comments and author response in bold	<p>1. The introduction provides a good overview of Ontario's health situation, useful for non-Canadians. Considering the patients' experience gives a good perspective on primary health care in Ontario.</p> <p>We are pleased to hear that the overview is helpful, especially to an international audience.</p>
	<p>2. The title explains the content of the paper, but perhaps is too long.</p> <p>Thank you for this observation. We have received additional guidance from the Editor and our new title is now: "Correlation of primary care bonus payments with patient-reported access in urban Ontario: A cross-sectional study"</p>
	<p>3. On line 13 of page 6, the abbreviation ED appears but I did not see the extended form before.</p> <p>Thank you for noticing this. We have now expanded the term "ED" prior to using the acronym.</p>
	<p>4. On lines 13-14 of page 8 in "Methods," there are some wrong brackets.</p> <p>We checked the prose and did not identify the wrong brackets to which the reviewer is referring.</p>
	<p>5. To support the "variable definitions" I would place a map to better explain the geographical distribution of "RIO".</p> <p>We agree that more detail about the RIO classifications would be helpful for the reader. We have now added a more in-depth explanation about the RIO system to our Methods section (subsection "Variable definitions") and have referenced Kraij's RIO methodology paper (reference #20), which includes maps.</p>
	<p>6. In the paragraph "covariates" I would add a table to better explain the elements considered.</p> <p>Thank you for this suggestion. We have added to the manuscript a statement that further elaboration of these covariates are reported in the results Tables 2-4, but are happy to provide new tables separate from results to address the reviewer's concern. We will leave this to the discretion of the editors.</p>

	<p>7. In line 37 of page 17 write, “these results cannot confirm causality”. Could you explain it better? To have causality some criteria must be met, some are reserved for biological events but others could be applied for this condition.</p> <p>We agree with the reviewer’s suggestion. We find that while our results meet some criteria for establishing causality, such as a dose-response</p>
	<p>relationship and consistency, it does not meet others, most importantly temporality.</p> <p>We have now amended the wording in our manuscript to better explain our interpretation: “While a cross-sectional study cannot confirm causality, the dose-response seen for three of the four outcomes (i.e., the higher the Access Bonus, the higher the odds ratio for favourable Telephone Access, After-Hours Access, and Wait Time) provides support that the relationship may be causal.” We have also added Cochran-Mantel-Haenzel trend testing to our analyses to lend additional support for these dose-response observations.</p> <p>There was another consistency in our findings in that we found no significant relationships in the smaller centres across all four outcomes and positive relationships in the larger settings, which we have indicated in our “Interpretation” section as well.</p> <p>We have added to the “Limitations” section a statement to acknowledge that our study does not meet the criterion of temporality: “It is possible that better access was a pre-existing feature of large urban practices that opted to join the blended capitation model.”</p>
	<p>7. In the “Results” from line 24 to line 37 of page 15, there is the list of quintiles. A bullet list could be considered to improve clarity.</p> <p>We would be happy to make this formatting change if acceptable to the journal editors.</p>
	<p>8. Figures 2 could be merged into a single graph.</p> <p>We considered this idea earlier in the manuscript preparation and the team found that this would be difficult for an unfamiliar reader to understand. Therefore, respectfully, we have chosen to leave the figures in the current format.</p>
	<p>9. All the tables need editing improvements.</p> <p>We have now made edits to the formatting of Tables 1, 3 and 4 to ensure consistency of the visual presentation of each cell. We have also edited the reporting of percentages in accordance with the journal’s request to use one decimal place rather than two.</p> <p>We did not include additional formatting because our understanding is that the journal will format the published table in accordance with their own formatting style.</p>

