HOSPITAL RESTRUCTURING IN ONTARIO AND ITS IMPACTS ON QUALITY, COST, AND PATIENT WELFARE

by

JENNY WATT

A thesis submitted to the
Department of Economics
in conformity with the requirements for the
degree of the Doctor of Philosophy

Queen's University
Kingston, Ontario, Canada
September 2019

Abstract

In this thesis, I investigate the impacts of hospital mergers in markets where hospitals compete primarily on quality. In Chapter 2, I use previous literature to discuss the potential impacts of hospital mergers, while in Chapter 3, I discuss a specific wave of mergers occurring in Ontario, Canada from 1997 through 2001 and describe the data I use to study these mergers. In Chapter 4, co-authored with Michael Green, we evaluate the effect of the hospital mergers on measures of hospital quality and cost. We use a matched differences-in-differences approach based on the construction of synthetic controls, and we compare hospital mergers involving the closure of acute care services at one or more hospital sites to hospital mergers without this feature. The results suggest that relative to the matched control group, both types of mergers improve quality but only those involving the closure of acute care services improve financial outcomes. In Chapter 5, co-authored with Eliane H. Barker and Michael Green, we use the same data at the patient level, focusing on the impact of mergers on patient welfare. We use pre-merger data and a patient choice model to estimate preferences parameters for travel distance to the hospital as well as for hospital quality and attributes, finding evidence that patients choose hospitals based on distance and quality. We then estimate the distribution of idiosyncratic shocks to utility, allowing us to control for unobservable utility in two counterfactual scenarios. In the first counterfactual, which represents the immediate effect of mergers, hospital sites that close acute care services during the merger wave are removed from the choice set. In the second, these hospital sites are still removed, and hospital quality and attributes are updated using post-merger data. The results suggest that once hospital quality has adjusted, mergers benefit some patients but harm others. For example, compared to urban patients, a greater proportion of rural patients are harmed by mergers. Taken together, these chapters suggest that hospital mergers have the potential to improve quality and cost but that there are trade-offs in terms of patient welfare.

Co-Authorship

Chapter 4 of this thesis is co-authored with Michael Green of the Department of Family Medicine and Public Health at Queen's University at Kingston.

Chapter 5 of this thesis is co-authored with Eliane H. Barker of the Department of Economics at Queen's University at Kingston, and Michael Green of the Department of Family Medicine and Public Health at Queen's University at Kingston.

Acknowledgements

I would like to thank Allan Gregory and James G. MacKinnon for providing the early help that really got these projects going. It is not easy to help someone who is still finding a direction and they really pulled it off.

Even though my supervisor Rob Clark joined me partway through my degree, he made a tremendous difference to the progression of this thesis and I cannot imagine how it would have been without him.

Chris Cotton, Andrea Craig and Maggie Jones provided me invaluable support and advice while I was on the job market.

I would also like to thank the participants of the Industrial Organization Working Group at Queen's, but in particular Eliane H. Barker, who made great contributions to our co-authored project, and Brent Hickman, Eric Richert and David Rosé, whose comments greatly affected the direction of this thesis.

Last but not least, Jason Adams, Saad Ali Khan and Ken Chow helped me out with so many details and I am so glad we were in the program together.

Disclaimer

This study was supported by The Institute for Clinical Evaluative Sciences (ICES) which is funded by an annual grant from the Ontario Ministry of Health and Long-Term Care (MOHLTC). The opinions, results and conclusions reported in this paper are those of the authors and are independent from the funding sources. No endorsement by ICES or the Ontario MOHLTC is intended or should be inferred. Parts of this material are based on data and/or information compiled and provided by The Canadian Institute for Health Information (CIHI). However, the analyses, conclusions, opinions and statements expressed in the material are those of the authors, and not necessarily those of CIHI.

Dedication

To my partner Jason, and to my mother and sisters, for all of their love and support.

Contents

Table of Tables

MAIN BODY

APPENDICES

Table of Figures

MAIN BODY

APPENDICES

Chapter 1

Introduction

The chapters in this thesis are motivated by the fact that hospitals in private and public markets frequently undergo mergers, despite mixed evidence on the effects of these mergers on quality, cost, and other outcomes.[1] Mergers are prevalent in the United States and Western Europe (Weil, 2010), and in Canada, every province has been exposed to hospital restructuring in one form or another (Philipon & Braithwaite, 2008). In Ontario, where around one-third of hospital sites are the product of mergers occurring in the 1990s and 2000s, the current Ontario government is proposing further hospital restructuring that would involve the closure and relocation of services within Ontario hospitals (Ontario Health Coalition, 2019).

---

[1] For this thesis, the combination of the financial and decision-making bodies of two or more hospitals constitutes a hospital merger. A hospital is an organization that may operate out of multiple hospital sites. Mergers may involve other changes such as closures of some locations or the exchange of program between locations, but this is not required.

Given that governments so frequently engage in hospital restructuring, it must be that decision-makers perceive that hospital mergers result in positive outcomes, whether these be cost-related or related to another dimension, such as quality. At the same time, there are ample opponents to hospital mergers, and Ontario hospital mergers occurring during the 1990s and 2000s are still a subject of criticism despite little research on the impact of those mergers (Ontario Health Coalition, 2014a).[2] In Chapter 2, I discuss the perceived benefits and drawbacks of mergers, and review current empirical evidence on the effects of hospital mergers in various healthcare markets. In Chapter 3, I discuss a particular set of hospital mergers occurring in Ontario, Canada between 1997 and 2001, and provide the set of motivations and claims behind this merger wave, in which 65 organizations combined into 23. I provide a brief history of the mergers and the commission responsible for them, and discuss the data available to study these mergers, and some of their limitations.

In Chapters 4 and 5, I use data held by the Institute for Clinical Evaluative Sciences (ICES) to evaluate the impacts of the Ontario merger wave. Though I present some theory in the beginning of this thesis, in general, I am not aiming to model the mechanisms by which mergers change the outcomes of hospitals and patients; rather, I

---

[2] Note that this report by the Ontario Health Coalition relies on research from other countries to criticize the Ontario hospital mergers, pointing to the lack of evidence concerning the Ontario mergers.

have two main goals in my analyses. First, I contribute to empirical literature by focusing on knowledge gaps that harm the ability of planners to implement healthcare policy. Second, over two chapters, I provide some novel modifications of the existing techniques used to evaluate hospital mergers. These modifications are widely applicable and can easily be used to identify causal effects in studies that concern health care and those that do not.

In Chapter 4, co-authored with Michael Green, our knowledge contribution comes from our focus on the hypothesis that for mergers to result in positive outcomes (e.g., cost reduction), physical restructuring, such as site closures, is necessary (Weil, 2010). Since half of the mergers in the Ontario merger wave involve site closures, we can use matched differences-in-differences to compare measures of quality and cost between merged hospitals and a matched control group, constructed from hospitals that never merged during our study period (hereafter, never-merged hospitals). We extend this by allowing heterogeneous treatment effects, capturing differences between mergers with site closures and those without. By doing so, we find evidence that both types of mergers improve quality but that only mergers involving site closures improve financial outcomes. Our method is based on the method used to study United Kingdom (U.K.) hospital mergers in Gaynor, Laudicella and Propper (2012). Both methods begin by calculating propensity scores using pre-merger period data, and for each merged hospital, finding the never-merged hospitals with the closest propensity scores. In Gaynor et al. (2012), these matched never-merged hospitals are used to detrend the outcomes of the merged hospitals. In our method, we use the matches for each hospital to construct an artificial control for that hospital, by weighting the characteristics of the matches using the propensity scores. This allows us to double the number of observations relative to the method employed in Gaynor et al. (2012).

In Chapter 5, co-authored with Eliane H. Barker and Michael Green, we use our data on the patient level, making several contributions to the literature on the impact of hospital mergers on patient welfare.[3] As in other papers, we use pre-merger data and a patient choice model to estimate preferences parameters for travel distance to the hospital as well as for hospital quality and attributes and use these parameters to simulate the impact of mergers on patient welfare. Unlike other studies, we construct a counterfactual in which we incorporate post-merger data, reflecting the real changes caused by the Ontario merger wave. By doing so, we can compare the immediate impact of the mergers to the longer run impact, where quality has adjusted as a result of the mergers. The results suggest that hospital mergers can benefit some patients, but certain patient groups, including rural patients, are likely to be harmed. Another contribution of this study is that we use a relatively simple method to estimate the

---

[3] This chapter will also be included in Eliane H. Barker's thesis.

distribution of idiosyncratic shocks to utility, allowing us to control for unobservable utility in the construction of counterfactuals.

Combining Chapters 4 and 5, we can provide a fairly complete picture of the Ontario merger wave. Though there is evidence that the mergers increased quality and improved financial status for many of the hospitals, patient-level analysis reveals that only some patient groups are better off as a result of the mergers.

Chapter 2

Possible Impacts of Hospital Restructuring: Claims and evidence

In this chapter, I present arguments for and against hospital mergers, as perceived by health researchers, healthcare administrators, and other health-related organizations. I discuss the economic theories that can be used to support some of these arguments, but as the contributions of this thesis are not theoretical, I do so only briefly. A more complete review of theory associated with competition in healthcare markets can be found in Gaynor & Town (2012a).

