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Title	Change in health care utilization following coordinated care planning in Hamilton Niagara Haldimand Brant LHIN: a quasi-experimental study
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Reviewer 1	Lee Green
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General comments (author response in bold)	<p>1. For reference 13, my attempts to bring it up redirected to the current news page, rather than the 2012 page cited. This reference directs towards an archived news release from the Ministry of Health and Long-Term of Ontario which introduced the implementation of the Health Links model of care within the province.</p> <p>2. In the Methods, it is not clear how someone who is a candidate ends up without a coordinated care plan. How actual enrollees were chosen is important information, to assess selection bias that may not be fully controlled by propensity score matching. If (as seems likely from their death and long-term-care rates) they were indeed different in some way, regression to the mean could be a larger effect for them than for controls. Additional information has been added to address the process of being identified as a high health care user within the LHIN (starting on line 48). Briefly, individuals not on care plans may include individuals who have refused enrollment on the care plan or individuals who have yet to be approached for enrollment. As there are over 20,000 individuals on the high health care user list at the moment and there are a limited number of coordinated care plans that can established each year, it is also possible that individuals who are eligible to be included have not yet been approached for inclusion. Reference to this has been added at line 219 of the paper.</p> <p>3. In line 71 and following, how was information from NACRS and DAD used to determine the listed chronic conditions? Was a single occurrence of a diagnostic code sufficient to assign the patient to that condition, or was a more detailed assignment algorithm used? A single sequence of the selected chronic conditions (based on the ICD-10-CA diagnostic codes identified in Appendix 1) in any inpatient or ambulatory administrative record in IDS prior to care plan enrollment date was used to determine whether the patient had that particular condition.</p> <p>4. In line 94, was backward or forward stepping used, and what was the threshold? The model included stepwise selection with a significance level for entry and stay of 0.15 – this has been added to the manuscript at line 121.</p> <p>5. Was there a particular reason for using a 1:1 match? The potential control group seems large enough that a 2:1 match would have been feasible. The goal of higher matching schemes is to increase precision and statistical power [Rassen JA, Shelat AA, Myers J, Glynn RJ, Rothman KJ, Schneeweiss S. One to many propensity score matching in cohort studies. Pharmacoepidemiology and drug safety. 2012 May 1;21(S2):69-80]. However, 1:n matches (where n>1) come at a cost of increased bias in the resulting sample as second and consequent matches are weaker when compared to their counterparts [Rassen et al, 2012]. In the current study, both groups (coordinated care plan enrollees and control group participants) had a high sample size of more than 500 participants per group in the six month analysis and 494 participants per group in the twelve month analysis. If the matching was conducted using a higher scheme (1:2, 1:3, etc.), the likelihood of successful matching would have decreased especially for coordinated care plan enrollees who had high health care utilization. As was seen in the study, it was not possible to arrive at a match for 10% of the coordinated care plan enrollees. Furthermore, the use of 1:1 matching in this evaluation is consistent with the majority of cohort studies that use propensity score matching [Rassen et al, 2012].</p> <p>6. The results are presented clearly. Thank you.</p> <p>7. The interpretation discusses the main finding and the likely difference (lines 188-194) between enrollees and controls well. Not addressed are the decrease in utilization among controls (likely regression to the mean), the study's limitations, why hospital utilization was not affected but ED was, and especially (lines 169-181) why this intervention might work better than others cited. That would be important to know, and it seems likely that the authors would have some ideas to share. The following has been added to the paper starting at line 201: "One of the reasons that the high care user controls improved over time is that "high use" is not a permanent condition for most people. Based on a previous study of health care use among Ontarians, it was shown that of individuals who were identified to be in the 95th percentile or greater in terms of health spending in 2009, around 38% of the individuals were classified to be below the 90th percentile for spending by 2011" [Wodchis WP, Austin PC, Henry DA. A 3-year study of high-cost users of health care. Canadian Medical Association Journal. 2016 Feb 16;188(3):182-8.]. Furthermore, starting at line 201, the following was added: "The present study indicated that the number of emergency department visits was lower among care plan enrollees compared to matched controls. A similar finding was not shown for hospitalizations. A possible explanation for this may be that coordinated care planning had the greatest impact on reducing the number of less major incidents that did not need an emergency medicine visit, such as managing minor and moderate issues in the community."</p>
Reviewer 2	Joseph Puyat
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General comments (author response in bold)	<p>a.) However, 9% of the individuals with coordinated care plans were excluded from the analysis because they couldn't be matched with suitable controls. These 9% were less healthy and high users of healthcare services relative to those who were matched. This unmatched fraction of patients is probably the proportion of individuals on whom coordinated care plans could have had the most dramatic impact. Future studies could, perhaps, try and assess the benefits of coordinated care plans on these select individuals. Thank you for this comment. In fact, we are further exploring the health utilization trajectories of individuals who are the highest users of services among care plan enrollees to better understand the role of care planning on these individuals. As shown in Table 3 of the paper, these individuals did experience decreases in utilization when comparing 12 month pre- and 12 months post-index data.</p> <p>1) The link between coordinated care plans and ED visits is easy to appreciate. The anticipation that coordinated care plans could</p>