	<p>10. It would be better to divide the data presented in Table 4 in two sections: i) into dichotomous variables and ii) continuous ones; the latter needs also descriptive information such as the Standard Deviation. Please add the % symbol to the table title.</p>
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	<p>Thank you for this suggestion. We agree and have now grouped together the continuous variables and the dichotomous variables within Table 4. Standard deviations are reported. The % symbol is included where needed in the table subheadings along the first column.</p>
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	<p>11. It is confusing whether Table 5 has a multivariate analysis. It seems you have conducted a univariate analysis by different exposition level with the outcome. I would expect univariate analysis and bivariate analysis, and for those variables which emerged to be significant at the bivariate analysis having a multivariate model.</p> <p>Our analysis proceeded as follows:</p> <p>1) We conducted a descriptive analysis to describe our sample within each geography (Tables 1, 3, and 4)</p> <p>2) A univariate analysis (Figure 3) was conducted where we determined the relationship between each outcome variable and the Access Bonus (note: our statistician used the term “univariate” but this could also be called an “unadjusted bivariate analysis”)</p> <p>3) We conducted a multivariable regression where we determined the relationship between each outcome variable and the Access Bonus and other covariates (Tables 5a-d (originally entitled Multivariate Analysis, now entitled “Multivariable Analysis” in accordance with the journal’s statistical terminology).</p> <p>To make Tables 5a-d more easily apprehensible to the reader, we could limit it to presenting the multivariable analysis for only the main exposure (Access Bonus achievement) and move the multivariable analyses for the remaining covariates to a Supplemental Table. We will take direction from the Editor on this idea.</p> <p>With respect to the use of the term “univariate,” which we appreciate may be confusing, we have now added more description to Figure 3 to clarify that this Figure depicts the results of an unadjusted analysis.</p> <p>With respect to model-building, we did not use the significance found in the univariate analysis to determine which covariates were entered into the multivariate model. Given that our main interest was the effect of the Access Bonus, and given that we had sufficient sample size, we chose to adjust the multivariable model for all covariates.</p> <p>We hope these clarifications and changes address the reviewer’s concerns and make the results easier for the reader to interpret.</p>
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	<p>12. These of my studies could be cited to implement background and discussion:</p> <ul style="list-style-type: none"> • Golinelli, D., Toscano, F., Bucci, A., Lenzi, J., Fantini, M. P., Nante, N., & Messina, G. (2017). Health Expenditure and All-Cause Mortality in the ‘Galaxy’ of Italian Regional Healthcare Systems: A 15-Year Panel Data Analysis. <i>Applied Health Economics and Health Policy</i>, 15(6), 773–783. https://doi.org/10.1007/s40258-017-0342-x
	<ul style="list-style-type: none"> • C. Quercioli, F. Nisticò, G. Troiano, M. Maccari, G. Messina, M. Barducci, G. Carriero, D. Golinelli, N. Nante. Developing a new predictor of health expenditure: preliminary results from a primary healthcare setting. <i>Public Health</i>, Volume 163, 2018, Pages 121-127, ISSN 0033-3506, https://doi.org/10.1016/j.puhe.2018.07.007. <p>Thank you for bringing these papers to our attention. We have reviewed them, and while finding them interesting with respect to the context of health care expenditures, we were unsure how best to implement them in our Background and Discussion concerning the effect of an Access Bonus on access to care.</p>
Reviewer 2	Dr. Richard Lewanczuk, University of Alberta
Reviewer comments and author response in bold	<p>This study would be of both great importance and great interest to both the medical community and to health system administrators.</p> <p>We appreciate the reviewer’s interest in this study and acknowledgement of its relevance to the current health policy environment.</p>
	<p>Unfortunately, the way in which the results are presented and described is extremely confusing. Despite being very familiar with the field, I am not sure I completely understand the results. I think this data should be published but the authors need to be much more clear in the presentation and interpretation of the data.</p> <p>We appreciate the reviewer’s feedback around clarity and have made a number of changes to address the reviewer’s concerns.</p> <p>Because of the confusing way in which the data is presented it is somewhat difficult to provide explicit comment. My best efforts are as follows:</p> <p>1. The authors need to be very clear on the exact definition of principle variable. I interpret this as the proportion of physicians achieving the maximum bonus.</p> <p>The variable “Access Bonus achievement” was calculated as the proportion of the maximum potential bonus that was achieved by the physician in the fiscal year closest to the HCES interview date.</p> <p>We have now added the following example to our manuscript to make this calculation clearer to the reader: “For example, if a physician was eligible for a maximum bonus of \$10,000 and achieved \$5000, the bonus achievement for that physician was calculated as 50%”</p>

