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Title: Trends in attachment to a primary care provider in Ontario 2008–2018: an interrupted time-series analysis

Authors: Imaan Bayoumi MD MSc, Richard H. Glazier MD MPH, Liisa Jaakkimainen MD MSc, Kamila Premji MD, Tara Kiran MD MSc, Eliot Frymire MA, Shahriar Khan MSc, Michael E. Green MD MPH

Reviewer 1: Dr. Raisa Deber

Institution: University of Toronto

General comments (author response in bold)

Comments to the Author

This is a very interesting study making excellent use of ICES data. It has some major policy implications, but it might benefit from a bit of clarification.

There are a number of key points mentioned in the abstract, but they are not all discussed in detail in the paper (e.g., there is little about rurality). It might be helpful to ensure that the key points made in the abstract are noted in the paper?

We have added a summary statement regarding rurality.

“Urban dwelling Ontarians had higher odds of attachment compared with rural Ontarians.”

As a minor point, they use the term aOR frequently in presenting their results, but do not define it.

The definition of aOR has been added.

As another minor point - is it possible that some of the young adults without clear primary care providers are temporarily living outside the province (e.g., students studying in other localities)? Would the data allow that to be noted?

Young adults living outside Ontario – added as another limitation.

There is also an interesting contrast in findings – the introduction states that people with co-morbidities are more likely to be unattached, but their findings seem to contradict that and find that sicker people are more likely to have a defined provider. It is also not clear how that is compatible with the finding that a substantial proportion of uncertainly attached people had frequent contact with the health care system?

Thoughts here? Suggestions on how we address this succinctly, given word count constraints?

It also might be worth a few more sentences about whether the results are likely to have changed with COVID-19?

But congratulations on a very important piece of work.

A future analysis will examine changes in attachment during COVID. We have added a sentence calling for this analysis. We have also discussed some HHR trends that may impact attachment during COVID.

Reviewer 2: Richard Lewanczuk

Institution: Endocrinology, University of Alberta

General comments (author response in bold)

This paper describes longitudinal changes in attachment to primary care in the province of Ontario, Canada from 2008-2018. It further segregates changes in attachment by variables known or expected to influence the rate.

Overall, the general results are consistent with what is already known concerning factors which influence attachment, although in many instances these are not explicitly pointed out or explained. For example, it is well known that young males have low rates of attachment because they perceive no need to have a regular primary care provider. Some of the results are likely specific to Ontario and hence context is very important in this regard. Similarly, some factors which influence attachment (and hence continuity of care) are common across groups, such as access (see Cook et al *CMAJ Open* 16; 8:E722, 2020). Other factors differ between groups such as the availability of alternatives to care in urban vs rural areas.

While the data presented may be contributory to medical knowledge, I find the authors have taken great liberties in assigning causation to association. The area of attachment is very complex, definitely influenced by policy, but also subject to social considerations. For example, reduced attachment and continuity with higher deprivation is largely influenced by the density of primary care providers (typically lower in these communities and hence access is an issue) but also by competing priorities of individuals, whereby care is only sought for acute conditions from the most convenient source at the time, and preventative care, or routine care of chronic diseases, is subordinated to those other competing priorities, such as providing for food and housing.

We have added text noting that we are reporting associations, not attributing causation, as noted above.

In addition, we agree that some unattached patients may not be actively seeking care or may have competing demands which limit them from seeking a regular site of primary care. We cannot determine patients' motivation or actions taken to find care using administrative data. We have noted this as a limitation.

3. An implied hypothesis in this paper seems to be that policy changes in 2014 negatively affected attachment. If this is the case, that hypothesis should be explicitly stated rather than being more casually mentioned in the Introduction. Following from this, the study and its presentation should have included whatever data might be available to support or address this hypothesis. For example – and perhaps the authors did this – were there other policies or practices which may have had an influence in addition to the policy on alternative payments?

Strong evidence would be a comparison to longitudinal attachment rates in other provinces, if available.

We have stated the hypothesis more clearly in the final sentence of the Introduction.

4. Personally, I would find the Context section better presented as a paragraph in the Introduction so that the reader can judge the applicability of the results. However, I defer to the editor. But, in line 50 of page 1, the context is confusing. Were there multiple interventions in different areas or groups? For example, did one area or group of physicians have blended capitation and in another area was there formal rostering, and what did the pay-for-performance refer to?

Physicians who chose to change to a new model, opted for one of a number of models, which we have highlighted.

Pay for performance refers to payments for achieving specific targets for preventive care including cancer screening, influenza immunization and

childhood immunization. However, pay for performance is not relevant to this study, so we have removed this phrase, to try to avoid reader confusion. We have tried to further clarify the PEMs by stating: ‘Several models of care in Ontario require patient enrollment, including models in which physicians are compensated by blended capitation (monthly age and sex adjusted payments and a small proportion of fee for service payments), and those paid by fee for service, collectively described as Patient Enrollment Models (PEM).’

5. I have no issue with the methodology – it is fairly standard in this sort of analysis. The measure of attachment differs from those used in other jurisdictions, but has been well-validated.

6. Beginning in line 10, page 5, the differences in utilization rates beg addressing, but this is not done in the interpretation. Not doing so could risk the naïve reader to conclude that attachment is bad thing, leading to increased utilization, whereas it is known that those with chronic (complex) conditions necessarily have a higher attachment rate but also need formal institutional services more frequently.

We thank the reviewer for this feedback and have added the following sentence: “High rates of health services utilization among attached patients likely reflects the higher comorbidity in this population, with associated greater need for health services.”