result in reduced LOS, however, is a bit of a stretch. Could the authors articulate in a few sentences in the manuscript why coordinated care plans could reasonably reduce LOS?

In terms of length of stay, with greater care coordination, the length of stay in hospital could be potentially reduced as the smooth transition from hospital to community can expedite the process. This reflects what other studies have found. We have made the following addition to the Measures and Sources of Data section: "The primary outcomes of interest for the analysis were the number of emergency department visits, the number of inpatient admissions, and the length of inpatient stay. These measures were chosen as they were identified as priorities for the Health Links model of care provincially or have been identified to be impacted by care planning in previous studies.(15,18-20)".

2) Related to the previous point, there is a risk to evaluating an outcome that cannot be reasonably linked to an intervention, especially when the intervention does not yield appreciable effects. This is because most of the time such evaluations can lead to an over generalization that the intervention has little value. I strongly encourage the authors to clearly articulate their justification for linking coordinated care plans with LOS or else just stick to evaluating outcomes that are closely linked to the intervention, e.g. supporting patients navigate the health system or connecting them to available community supports and services.

The measure of length of stay was decided a priori, along with number of ED visits and inpatient hospitalizations. The reason behind including this measure in the analysis was that based on previous research findings, having a coordinated care plan shortened time in hospital. In the case of the Health Links model of care, it was hypothesized that patients with a coordinated care plan might have experienced a shorter length of stay as individuals with more serious admits already had a community plan in place with coordination among their health care providers post-discharge.

3) I would question the investigators' estimation of the intervention effects using regression analyses that adjusts for baseline levels. Adjusting for baseline is appropriate for study designs where baseline differences between study groups can be assumed to be nonexistent or due to random variations, as is the case with randomized studies. In randomized-groups designs, adjusting for baseline allows for a precise and unbiased measurement of intervention effects. In non-randomized studies, however, like the study under review, adjusting for baseline could result in biased estimates. I would, therefore, recommend that the authors also perform a change from baseline analysis using generalized linear mixed models or similar approaches appropriate to their outcome measures. Results from this type of analysis that are consistent with the authors' original conclusion would lessen doubts that the reported relative differences between groups in ED visits at 6 months (i.e. IRRs in Table 5) is not an artifact of the type of statistical analysis performed on the data.

This is an important consideration and we sought statistical advice prior to undertaking the analyses. With a study design using propensity score, the intent is to adjust for baseline characteristics that could be related to receiving the intervention that would also be dependent upon anticipated outcomes (confounding by indication). The propensity score intends to replicate the randomization such that patients are equivalent on baseline characteristics after propensity matching. Having demonstrated baseline equivalence, adjusting for baseline and looking at change in utilization should not result in biased estimates of the change in scores. When the models were rerun without adjusting for baseline utilization at six months, the IRR for emergency department visits between the groups was 0.92 (0.80-1.04) versus 0.81 (0.72-0.91) when controlling for baseline. At twelve months, the IRR for emergency department visits between the groups was 1.00 (0.88-1.14) without adjusting for baseline as opposed to 0.88 (0.79-0.99) when adjusting for baseline. When the change from baseline analysis was undertaken, the matched care plan enrollees experienced greater decreases in emergency department visits and hospital admissions compared to the matched controls, although the differences were not statistically significant. The results of this analysis are presented in Table 7.