	<p>This approach to defining Access Bonus achievement (i.e., as a proportion of the maximum potential bonus) was also used in our comparator paper, Glazier 2019 (reference #12), and we wished to be consistent in this paper.</p>
	<p>2. Based on the narrative description the figures are very confusing. The authors state that small urban practices achieved the highest bonuses (or is it a greater proportion achieved maximum bonuses? - I can't tell) yet the height of the bars in the quintiles in Figures 3 and 4 all show a greater odds ratio for large and medium sized practices. Am I confusing absolute numbers with proportions? In any case, this is very confusing to the reader.</p> <p>We have attempted to make the variable definition for “Access Bonus achievement” clearer by adding an example of the calculation in the “Methods” section.</p> <p>In our descriptive analysis, physicians practicing in smaller settings were observed to have achieved a larger proportion of their maximum potential Access Bonus when compared with physicians practicing in larger settings. We have amended the title of Tables 1, 3, and 4 to make clear that these are descriptive analyses, and in our “Results” prose we have specified: “Tables 1,3, and 4 report the descriptive analysis of patient, physician, and practice characteristics.” We have also added wording in our prose to make clearer that the subsequent paragraph reports the results of a univariate regression analysis and the paragraph after that reports a multivariate regression analysis.</p> <p>Figures 3 and 4 depict the regression analyses. The regression analyses presented in Figure 3 are unadjusted (i.e., univariate) while the analyses presented in Figure 4 are adjusted. We have expanded the titles of the figures to make these distinctions more clear. We have also added a note to Figure 3 and 4 to explain that the odds ratios in these figures refer to the odds of the patient reporting favourable access within each physician bonus quintile and within each geography.</p>
	<p>3. In continuation of the above theme, it would be helpful if the tables and figures had more explanatory titles or legends to help the reader understand what data was being presented.</p> <p>We agree and have amended the titles of our tables and figures to make clearer what is being presented, along with additional explanations as described in our response to comment #2 above.</p>
	<p>4. I agree with the speculation that larger urban practices, where there are more access alternatives such as walk-in clinics, must provide better access options from both a business perspective and to ensure continuity of care is maintained. In smaller urban (and rural) areas where there are fewer choices, patients are obliged to see their primary care provider and may be more accepting of both the wait time and timeliness, although I do note that the definition of wait time is based on an absolute number. On the other hand, the experience in my province is that in rural and small urban areas, the ER is staffed by primary care physicians and is used as an extension of their office practice. Thus, bonuses (or negation) based on the individual physician are not affected, whereas if based on site of service delivery, there is an impact. A discussion around these concepts would be of interest.</p> <p>We fully agree that these distinctions in the delivery of health services in rural vs urban settings play a significant role in driving “outside use,”</p>

	<p>especially given the way the Access Bonus is designed. We have added a statement to the “Background” to make more explicit that ED use is not negated in the Access Bonus structure. We have furthermore elaborated on a statement in our “Interpretation” section in reference to this concept: “This suggests that this incentive may not be effective in geographies where competition is minimal, patient choice is in turn limited, and EDs (often staffed by family physicians) are the primary setting for time-sensitive care.”</p>
	<p>5. A further point of interest, in terms of discussion, is based on my presumption that small urban practices receive higher, or a greater proportion, of the maximum bonus, presumably due to the authors speculation around limited option. Research carried out by our group has shown that lack of continuity of care - or in this circumstance, a decrease in the amount of the bonus - is not related to physician or practice factors, but is related to patient factors, namely convenience. Our data demonstrates that the majority of visits outside the practice in large urban areas occurs when the practice is open and when the primary care physician would have agreed to see the patient. In other words, because of (likely geographic) convenience, the patient chose to use a walk-in clinic (in most cases). Thus, physician-based incentives or dis-incentives are of limited value and perhaps the patient disincentives or competing practice disincentives would be more effective. Based on the comments in the Introduction, some discussion around this aspect (physician/practice vs patient factors affecting continuity) would be interesting, if the data is supportive.</p> <p>We agree and our ICCs certainly align with your observations. We have now added the following statement to our Interpretation section: “We furthermore note that based on our ICCs, patient factors are more influential than physician or practice factors, which is consistent with prior research finding that physician incentives may be of limited utility.”</p> <p>We have added references from the broader pay-for-performance literature to this statement. We agree that the work of your group, as described in your comment, would also be highly relevant here. We would therefore like to cite your group’s work but we were unable to locate this in our own search. Would you kindly point us toward these references?</p>
	<p>6. A better discussion of Next Steps or follow-up research would be appreciated as this is an area of interest and we only speculate on physician and patient behavior based on indirect data versus e.g. patient surveys or interviews.</p> <p>We agree and have included a statement in our Interpretation section that future “Longitudinal and qualitative research would help better understand the nature and direction of causal relationships.” We have also added an example of future work we see as an important next step: “For example, a qualitative study exploring organizational factors may help us understand the differences in bonus achievement and patient-reported access between otherwise similar practices, and could provide guidance for practices wishing to improve their delivery of services.”</p>
	<p>7. Some overall comment on the multivariate regression would be of use. Do the authors think that practice, physician or patient factors determine achievement of the bonus? Is achievement of the bonus a surrogate for continuity of care? If so, can comment be made on factors affecting continuity? Is continuity measured in Ontario and can that be compared to achievement of the bonus, thereby allowing the data to be interpreted in the context of continuity of care?</p>

	<p>We share the Reviewer’s perspective and interest in continuity of care. Given that the variation in patient-reported access was determined mostly at the patient level rather than at the physician or practice level, it is reasonable to conclude that patient factors also relate to continuity insofar as patients are driving, by choice, access to primary care outside of their regular provider. We agree that the Access Bonus is a surrogate for continuity of care as it is constructed in Ontario. We have added a statement to our Interpretation that: “These findings raise interesting questions around the relationship between access-oriented incentives and continuity of care.”</p>
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