Following the review of potential benefits and drawbacks, I review the empirical evidence surrounding the impacts of hospital mergers. I divide this into only two sections: empirical evidence on hospital performance (in terms of quality and costs)[4] and empirical evidence on patient welfare. I divide the sections in this way because quality

---

[4] At this point, I also briefly discuss evidence regarding the quality effects of mergers in other industries.

and cost are sometimes evaluated in the same paper, using the same methods. Papers concerning patient welfare on the other hand focus only on patient welfare and use methods distinct from those in other papers.

Note that I do not review the literature on the price effects of mergers in depth. In Ontario, prices for hospital services are administered by the provincial government, and as a result, mergers do not affect prices (Marchildon, 2013). In systems with market-determined prices, such as that in the U.S., price impacts are another consideration. Gaynor, Ho, and Town (2015) provide a review of empirical studies examining price and market concentration, suggesting that mergers will tend to result in price increases when prices are market-determined. Empirical evidence generally supports this suggestion (Gaynor & Town, 2012b).

2.1 Arguments for and against mergers

Whether the prices in a healthcare system are market-determined or administered, many of the arguments for and against mergers are the same. This is because in theory, the behaviour of hospitals in both types of systems is highly similar in many cases (Gaynor & Town, 2012a). For example, hospitals that face administered-prices are theorized to compete on quality,[5] but hospitals that face market-determined prices may

---

[5] Note that this is only a theory, and whether Ontario hospitals actually compete to attract patients has not been well studied. The analysis in this thesis does not rely on competition as a mechanism for quality improvement.

still compete on quality when a high proportion of patients are insured. Therefore, the arguments presented below are relevant to a variety of healthcare systems.

2.1.1 Arguments for mergers

The most common arguments for mergers are as follows:

- *Mergers allow hospitals to downsize services that can be shared, such as administrative and clerical services.* In an older survey of hospital administrators in Ontario, 97% of administrators identified the downsizing of administration and clerical services as an effective form of restructuring (Hanlon, 1998). Though the survey has not been updated in Ontario, a recent survey of U.S. hospital leaders found similar results (Noether & May, 2017).
- *Mergers allow fixed costs to be spread over a larger hospital system*. For example, costs associated with IT do not vary much by hospital size (Noether & May, 2017).
- *Mergers allow economies of scale in quality and cost.* Merging hospitals may increase quality or decrease cost through scale effects other than those above (Noether & May, 2017). For example, hospitals may increase quality via learning-by-doing.
- *Mergers allow "capital avoidance"*. Hospital competition may lead to excess infrastructure on a market level, if hospitals are competing for patients using

quality (Oliver & Leibenluft, 2015). Hospital mergers may prevent the duplication of infrastructure at nearby hospital locations, avoiding unnecessary costs.

- *Mergers allow superior hospitals to influence inferior hospitals.* In the U.S., successful hospitals wishing to expand their market influence may use a "failing firm" defense to justify the acquisition of a hospital that is performing poorly (Perry & Cunningham, 2013). In less extreme situations, the argument can still be made that mergers allow hospitals to share best practices and standardize clinical practices (Oliver & Leibenluft, 2015).
- *Mergers allow the elimination of excess capacity.* In some healthcare systems, decreasing length of stays have led to excess capacity. If this excess capacity is spread between hospitals, hospital closures may result in needs not being met in a particular geographic area (Tijani-eniola, 2016). Mergers allow hospitals to reallocate capacity between locations. Hospitals can use their comparative advantages to decide which location offers which services, which results in the less efficient programs being eliminated. Some proponents have argued that cost savings from the elimination of capacity can be reinvested in quality (Sinclair, Rochon, & Leatt, 2005).

2.1.2 Arguments against mergers

Some of the most common arguments against mergers are as follows:

- *Hospital mergers increase market concentration, which may reduce quality and increase cost growth.* Analogous to well-known theory that there is an inverse relationship between competition and prices when firms compete on price, theory posits that there is an inverse relationship between competition and quality when firms compete on quality, even when prices are flexible (Gaynor & Town, 2012b). From this perspective, there is a concern that mergers could decrease quality competition and therefore quality. Others speculate that hospital administrators in highly concentrated markets are less aggressive in implementing cost saving measures (Kwoka & Kilpatrick, 2018).
- *Hospital mergers are costly, and the savings from mergers do not compensate for their initial cost.* At a minimum, there are legal and administrative costs associated with the combination of finances and decision-making bodies (Sinclair et al., 2005). Depending on the merger, there can be extra costs such as those associated with merger-related layoffs.
- *For mergers not involving service closures, there is no convincing reason that mergers would improve quality or care.* Some researchers have speculated that if

mergers are purely administrative, there is no reason for them to improve outcomes (Weil, 2010).

- *For mergers involving closures, access to care is reduced.* If mergers involve service closures at some locations, some patients will have to travel farther to access care. This may be especially harmful for rural patients, who already have fewer choices (Ontario Health Coalition, 2014a).

2.2 Empirical evidence on quality and cost

The literature on the effect of hospital mergers on quality is small, and inconclusive. Hamilton and Ho (2000) find that hospital mergers in California have no measurable impact on inpatient mortality but raise readmission rates. Cuellar and Gertler (2005) find no negative effects and find evidence that merged hospitals perform fewer unnecessary medical procedures. Romano and Balan (2011) study a single Illinois hospital merger in depth and find that inpatient mortality increases for some procedures but declines for others. Hayford (2012) finds that living in an area with merger activity increases inpatient mortality among heart disease patients in California. In a study of sixteen American states, Mutter, Romano and Wong (2011) find that hospitals that acquire other hospitals improve their quality in several dimensions, including pneumonia mortality.

Studies specific to public systems are similarly non-conclusive. Studies in Ontario (Pérez, 2002) and Newfoundland (Curtis et al., 2005) do not find strong evidence of an impact of acute care[6] hospital restructuring on quality. However, these studies are based on the comparison of means between hospital groups and may not adequately control for selection into merging. A sophisticated study of a large wave of hospital mergers in the United Kingdom (UK) finds that mergers negatively impact mortality and readmission rates for some conditions, but there is no effect for a large variety of outcomes (Gaynor et al., 2012).

Given that the literature on the quality outcomes of mergers is not particularly large and the evidence so far has been mixed, policymakers may wish to also consider evidence from other industries. Unfortunately, papers evaluating the quality effects of mergers are even more rare outside of healthcare. Studies of U.S. airline mergers use measures of timeliness as proxies for quality and find conflicting results. One study finds evidence that mergers decrease airline quality (Steven, Yazdi, & Dresner, 2016), while another finds evidence that they increase it (Prince & Simon, 2017). In a third study, the direction of the impact appears to depend on the intensity of competition prior to

---

[6] Acute care services concern the short-term intensive treatment of conditions such as strokes, severe infections, and bone fractures (Hirshon et al., 2013). Some hospital sites that transferred acute care services closed entirely, while others continued to provide some combination of diagnostic, chronic, rehabilitative, and psychiatric care.

the mergers (Chen & Gayle, 2019). In a study of U.S. banks, Scott and Dunkelberg (2003) use a composite survey-based measure to find that consumers are less satisfied with service quality after bank mergers. Fan (2013) uses a structural model to simulate the impact of mergers on a quality index of news content, finding evidence that mergers can decrease content quality. In a study of mergers of U.S. universities, Russell (2016) finds no evidence that merging universities increase their retention rates.

Studies on the impacts of hospital mergers on costs are also sparse, potentially because of the focus on price outcomes in the literature (Kwoka & Kilpatrick, 2018). In the U.S., results are mixed. One study finds that hospitals across the U.S. have lower cost growth following merger (Spang, Bazzoli, & Arnould, 2001), but another finds no evidence of cost savings from mergers (Capps, Dranove, & Satterthwaite, 2003). A study using only urban markets find that mergers decrease costs, but only when they occur in highly competitive markets (Spang, Arnould, & Bazzoli, 2009). A recent study using hospital mergers throughout the U.S. finds that operating costs per patient decline following mergers (Noether & May, 2017), but another does not find evidence of cost savings from mergers (Schmitt, 2017). In the U.K., one study finds evidence that mergers reduce management costs (Hutchings et al., 2003), but another finds evidence that mergers increase hospital deficits (Gaynor et al., 2012).

2.3 Empirical evidence on patient welfare

The empirical evidence on the effect of hospital mergers on patient welfare is lacking. As shown above, many studies indirectly contribute to the literature on patient welfare by examining factors that affect patient welfare, such as quality. However, examining quality does not provide a complete picture of patient welfare, as even mergers that increase quality may harm patients if the closure of services cause patients to travel further on average to access care. Whether mergers increase or decrease patient welfare will thus depend on the changes caused by the mergers, as well as how patients value those changes.

Several papers estimate patient preferences for quality and travel time by using the demand estimation methodology originally proposed by McFadden (1974). Papers find that both quality and distance matter to patients in the United States (U.S.) (Hodgkin, 1996; Howard, 2006; Luft et al., 1990; McNamara, 1999; Tay, 2003), the United Kingdom (U.K.) (Beckert, Christensen, & Collyer, 2012; Gutacker, Siciliani, Moscelli, & Gravelle, 2016; Moscelli, Siciliani, Gutacker, & Gravelle, 2016; Santos, Gravelle, & Propper, 2017) and other European countries (Beukers, Kemp, & Varkevisser, 2014; Moscone, Tosetti, & Vittadini, 2012; Varkevisser & Van Der Geest, 2007). This indicates that in both markets with administered prices and without, some patients will bypass the nearest hospital to access a hospital of better quality, indicating that both distance and quality are determinants of welfare.