7. I have major concerns with the Interpretation. I find its tone is one which concludes that the mentioned policy changes in Ontario are causally linked to attachment rates and changes in rates. As noted above in point 6, there are many results in this study which should be commented upon, even though only brief comment may be necessary. For example, pointing out that the greatest reason for “uncertain attachment” in younger people is by personal choice (70% of unattached in my province are so by choice). Identifying that those who are under greater social strain (which this study confirms) may have lower attachment due to issues of access and competing priorities. The final statement of the paragraph beginning on page 6, line 28, and in the Conclusion, come across as a bit naïve, and are more system-centred than person centered. In other words, solutions may not be so much medical as social. Perhaps the authors imply this; if so, it should be explicitly stated.

We have added these points to the Limitations section. We are unable to determine the extent to which patients’ choices and priorities affected attachment from administrative data. Clearly social factors play an important role but given the importance of having an ongoing site of primary care, system wide approaches to addressing barriers to attachment remain an important policy priority.

8. The final paragraph of the Interpretation is certainly very relevant in Canada at this time.

However, this paragraph sort of “hangs” alone, without context being drawn between the rest of the findings and knowledge base. Whether this paragraph remains is up to the authors or editor, but if it remains (and I think it is relevant) the linkage between access, attachment, continuity of care and outcomes needs to be strongly highlighted.

I found this comment to be confusing – the last paragraph relates to HHR considerations. Thoughts on a response?

Some specific comments are as follows:

1. In the last sentence of the first paragraph of the Introduction, I presume the authors are referring to independent states i.e. people who are new immigrants or low income versus new immigrants with low income... Please clarify.

Yes each of the named groups have been found to be less likely to be attached. We have tried to clarify the sentence. We have used 'or' rather than 'and' to indicate that any of these groups have been reported to be less likely to be attached.

2. Because of differences in policy, practice, as well as differing methodologies in the measurement of continuity, the authors should be cautious when comparing Ontario to other provinces in both the Introduction but particularly in the Interpretation. In a similar vein, due to significantly differing health system policies, I would not recommend comparing Ontario to other countries. For example, in some countries, attachment is virtually 100% as there is mandatory rostering.

The cross Canada comparators are derived from the Canadian Community Health Survey, which use patient reported and use similar methodology across Canada. Similarly in countries with mandatory rostering, patients have a site of ongoing primary care, even if they don't necessarily see it as optimal.

Reviewer 1: Dr. Darren Lau

Institution: University of Alberta

General comments (author response in bold)

Comments to the Author

Bayoumi et al. report a panel study of patient-primary care attachment over a 10 year period 2008-2018. Using a piece-wise linear regression, they were able to identify a temporal increase in attachment until 2014, coinciding with a period of policy activity in primary care reform in Ontario. They documented disparities in patient attachment. While the growth period 2008-2014 some of these disparities diminish, they remained prominent on adjusted logistic regression in 2018. Their overall conclusion is that progress has "stalled", and persistent gaps continue to require targeted intervention and policy changes.

This was a very well-designed, well-analyzed, and well-written paper.

I have the following comments:

Is the data required to calculate the Ontario Marginalization Index part of the health administrative data, or is this linked in separately from a database not mentioned? Is it routinely updated every year?

The OnMarg index is based on variables from the census. This has been clarified in the text. It is updated with each new census.

Methods

- May need to describe that the ACG system produces both ADG and RUG and whether these refer to independent quantities. Then ACG/ADG sentence should probably go under Covariates, where comorbidity variables are presently not mentioned (but should be).

As noted above, we have augmented the description of the ACG system including the ADGs and RUBs.

- How does patient attachment differ from continuity of care?

Continuity of care is used in the third step in our attachment algorithm in order to limit attributing attachment to patients who receive the bulk of their care from physicians with low continuity of care for their patients, such as walk in clinic PCPs. We have added this clarification to the text.

- The 3rd step of determining attachment made much more sense after I had a look at the validation paper (Jaakkimainen et al., 2021).

We tried to better contextualize the third step, as noted above.

Results

I found the presentation of the results straightforward and have no additional suggestions or comments.

Interpretation

- I would suggest revising for more neutral language.
 - o Could use “plateau” instead of “stagnate”.
 - o Could drop the word “impressive” (interpretation first para).
 - o In conclusion paragraph – could remove entirely “lack of on-going progress”. I do like the last sentence.

The suggested changes have been made.

- No mention of why policies changed in 2015 – could serve as important context.
The policies were changed in an attempt to contain costs.

Additional Limitations

- o Does quality of attachment or traditional continuity of care still matter? I note that a patient would still be considered attached if they had the majority of their health care visits to a walk-in doctor who happened to have > 10% of her / his patients having the majority of their visits with her / him – not exactly a high standard to “having a family physician” but perhaps a minimum standard of being able to access a provider of some sort of longitudinal care.

• The assessment of quality of attachment is important but cannot be assessed with our algorithm using administrative data. We have added this to the limitations.

- o Is it quite clear in the literature that increasing attachment will decrease health care utilization and bad outcomes, or increase targeted intermediates (e.g.: HbA1c in diabetes)? While attachment is a national aim that is relatively non-controversial (the system really suffers when patients can't find reliably accessible primary care), I suspect the literature on changes in attachment --> future outcomes may benefit from more longitudinal studies, and there are probably patient populations who don't benefit from the drive to attachment.

• Observational studies consistently describes associations between attachment and improved health outcomes including more preventive care, better chronic disease management, reduced mortality, and reduced use of emergency departments, hospitalizations and readmissions. The questions posed by the reviewer represent important areas for future research, which we have added in the Conclusion.