Only a handful of papers use these models to simulate the effects of hospital restructuring. Three of these papers do not calculate an impact on patient welfare, but provide important methodological contributions that are related to the calculation of patient welfare. Adams, Porell, and Robbins (1996) show how patient choice models can be used to predict changes in distance travelled following hospital closures. Capps, Dranove, and Satterthwaite (2003) use U.S. data to estimate willingness-to-pay estimates and use these estimates to predict the price impacts of mergers. Only Beckert et al. (2012) use U.K. data. They simulate mergers and find that increased market concentration from mergers decreases elasticity with respect to quality, indicating that responsiveness to quality decreases after mergers. Only two papers calculate the impact of restructuring on patient welfare. McNamara (1999) uses a patient choice model to simulate the impact of closures on patient welfare for rural U.S. patients. Dranove, and Lindrooth (2010) simulate the impact of closures on patient welfare in a single urban market. Neither study uses merger data. Instead, estimates from a patient choice model are used to simulate the impacts of hypothetical mergers. In both papers, the change in welfare comes only from changes in distance travelled, and since patients dislike extra distance, the welfare impacts of mergers are estimated to be negative. To my knowledge, no study calculating the welfare impact of mergers takes changes in hospital quality into account.

Chapter 3

Hospital Restructuring in Ontario: Institutional details and data

The healthcare system in Canada is public, and decentralized relative to those of European countries (Marchildon, 2013). Each province has the primary responsibility of funding, planning and administering healthcare within their province, and most delegate this responsibility further by allocating power to smaller decision-making bodies within their province. At the beginning of our study period, Ontario is divided into 17 health districts, and hospitals within each district are advised by their respective District Health Council (DHC). Hospitals are operated by non-profit organizations that receive the majority of their funding from the government, allocating funds to staff and other expenses as needed.[7] As such, the Ontario government does not directly manage

---

[7] An exception to this is the funding of physicians, some of whom receive payment directly from the government, most often on a fee-for service basis (Marchildon, 2013). My data does not contain physician information, so I am unable to speak to the effects of mergers on physicians.

hospitals; rather, it influences hospitals by controlling hospital funding (Public Hospital Act, R.S.O. 1990, 2017). The government can also force major changes on hospitals, such as mergers, through the use of parliamentary bills (Sinclair et al., 2005).

In the early 1990s the Ontario government was seeking to merge Ontario hospitals in an effort to remove excess capacity from hospitals and reduce health care costs (Sinclair et al., 2005). Merger proponents within the government claimed that many hospitals had excess capacity due to falling length of stays caused by technological advances, but that the excess capacity was spread unevenly between communities, making it difficult to restructure strictly via hospital closures. In fact, from 1991 through 1995, approximately 11,000 out of 49,000 hospital beds were closed, and these beds continued to take up space in Ontario's 200 public hospitals (Health Services Restructuring Commission, 2000; Ontario Health Coalition, 2014b). Despite the bed reductions, health spending had increased to 32% of the provincial budget by 1995, at a time when the province faced relatively low tax revenue and increasing debt levels (Sinclair et al., 2005). In 1996, Ontario Parliament passed a bill granting the Health Minister of Ontario additional powers over hospitals, with the intention of facilitating widespread hospital restructuring (Health Services Restructuring Commission, 2000). This bill allowed the Health Minister to defund hospitals, to remove the rights of hospitals to operate, and to replace non-cooperative hospital board members, but stipulated that the powers would expire in 2000. In order to take advantage of the powers provided by the bill, the Health Minister formed a commission dedicated to making hospital restructuring decisions—this commission was named the Health Services Restructuring Commission (HSRC).

3.1 The Health Services Restructuring Commission

The aim of the HSRC was to, among other objectives, perform a series of hospital mergers that would better allocate hospital capacity across the province through hospital site closures, service closures, and service relocations (Sinclair et al., 2005). The hope of the HSRC was that this reallocation of hospital services would allow hospitals to eliminate excess capacity and take advantage of quality and cost efficiencies as discussed in the previous chapter.

Near the beginning of its four-year run, the HSRC solicited recommendations from the DHCs regarding hospital restructuring (Health Services Restructuring Commission, 2000; Sinclair et al., 2005), while performing an independent analysis of each district, beginning with urban communities. Sometimes, these recommendations included the closure of acute care services at one or more sites, and the transfer of programs to the merging partner. Since the recommendations of the HSRC often coincided with the recommendations of the DHCs, hospitals were able to predict that they would have to merge even before the HSRC officially issued directions.

Importantly, it was also known when it was very unlikely that a hospital would be directed to merge, meaning that the majority of the unmerged hospitals never anticipated merging.

With regards to the specific selection strategy, the HSRC attempted to use a similar strategy in each geographic area, relying on indicators of hospital quality and cost (Sinclair et al., 2005). However, additional complexities prevented the HSRC from being able to rely on quality or cost cut-offs. Clearly, the HRSC could not simply close acute care services at poorly performing hospitals, as they had to ensure that there was sufficient capacity left in each community. The HSRC was also limited in their ability to merge poorly performing hospitals due to restrictions on which hospitals could be merged. Besides the fact that hospitals had to be in the same district, the HSRC aimed to avoid mergers between Catholic and secular or Anglophone and Francophone hospitals.

By 2000, the year in which the extended powers of the Health Minister were to expire, the HSRC had proposed mergers in every district, and most had been enacted. The HSRC dissolved in 2000, and the handful of still unconsumated mergers occurred over the next few years.

3.2 Available data

During the data creation stage for this research, a history of hospital mergers and closures was available online in the document "Guide to The Master Numbering System" (Ontario Ministry of Health and Long-Term Care, 2016). This guide has since been removed but I retain a digital copy of the file. I created the merger data used in this thesis by using the guide above and cross-checking the dates of mergers using other online sources, such as the websites of hospitals.

Below, Figure 3-1 shows the number of acute care hospital mergers 1997 through 2001. This time period includes most HSRC-directed mergers, except one that was delayed until 2003. There are 23 mergers in total.

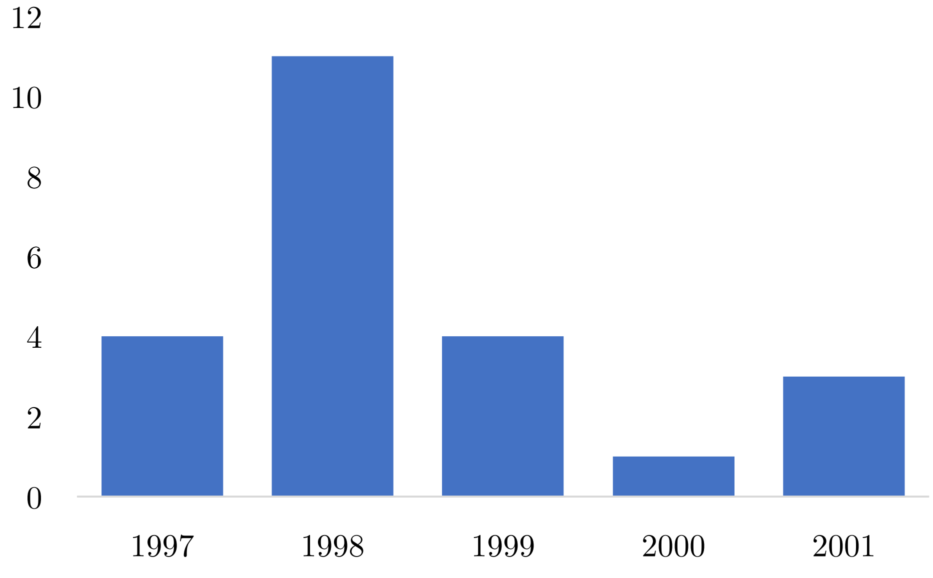


Figure 3-1. Number of acute care hospital mergers per year, Ontario, 1997-2001

As a result of the widespread nature of the merger wave, merged hospitals are located all over Ontario in urban, suburban and rural areas. A map below shows the location of merged hospital sites, including closed sites, 1997 through 2001.

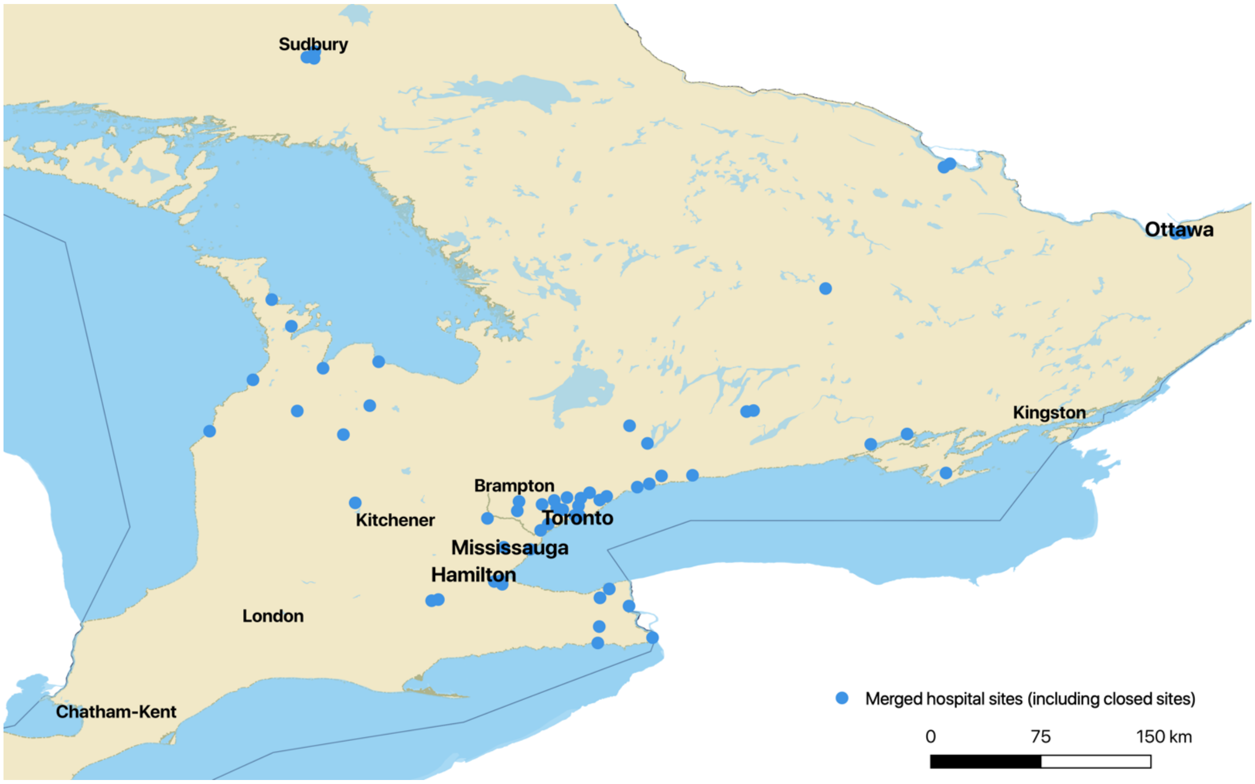


Figure 3-2. Locations of merged hospital sites, including closed sites, 1997 through 2001

To make use of this merger wave, the merger data has to be combined with another data source. For this thesis, the remaining data are held by the Institute for Clinical Evaluative Sciences (ICES), a non-profit organization established in 1992 by the Ontario Ministry of Health and Long-Term Care (Dolan et al., 2012). This organization holds various data, including confidential health data, death records, and census data. Data is accessed in a secure lab, and results released from the lab must conform to confidentiality requirements. This restricts the types of summary statistics that can be produced slightly, but the requirements are generally not an obstacle.

I use data from several data holdings at ICES, cleaning and combining the data into a patient-level dataset, which can then be aggregated when needed. The basis of these data is the Discharge Abstract Database (DAD), a database containing hospital discharge records. A benefit of this database and others held at ICES is that submission of the data is mandatory for Canadian hospitals, so that all Ontario hospitals are included.

Data are available from 1994 and for this thesis, the study period is cut off at 2005, to avoid policy contamination from more recent changes.[8] I combine the DAD with other ICES data holdings, creating variables as needed. Table 3-1 shows the variables used in the first study, presented in Chapter 4. For this study, the variables are defined at the hospital level, rather than the hospital site level. For example, if a hospital has multiple hospital sites, the mortality rate is based on the patient records from all hospital sites.

---

[8] In particular, restructuring plans for the DHCs were formalized in 2006 (Gardner, 2006).

TABLE 3-1. SELECTED VARIABLE DEFINITIONS

Variable	Source	Definition
Outcomes		
Hospital-standardized mortality ratio (HSMR)	*DAD* *ORGD*	The hospital-standardized mortality ratio is the number of observed deaths over the number of expected deaths (calculated using logistic regression), multiplied by 100. Conditions that account for few or no hospital deaths are excluded.[*]
Average ALC length of stay (days)	*DAD*	The average number of days a patient remains admitted after they no longer require acute care, all cases. This is not available at the patient level, and therefore cannot be disaggregated by patient subgroup.
Average distance (km)	*DAD* *PCCF*	The in-sample average of the geodesic distance between the patient and the hospital site of admission.
30-day (std) mortality, condition-specific	*DAD* *ORGD*	The proportion of patients who die within 30 days of admission, indirectly standardized[**] by age, sex, admission type (ambulance or other), and comorbidity, by condition.
Average length of stay (days), condition-specific	*DAD*	The average number of days from admission to discharge, by condition.
Occupancy (%)	*CMDB*	The number of inpatient days in the period multiplied by 100, over the number of available beds in the period multiplied by the number of days in the period.
Total beds	*INST*	The total number of funded beds. This may be less than the physical number of beds.
Volume (discharges)	*DAD*	The annual number of discharges, including deaths.

table continued on next page

TABLE 3-1 CONTINUED

Specialization	*DAD*	The number of discharges for a specific condition over as a percentage of total discharges.
Compensation paid to unit-producing personnel (UPP) ($00,000s)	*CMDB*	The compensation paid to unit-producing personnel, in 1000s. Unit-producing personnel are the personnel who directly fulfill the service mandate of the hospital.
% spent on admin (%)	*CMDB*	Expenses towards administrative support as a percentage of total expenses.
Total margin (%)	*CMDB*	The hospital surplus or deficit as a percentage of total revenue (cost recoveries are not included in revenue).
Additional characteristics		
Teaching	INST	One or more sites operates a teaching program.
Urban	INST	All hospital sites are classified as "Urban" based on the Rurality Index for Ontario.***
Suburban	INST	All hospital sites are classified as "Suburban" based on Rurality Index for Ontario (RIO) scores.
Average Comorbidity	DAD	In-sample average comorbidity is based on the average ADG Score, derived from Johns Hopkins ADG categories.****
% admitted by ambulance	DAD	In-sample number of patients admitted by ambulance as a percentage of the total in-sample number of patients.

CMDB: Canadian Management Information System Database. *DAD:* Discharge Abstract Database. *INST:* ICES Institution Database. *ORGD:* Vital Statistics. *PCCF: Postal Code Conversion File.*

[*] See Appendix C and Canadian Institute for Health Information (2016) for details.

[**] See Appendix C and Naing (2000) for details on indirect and direct standardization.

[***] See Kralj (2009) for an overview and a list of communities and their RIO score.

[****] See Appendix B and Austin & Van Walraven (2011) for further discussion.

In Chapter 5, these variables are calculated at the level of the hospital site, but the variable definitions are mostly the same, so a separate table is not presented here. Chapter 5 also utilizes patient-level variables, described within the chapter.

3.2.1 Note on measuring quality using ICES data

In this thesis, quality is reflected by measures that are calculable from discharge data, such as mortality rates and length of stays. While deaths are clearly undesirable, the relationship between length of stay and quality is less clear. Shorter length of stays in merged hospitals may indicate that patients recover faster from surgery, but it may also indicate that institutions are discharging patients earlier due to increased pressure on the hospital system. Consistent with the second scenario, Hamilton and Ho (2000) find that merged institutions in California discharge heart attack patients sooner but experience a higher rate of readmissions for heart attack patients relative to non-merged institutions.

I acknowledge that these outcomes are crude measures of quality that essentially reflect the failure rates of hospitals. However, as hospitals do not report meaningful quality measures,[9] existing studies on hospital mergers use similar outcomes to measure hospital quality. The lack of meaningful measures means that quality changes in chronic

---

[9] See Rubin, Pronovost and Diette (Marchildon, 2013) for a discussion of some detailed quality measures.

and psychiatric care are difficult to measure, which may explain why none of the papers discussed in Chapter 2 examine outcomes specific to these types of care. The studies in this thesis are no exception—only hospitals providing acute care are used in analysis. Though this means that the policy recommendations are most relevant to acute care hospitals, the majority of hospitals offer acute care services.

Due to the limitations in available quality measures, I must restrict myself to conditions with non-negligible mortality risk. This is not a large limitation. Theory and evidence suggest that hospitals do make investments in quality with respect to emergency conditions, even though we would expect patient choice to be limited in an emergency (Gaynor & Town, 2012a). This is consistent with a model in which hospitals invest in quality in broad categories (e.g., emergency room quality) and cannot simply neglect conditions where patients cannot be as sensitive to hospital quality because of time sensitivity. It should then be possible for mortality rates and length of stays to change in response to policy, even when emergency conditions are considered. In my studies, I focus on four diagnoses: acute myocardial infarction (AMI, commonly known as heart attack), pneumonia, sepsis (blood infection), and stroke. All four conditions involve a high number of emergency admissions with around 50% of patients in the data arriving via ambulance. Pneumonia, sepsis, and stroke may also occur as complications of medical treatment,[10] but unfortunately, I am unable to determine whether the condition I observe is the result of medical treatment and must consider this as a confounding factor in the interpretation of my results. During the study period, rates of hospital-acquired pneumonia and hospital-acquired sepsis are relatively stable in Canada (Taylor et al., 2016).

3.3 Note on transferability of results to other healthcare settings

As the Ontario merger wave occurred in a publicly manage healthcare system, evidence obtained from the study of the merger wave is highly relevant to other publicly managed systems such as those in the Nordic countries, the U.K., and Southern Europe. However, there is still relevance to systems such as those in the United States, for two reasons. First, private systems may still have some public components. This is the case in the U.S. system where around one quarter of hospitals are publicly run (American Hospital Association, 2017). Second, even private hospitals may compete on quality to some degree and may even compete primarily on quality in markets where patients are insensitive to price, such as those markets where most patients are insured (Gaynor & Town, 2012a).

---

[10] See Taylor et al. (2016) for a discussion of hospital-acquired infections in Canada.

Chapter 4

The Impact of Restructuring on Quality and Cost

4.1 Introduction

Hospitals in both public and private healthcare systems frequently undergo mergers, despite mixed evidence on the effects of hospital mergers on quality and cost (Gaynor & Town, 2012a). This chapter provides new evidence on the effects of hospital mergers in public healthcare systems, using both patient and hospital outcomes. More specifically, we seek to provide evidence regarding three main questions. First, what is the effect of hospital mergers on patient outcomes, such as mortality, readmission, and length of stay? Second, what is the effect of mergers on hospital outcomes, such as volume, and the percentage of expenditure spent on administration? Third, are these effects different for hospital mergers that involve extensive physical restructuring? We motivate this third question by the claim that hospitals that physically restructure at merger realize greater improvements in outcomes (Gaynor & Town, 2012b; Weil, 2010).

To address these questions, we study hospital mergers between 1997 and 2001, a time period which overlaps with the time period during which the HSRC was in operation. In total, we study 21 mergers involving 61 hospitals. Out of these mergers, 11 involve acute care service closures and relocations, which can be used to examine the effects of physical restructuring.[11] Specifically, we examine the case where merged hospitals close acute care services at one or more sites and transfer the service programs to the remaining sites. We use matched differences-in-differences to compare these hospitals to merged hospitals without acute care service closures, and to selected never-merged hospitals. By doing so, we examine the differences between mergers involving significant physical restructuring and mergers that are more administrative.

Using the Ontario merger wave to provide evidence of the causal effect of mergers on quality, cost and capacity provides us with several advantages.[12] A key challenge in analyzing hospital mergers is dealing with the fact that selection into merging is clearly non-random and may be based on characteristics unobserved to

---

[11] We study fewer mergers than the number that occur during the time period. We exclude one merger because the involved hospitals form a network several years prior to merging, contaminating the treatment effect. We exclude one more merger because a hospital site is closed several years after merging, rather than at the time of the merger.

[12] All variable definitions are given in Table 3-1.

researchers using administrative data. In our case, mergers are determined by a central decision-maker and elements of the decision-making process are published (Sinclair et al., 2005). Accordingly, we can use published selection criteria to inform the variables used in matching. Another common challenge in evaluating mergers is that large-scale merger waves typically occur over several years, making contamination from other policies a concern. In the Ontario case, there are no significant healthcare reforms previous to or following the merger period, allowing us to estimate the merger effects with relatively little worry about confounding policies. This lack of policy contamination is in contrast to a set of hospital mergers in the United Kingdom (UK), where prior to the merger wave, the then-ruling political party had sought to increase competition by breaking up hospital corporations (Gaynor et al., 2012). There is also evidence that the Patient Choice Reform following the UK merger wave increased hospital quality via competition (Cooper, Gibbons, Jones, & McGuire, 2011).

Despite these advantages, our main empirical challenge is the non-random selection of hospitals into merging. Merged hospitals are significantly different from hospitals not selected to merge in almost all characteristics examined, and it is unlikely that the two groups would have followed parallel trends in the absence of the mergers—an assumption required for differences-in-differences. To overcome this, we use a variation of matched differences-in-differences, similar to Dranove & Lindrooth (2003) and Gaynor et al. (2012). As in those papers, we use pre-merger data to compute propensity scores, which are then used to select from hospitals that do not merge during the study period (never-merged hospitals). This removes hospitals that would prevent the parallel trends assumption from being met. Our approach differs from that in earlier papers in that for each hospital, we use the closest matches to create a synthetic control hospital, where the characteristics of the synthetic control hospital are a weighted average of the characteristics of the matches. This is similar to the synthetic control method as outlined in Abadie and Gardeazabal (2003), but the method to determine the weights differs, and some modifications are required because of the nature of merger-related data.

Specifically, when examining a hospital merger, we observe two or more hospitals before the merger but only one after. To create comparable outcomes, we must pseudo-merge the hospitals in the pre-merger period, aggregating the outcomes of the hospitals involved in the merger. Accordingly, we pseudo-merge the corresponding synthetic control hospitals, allowing us to use the pseudo-merged/merged hospitals and their pseudo-merged matches in a differences-in-differences estimation with entry into treatment in multiple time periods. We then allow heterogeneous treatment effects, investigating the claim that physical restructuring impacts merger outcomes.

By comparing merged hospitals to never-merged hospitals that are similar to the merged hospitals in characteristics that predict merging, we find some significant effects including a decrease in the hospital standardized mortality ratio (HSMR) and a decrease in total compensation paid to staff. In regard to physical restructuring, hospitals that close one or more acute programs at the time of merger do not appear to experience a greater improvement in quality, but they do experience an improvement in financial performance—as measured by total margin—that is not observed in the absence of physical restructuring. This is consistent with previous speculation that restructuring may be necessary to improve financial outcomes (Weil, 2010).

This chapter contributes to the small empirical literature about the effects of hospital mergers on hospital quality, discussed in Chapter 2. We add to the literature on hospital mergers and quality by examining the differences between hospital mergers that involve the closure of acute care services at one hospital site and those that do not, finding no significant difference in terms of quality. This chapter also contributes to the literature on the impact of hospital mergers on financial performance, though a smaller number of measures are used compared to some studies that focus on financial performance. As a reminder, several studies find evidence that hospital mergers reduce costs, but the evidence is mixed and null results are common in the literature (Schmitt, 2017). Our study provides evidence that mergers have a positive impact on financial performance—as measured by total margin—when physical restructuring is involved. An increase in this measure is especially significant because total margin is highly correlated with other cost measures such as low average costs (Rosko, Wong, & Mutter, 2018). The impact of Ontario mergers on financial performance and productivity is investigated further in Barker and Tranmer (2019) which is preliminary at the time of this writing.

The reminder of this chapter is organized as follows. Section 4.2 provides descriptive statistics specific to this chapter. Section 4.3 outlines the empirical framework, and Section 4.4 presents the results. Section 4.5 concludes the chapter.

4.2 Data and descriptive statistics

Chapter 3 of this thesis discusses the history of the hospital mergers and presents the locations of the merged hospitals. It also contains information on the data source, including a table of variable definitions relevant to this chapter. The text below briefly overviews the data source and provides descriptive statistics specifically relevant to this chapter.

Our primary data source is the Discharge Abstract Database (DAD) for Ontario, submitted to the Canadian Institute for Health Information (CIHI) and maintained by the Institute for Clinical Evaluative Sciences (ICES). The submission of discharge abstracts to this database is mandatory for Canadian hospitals. We use data from 1994 through 2005, a period corresponding to three years before and the four years after the merger wave.

We use approximately one-and-a-half million discharge records to construct an aggregated dataset that allows us to analyze all outcomes at the hospital level. The primary reason for doing so is practical; the hospital-level data facilitate several matching strategies that the patient-level data do not. Additionally, aggregating data can increase statistical power when aggregating reduces variation in outcomes (MacKinnon & Webb, 2016). The intuition is that removing variation increases the signal-to-noise ratio when unobserved factors cause the variation (Kourentzes, Rostami-Tabar, & Barrow, 2017). In the case of hospital mortality, for example, it is easy to imagine that for the economist using secondary data, it is difficult to observe the factors that predict mortality for individual patients. Indeed, various patient-level scoring systems have been shown to be insufficient in predicting mortality (S. Scott et al., 2014). Predicting a mortality rate, an indicator of how hospitals perform on average, is a less daunting task. Additionally, patient-level factors that do have some explanatory power (such as age and sex) can be incorporated into aggregated statistics using risk adjustment. Thus, we proceed with the aggregated dataset for this particular chapter and revisit the patient-level data in Chapter 5.

4.2.1 Outcomes

4.2.1.1 Patient outcomes

We separate patient outcomes into two groups: condition-specific and non-condition-specific. Condition-specific patient outcomes are the 30-day mortality rates and the mean length of stay. The standardization of the mortality rates is discussed further in Appendix C.

Non-condition-specific outcomes are the hospital-standardized mortality ratio (the HSMR, discussed further in Appendix C), the average distance travelled to the hospital, and the mean alternate-level-of-care (ALC) length of stay—the mean number of days patients remain the hospital after no longer requiring acute care. Average distance is calculated based on the home address of the patient, which hospital staff update at each admission. We acknowledge that errors may arise from patients attending the hospital while travelling, but that these errors affect both merged and never-merged hospitals, reducing the potential for bias.

All patient outcomes, whether specific to a condition, exclude patients under the age of 18. Outcomes and other controls are calculated based on the entire DAD whenever possible, but average distance and some controls are not available for the whole sample and are based on patients with the conditions listed above.

When examining a hospital merger, we observe two or more hospitals pre-merger but only one following the merger, making it necessary to construct aggregate outcomes for merged hospitals pre-merger. For the patient outcomes, the pre-merger outcomes are a weighted average of the outcomes pre-merger. We calculate the time-varying weights for each hospital as the annual volume of the hospital over the total annual volume of the set of hospital involved in the hospital merger.

4.2.1.2 Hospital outcomes

We also examine hospital outcomes. To assess hospital capacity, we use volume (as measured by discharges), the occupancy rate, the total number of beds, and the compensation paid to unit-producing personnel. Unit-producing personnel (UPP) are personnel who directly fulfill the service mandate of the hospital. Together these outcomes allow us to provide evidence as to whether merging causes a decrease in capacity as it appears to in the UK case (Gaynor et al., 2012).

To assess hospital costs, we use the percentage of expenditure spent on administration, and total margin. Total margin is given by the surplus or deficit of the hospital as a percentage of total revenue, allowing us to examine the relative financial success of merged hospitals while taking hospital size into account.

Similar to the patient outcomes, we construct aggregate measures for the merged hospitals pre-merger. Most outcomes are constructed using the same weights used to construct the patient outcomes. Volume, compensation paid to UPP and total beds are given by the annual sum of the outcomes within the sets of merging hospitals.

4.2.2 Descriptive statistics

We analyze data from 1994 through 2005 on 21 merged hospitals and 102 never-merged hospitals, where our 21 merged hospitals are the result of mergers between 61 hospitals. Though data are available past this time, we cut the study period short to avoid introducing bias caused by the restructuring of the DHCs in 2007 (Gardner, 2006).

Hospitals selected to merge differ from hospitals that are not selected to merge in observable characteristics that may affect both quality and the probability of merging. For example, hospitals that were selected to merge are more likely to have teaching status (28% versus 9%) and urban status (66% versus 24%), relative to hospitals that were not selected. In our pre-merger period, hospitals that were selected to merge are significantly different in terms of our chosen outcomes, relative to hospitals that were not selected to merge. We first summarize the differences in baseline outcomes between these two sets of hospitals. We then examine the differences between merging partners.

4.2.2.1 Baseline outcomes in merged and never-merged hospitals

In the year prior to the merger wave, there are some significant differences between (to be) merged and never-merged hospitals. Using data from the 1996 fiscal year, we compare hospitals from these two groups and perform Welch tests of the difference in means of all outcomes. Table 4-1 presents the results. Relative to never-merged hospitals, hospitals that would merge in the next five years are larger in terms of total beds, volume, and compensation paid to UPP. They are more efficient in terms of compensation paid to UPP, but they have a much lower total margin, indicating poorer financial performance. Given that hospitals selected to merge are significantly different from those not selected to merge, a goal of our estimation strategy is to find a more comparable control group.

TABLE 4-1. MEAN OUTCOMES, MERGED VS. NEVER-MERGED, 1996

	Merged		Never-merged		
	Mean	SD	Mean	SD	Difference
Patient outcomes					
Hospital-standardized mortality ratio	100.22	0.44	104.62	2.70	-4.40
ALC length of stay (days)	8.56	0.83	8.68	0.71	-0.12
In-sample distance travelled (km)	17.43	1.76	23.87	5.85	-6.44
30-day (std) mortality (%)					
AMI	31.92	5.76	28.16	4.46	3.76
Pneumonia	25.05	1.41	30.13	7.08	-5.08
Sepsis	24.73	2.81	27.44	3.78	-2.71
Stroke	37.36	3.48	36.39	5.38	0.97
Average Length of stay (days)					
AMI	8.90	0.34	8.23	0.21	0.67
Pneumonia	12.00	0.68	10.42	0.49	1.58
Sepsis	15.56	1.19	10.85	0.59	4.71***
Stroke	14.91	0.67	12.10	0.66	2.81**

table continued on next page

TABLE 4-1 CONTINUED

Hospital outcomes					
Occupancy (%)	85.09	2.51	70.34	2.16	14.75^{***}
Total beds	198.25	28.80	91.91	9.77	106.34^{***}
Volume (discharges)	784.56	87.99	339.97	39.10	444.59^{***}
Compensation paid to UPP ($100,000s)	402.03	57.03	170.77	20.49	231.26^{***}
% spent on admin (%)	6.03	0.39	6.67	0.28	-0.64
Total margin (%)	13.07	3.27	24.85	2.98	-11.78^{**}
Observations	61		102		

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Variable definitions in Table 3-1. These values are unweighted and are calculated by averaging over hospitals.

4.2.2.2 Differences between merging partners

Given that merging is a strategy used to deal with poorly performing hospitals, we might expect to see poorly performing hospitals merge with successful hospitals, in the hopes of improving outcomes. Due to restrictions surrounding data confidentiality, we cannot present the characteristics of individual hospitals or even the mean outcomes of small groups of hospitals. Instead, for each of our non-condition-specific outcomes, we find the merging partner with the highest value and the merging partner with the second highest value in each pair and calculate the average in these groups. Table 4-2 presents the results. For measures of quality (the HSMR and mean ALC length of stay) and measures of capacity (total beds, volume, and compensation paid to UPP) the values are significantly different between the partner with the highest value and the partner with the second highest value. The in-sample mean distance travelled is not significantly different, nor are the measures of cost (the percentage spent on administration and total margin). It should be noted that some mergers involve more than two merging partners, but ICES confidentiality requirements prevent us from presenting statistics on small subsets of the hospitals. Were we able to present statistics on additional partners, we may observe differences in the percentage spent on administration and total margin.

TABLE 4-2. MEAN NON-CONDITION-SPECIFIC OUTCOMES, DIFFERENCES IN MERGING PARTNERS, 1996

	Partner with highest value		Partner with 2nd highest value		
	Mean	SD	Mean	SD	Difference
Patient outcomes					
Hospital-standardized mortality ratio	101.96	0.58	104.62	0.64	-2.66**
Mean ALC length of stay (days)	12.92	1.61	8.68	1.10	4.24*
In-sample mean distance (km)	22.81	4.06	23.87	2.24	-1.06
Hospital outcomes					
Occupancy (%)	104.74	2.71	70.34	2.91	34.40***
Total beds	357.48	66.77	91.91	22.00	265.57***
Volume (discharges) (10s)	1380.03	162.54	339.96	88.98	1040.06***
Compensation paid to UPP ($00,000s)	710.43	117.21	170.77	74.43	539.66***
% spent on admin (%)	8.10	0.87	6.67	0.41	1.43
Total margin (%)	27.61	5.01	24.85	5.48	2.76
Observations	21		21		

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Variable definitions in Table 3-1. These values are unweighted and are calculated by averaging over hospitals.

4.3 Empirical Framework

Our goal is to estimate the average treatment effect of mergers, allowing for a heterogenous effect for mergers involving the closure of acute care services at one or more hospital sites. Our empirical framework must account for the fact that hospitals that merge are different from hospitals that do not merge. These differences cause an endogeneity problem if the characteristics that cause hospitals to merge influence pre- and post-merger performance. Our chosen framework must also account for the fact that mergers happen over time, and the order in which hospitals merge is non-random. In this case, the regulator in charge of the mergers chose to begin with large hospitals in urban centres (Sinclair et al., 2005). We expect that the characteristics of merging hospitals will vary significantly over the study period, and accordingly, we re-estimate our matching function for each year. We use this matching function to create a synthetic control hospital for each merged hospital. We then specify a difference-in-differences model where hospitals enter into treatment over multiple time periods. This allows us to combine the merged hospitals and synthetic matches from each year, estimating a single equation for each outcome.

Our approach is similar to that of Gaynor et al. (2012) and of Dranove et al. (2003) and can be classified as an extended differences-in-differences approach. As in those papers, our approach uses propensity score matching to construct an appropriate control group for the merging hospitals. Matching is performed for each year in which hospitals merge so that the characteristics of merging hospitals are allowed to vary over the study period. In Gaynor et al. (2012), matched hospitals are used to de-trend the outcomes of the merged hospitals. In Dranove et al. (2003) matched hospitals are pseudo-merged, and the pseudo-merged hospitals form the control group. We modify this by using the propensity scores to construct artificial control hospitals by weighting the characteristics of never-merged hospitals that are similar in propensity score to merging hospitals. We construct an artificial hospital for each merging hospital and use these along with the merged hospitals in a difference-in-differences model with multiple treatment periods. This allows us to construct controls that are more similar to the merged hospitals, versus an approach that uses single matches as controls. It also allows us to double the number of observations used in estimation, versus an approach that uses control hospitals to de-trend the outcomes of the treated hospitals. The identifying assumption of our estimation strategy is that the treatment group and the modified control group would have followed parallel trends in the absence of the merger.

4.3.1 Matching and construction of pseudo-merged controls

The goal of our approach is to estimate, for each outcome of interest, the average treatment effect on the treated for hospital *i* at time *t*:

$$\mathrm{ATT} = \mathrm{E}\{\mathrm{y}^{1}_{\mathrm{it}}|\mathrm{m}_{\mathrm{it}} = 1\} - \mathrm{E}\{\mathrm{y}^{0}_{\mathrm{it}}|\mathrm{m}_{\mathrm{it}} = 1\}$$

Here, m_{it} is an indicator that is equal to 1 for merging hospitals, y^1_{it} is the observed outcome post-merger, and y^0_{it} is the counterfactual outcome—the outcome that would have occurred had the hospital not merged. Identification of the ATT relies on the construction of a valid counterfactual.

We begin with the set of hospitals that never merge during our study period, as hospitals that have been selected to merge in the future are likely not appropriate controls. This is because the merger announcement often precedes the actual merger by several years, causing the potential for anticipation effects. In the context of the involuntary mergers brought about by the HSRC, merged hospitals were aware well ahead of time that they would have to merge.[13] Similarly, decision makers for never-merged hospitals knew ahead of time that their hospitals would not have to merge.

We use a probit model to generate a set of propensity scores for each year, using characteristics that could predict hospital mergers. For a given year, we use in the estimation hospitals that merge in that year and all never-merged hospitals in the sample. As a result, it is possible for a never-merged hospital to be a control in multiple years.

---

[13] By the same logic, anticipation effects could be present in merged hospitals. We test for these anticipation effects in Section 4.4.4. Though we find little evidence of the presence of anticipation effects (when our matching strategy is in place), we make the conservative choice and do not allow hospitals that will merge in the future to act as controls.

Using the propensity scores for a given year, we find the never-merged hospitals with the closest propensity scores to each merging hospital, and we use these hospitals to construct an artificial control hospital for each merging hospital. The artificial hospitals are then *pseudo-merged* so that there is only one control hospital for each merged hospital post-aggregation.[14] For example, if hospital A and hospital B merge, and hospital C is the control for hospital A, and hospital D the control for hospital B, we aggregate the records of hospital C and hospital D. We perform this aggregation for each year in the study period.

To select the number of hospitals used to construct each synthetic control, we repeat the matching process with different numbers of matches and compare the reduction in bias (the difference between the characteristics of the treatment and matched control group) using balancing tests (Smith & Todd, 2005). Using multiple hospitals to construct each artificial control results in a greater reduction in bias than when a single hospital is used as a match, but the total bias reduction actually decreases after four matches, likely because each additional hospital is less similar to the merged hospital in propensity score. Regardless of the number of hospitals used, a weight for each never-merged hospital j matched to the merged hospital M is initially calculated from the propensity scores as follows:

---

[14] We describe the aggregation process in the *Data* section.

$$\frac{1}{|ps_M - ps_j|} \qquad \text{(Eq. 1)}$$

These weights are then standardized and used to calculate the characteristics of the artificial hospitals as convex combinations of the characteristics of the matched hospitals. This formulation means that hospitals that are better matches receive a higher weight in the construction of the artificial controls.

This method is similar to the method of synthetic control matching as outlined by Abadie and Gardeazabal (2003), in that weights are used to create an artificial control out of existing controls. In the standard synthetic control approach, the researcher selects the characteristics to match, and weights are chosen to make the control and treatment group as similar as possible in those characteristics. Our method aims to construct a control group that is similar to the treatment group in the characteristics that predict merging. This avoids forcing the treatment and control group to be similar in characteristics that are irrelevant to the treatment.

4.3.2 Outcome equations

Given the merged hospitals and the pseudo-merged control hospitals, we can then estimate an outcome equation for each of the outcomes of interest. Since our treatment group enters into treatment over time, this will naturally look different than the traditional differences-in-differences equation. Without using hospital characteristics as controls, the equation would take the following form:

$$\begin{aligned} y_{it} = \alpha &+ \beta_1\text{Merged} + \beta_2\text{Merged with Closure}_t + \beta_3\text{Post}_{it} + \beta_4\text{D}_{it} \\ &+ \beta_5\text{D}_{it}\times\text{Merged with Closure} + \eta\mathbf{Year} + \mu_i + e_{it} \end{aligned} \tag{Eq. 2}$$

Here, y_{it} is the outcome in hospital *i* at time *t*, *Merged* indicates that a hospital is a merged hospital, and μ_i represents hospital fixed effects. $Post_{it}$ is a dummy variable that is equal to 1 if 1) the record is for a merged hospital post-merger, or 2) the record is for a control hospital in a year in which the corresponding merged hospital is post-merger, and D_{it} is the interaction of *Merged* and $Post_{it}$. For example, if hospital F is a control hospital for a hospital that merges in 2000, *Post* will equal 0 for hospital F before 2000, and 1 in 2000 and onwards. The variable *Merged with Closure* is an indicator of whether a hospital closed acute care services at one site or more.

Ignoring *Closure* and $D_{it}\times Closure$, the outcome equation appears similar to a traditional differences-in-differences function but with a vector of year dummies, denoted by **Year**. When treatment occurs all in the same period, and $Post_{it}$ takes on the same values for each hospital, $Post_{it}$ is perfectly collinear with year dummies and it is not possible to include both. Since the values of *Post* vary between hospitals in this case, we can include year dummies. Lastly, e_{it} represents the error term.[15] As *Merged* is an indicator for any merged hospital, *Merged with Closure* represents the additional effect of closing acute care services at one or more sites. The hospital effects are then as follows:

TABLE 4-3. HOSPITAL EFFECTS IN TERMS OF COEFFICIENTS FROM (2)

	Total effect pre-merger	Total effect post-merger	Treatment effect
Control hospital	α	$\alpha + \beta_3$	-
Merged hospital	$\alpha + \beta_1$	$\alpha + \beta_1 + \beta_3 + \beta_4$	β_4
Merged hospital w/closure	$\alpha + \beta_1 + \beta_2$	$\alpha + \beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_5$	$\beta_4 + \beta_5$

When estimating these effects, we use time-varying analytical weights based on the volume of the hospital relative to the volume of all hospitals in the given year, as in Gaynor et al. (2012). This is standard practice when aggregating data (Dupraz, 2013), but in our case the results are highly similar with or without these weights.

4.4 Results

We begin by discussing the matching results, which are in themselves meaningful because they give an indication of which types of hospitals were selected to merge in each year. We follow this by discussing patient and hospital outcome results. Lastly, we

---

[15] In order for the DID estimator to be unbiased, the error term and the regressors must be uncorrelated. The use of matched controls can result in the violation of this condition when the predictors of treatment and the predictors of the outcomes overlap, creating dependency between the matched pairs (Hill, 2008). Previous research uses simulations to show that regression-based estimators can be unbiased and efficient when the confounding variables used in the matching process are also used in the regression equation (Wan, 2019). Our baseline specification therefore re-uses our matching variables as hospital controls.

provide some possible explanations for the reduction in volume observed in some merged hospitals and present additional data.

4.4.1 Matching

To predict merging, we perform probit regression using some of our outcome variables as well as teaching status, urban/suburban status, and average patient comorbidity. We avoid using a large number of outcomes as some outcome are highly correlated, which causes convergence failure in maximum likelihood estimation (Allison, 2008). If two outcomes are highly correlated, the omitted outcome is the one that is the least correlated with the other outcome variables and hospital characteristics. For example, total margin and the percentage spent on administration are highly correlated, so only total margin is included.

Table 4-4 presents the marginal probabilities associated with this regression, where negative values indicate a decrease in probability. We observe that characteristics that predict mergers change over the study period. For example, teaching status increases the probability of merging in 1999 but decreases the probability of merging in 2001. More generally, teaching status, urban or suburban status (as opposed to rural) and total beds significantly affect the probability of merging in multiple years. The coefficients on included quality measures are rarely significant, but this does not necessarily imply that the HSRC did not merge poorly performing hospitals. If poorly performing hospitals were generally merged with better performing hospitals, then quality measures may be poor predictors of merging overall.

TABLE 4-4. PROBABILITY OF MERGING BY YEAR

	1997	1998	1999	2000	2001
Teaching (%)	0.66	7.83	11.34*	-233.53	-28.00**
	(6.77)	(11.24)	(4.59)	(190.69)	(10.57)
Urban (%)	6.18	18.78	57.24***	65.19*	38.79
	(6.46)	(10.28)	(15.50)	(31.89)	(21.98)
Suburban (%)	-38.19**	-4.04	57.14***	56.29	12.45
	(13.94)	(9.15)	(17.35)	(30.55)	(17.41)
Mean comorbidity	0.02	-1.23	-0.07	1.05	0.51
	(0.44)	(1.08)	(1.14)	(0.94)	(0.54)
HSMR	-0.03	-0.77*	-0.65	0.84	-0.59
	(0.09)	(0.34)	(0.95)	(0.73)	(0.56)
Mean ALC LOS	0.17	-0.45	0.32	0.10	-0.22
	(0.29)	(0.59)	(0.29)	(0.36)	(0.36)
Total beds (10s)	0.23*	0.13	0.36	-0.36	0.16
	(0.10)	(0.30)	(0.25)	(0.37)	(0.27)
Total margin (%)	0.11	-0.05	0.04	0.08	0.08
	(0.12)	(0.15)	(0.09)	(0.10)	(0.07)
Pseudo-R^2	0.27	0.12	0.41	0.27	0.46
F	441.59	19.80	243.52	226.89	253.91
Observations	109	136	113	108	106

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Estimated via probit. Marginal probabilities and robust standard errors reported (in parentheses). Variable definitions in Table 3-1. Results are highly similar whether characteristics are from 1996 (pre-merger period) or from the year previous the merger year. Characteristics from 1996 are used, as these would have been available in the beginning of the Hospital Services Restructuring Commission.

After matching, we use balancing tests, or bias testing, to determine if the matched sample is more similar to the merged hospitals than the unmatched sample. Bias is the percentage difference in the values of a characteristic between the treated and control group.

TABLE 4-5. BALANCING TESTS FROM MATCHING

Variable	Sample	Mean			
		Treated	Control	% Bias	% bias reduction
Teaching (%)	Unmatched	28.57	8.82	69%	84%
	Matched	28.57	25.41	11%	
Urban (%)	Unmatched	66.67	23.53	65%	83%
	Matched	66.67	74.05	-11%	
Suburban (%)	Unmatched	4.76	31.37	-559%	85%
	Matched	4.76	8.64	-82%	
Mean comorbidity	Unmatched	18.69	17.35	7%	75%
	Matched	18.69	19.02	-2%	
HSMR	Unmatched	100.04	104.62	-5%	94%
	Matched	100.04	99.76	0%	
Mean ALC length of stay	Unmatched	8.37	8.68	-4%	13%
	Matched	8.37	8.64	-3%	
Total beds (10s)	Unmatched	57.75	9.19	84%	85%
	Matched	57.75	50.49	13%	
Total margin (%)	Unmatched	2.97	24.85	-737%	68%
	Matched	2.97	9.96	-235%	

Notes: Variable definitions in Table 3-1. To make the balancing tests more analogous to the estimation of the outcome equation, observations are weighted based on pseudo-hospital volume.

From Table 4-5, we can see that bias in the characteristics that predict merging (teaching status, total beds, the HSMR and urban/suburban status) is dramatically reduced. This table also shows that merged institutions differ significantly from all never-merged in characteristics endogenous to merging so that the sample of all never-merged hospitals is likely not the appropriate control group.

4.4.2 Outcomes

The main body of this paper presents the results obtained when four matches are used to construct each synthetic control hospital. Table A-1 and Table A-2 of Appendix A show the treatment effects obtained using various numbers of matches (one through six).

4.4.2.1 Patient outcomes

As Table 4-6 shows, merged hospitals experience a relative decrease in the HSMR and ALC length of stay, whether they close acute care services at one or more sites at the time of merger.[16] Both findings indicate a relative increase in quality. The HSMR is the ratio of the number of actual deaths in the hospital to the number of expected deaths in the hospital, based on a logistic regression using data from all sites in the study. ALC

---

[16] This table must be read differently from a traditional differences-in-differences table in that *Post* is equal to 1 for both the treatment group and the matched control group in the post-merger period, and therefore the coefficient on *Post* is not a part of the treatment effect. This is explained fully in Section 4.3.2.

length of stay reflects the amount of time patients spend in the hospital after acute treatment ends, and decreasing this measure was a goal of the HSRC (Sinclair et al., 2005).

TABLE 4-6. NON-CONDITION SPECIFIC PATIENT OUTCOMES

	HSMR	Average ALC length of stay	Average distance travelled
Merged	121.04*	10.98	-1.62
	(56.91)	(10.65)	(9.47)
Merged with Closure	-74.52***	-14.47***	-3.40
	(21.83)	(2.61)	(1.97)
Post (for Treatment or Match)	6.85**	1.29**	0.64
	(2.12)	(0.42)	(0.42)
Post * Merger	-9.53***	-1.09*	-0.86*
	(2.38)	(0.49)	(0.37)
Post * Merged with Closure	0.47	-0.01	0.42
	(1.30)	(0.61)	(0.50)
Mean outcome	94.78	9.19	16.36
R^2	0.45	0.72	0.96
F	15.04	49.71	218.17
Observations	504	504	504

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Proportion weights based on pseudo-hospital volume are used in estimation.

Recall that in this extended-DID specification Post = 1 for merged hospitals AND their matched control in the post-treatment period. It should not be interpreted as a part of the treatment effect.

For mergers involving acute care service closures, there is a relative decrease in the distance travelled. This may be because some patients that would have attended the site in the absence of the closure attend another hospital network. The hospital outcomes, presented in the next subsection, are consistent with this result.

As for condition-specific measures, Figure 4-1 shows that most effects are not significant (the corresponding coefficients and standard errors can be found in Table A-4 of the Appendix). There is some evidence that indirectly-standardized mortality increases in hospitals without an acute care service closure. Though we control for comorbidity, this may still be because patients admitted to merged hospitals have rising comorbidity scores and are increasingly admitted by ambulance over the post-merger period; in fact, when we re-estimate these outcome effects using only patients admitted by ambulance (see Table A-3 of the Appendix), the increase in sepsis mortality disappears.

There is a relative decrease in condition-specific length of stays at merged hospitals without an acute care service closure, but this is only significant for pneumonia. As mentioned earlier, the relationship between length of stay and quality is not clear.

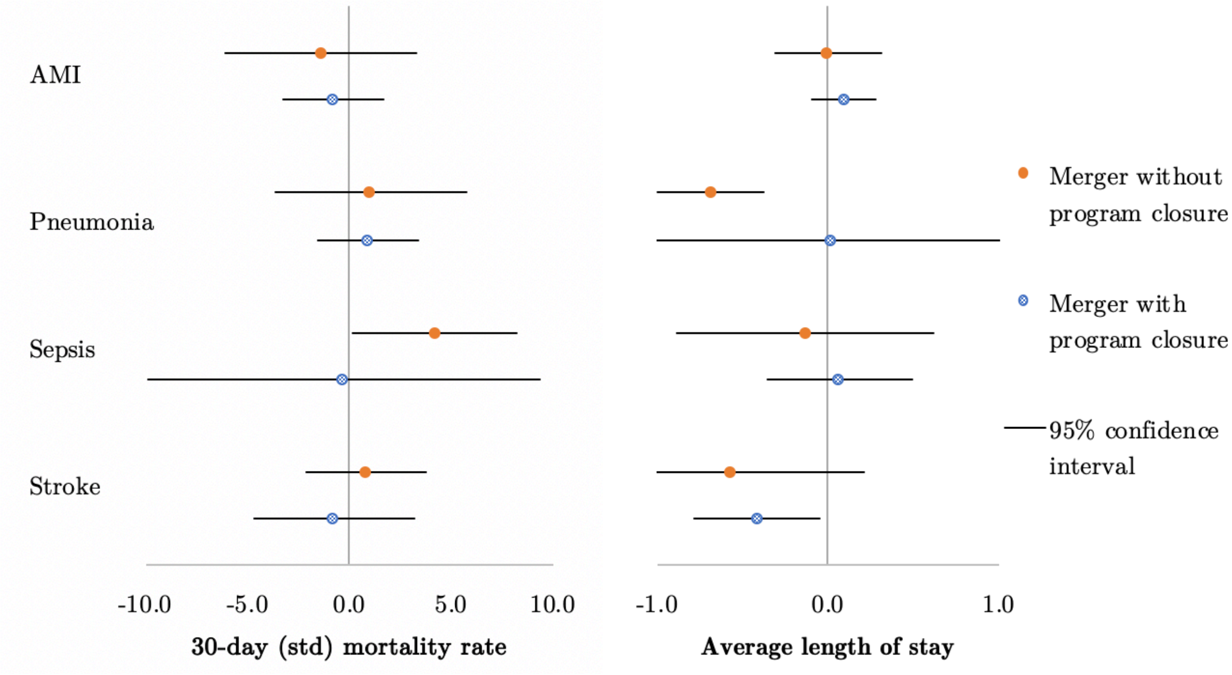


Figure 4-1. Condition-specific patient outcome regression results
Note: The coefficients used to construct this figure can be found in Table A-4 of the Appendix.

It should be noted that condition-specific outcomes have high variance and even tend to vary within hospitals from year to year, which makes it difficult to identify an effect. However, using directly standardized mortality rates or crude mortality rates, both of which have lower variance, provides similar results (see Table A-4 of Appendix A for results, and Appendix C for a review of standardization methods).

Since we use hospital records that were generated immediately before and immediately after merging, there is some concern that outcomes such as mortality and length of stay could be affected while hospitals relocate and reorganize hospital programs. To test this, we recreate our dataset omitting patient records generated within merging hospitals within a 6-month radius of merging. The results, contained in Table A-5 of the Appendix, are highly similar.

4.4.2.2 Hospital outcomes

Table 4-7 shows the results for the hospital outcomes. For both types of mergers, there is a relative decrease in total compensation paid to UPP. This is the only significant change for mergers not involving the closure of acute care services at one or more sites. Relative to merged hospitals without acute care service closures, hospitals with acute care services closures experience a significant decrease in volume and total beds, and a significant increase in total margin—the financial surplus/deficit as a percentage of revenue. This is consistent with theory and evidence that hospitals that physically restructure fare better in terms of financial outcomes (Weil, 2010).

The decrease in volume observed in some merged hospitals is not necessarily negative and should not be confused with a decrease at the individual hospital sites. In the case of a merger involving the closure of acute care services at one or more sites, volume could decrease at the hospital level but could remain the same or even increase at the individual hospital sites.

TABLE 4-7. HOSPITAL OUTCOMES

	Occupancy	Total Beds (10s)	Volume (10s)	Compensation to UPP ($00,000)	% on admin	Total margin
Merged	-41.29	-44.41	-409.95	1329.06	-3.65	4.08*
	(31.96)	(40.01)	(960.14)	(2322.82)	(1.94)	(41.54)
Merged w/Closure	-50.88***	-19.38	-1798.46***	-3164.36***	-2.06***	5.62
	(12.91)	(15.86)	(235.65)	(458.77)	(0.87)	(11.82)
Post	4.18*	3.77	164.96**	262.70**	0.09	2.54
	(1.95)	(4.98)	(55.65)	(106.23)	(0.18)	(2.11)
Post * Merged	-91.00***	-0.90	-72.52	-558.74	0.21	-4.65
	(1.84)	(4.13)	(40.41)	(87.95)	(0.17)	(2.65)
Post * Merged w/Closure	-2.33	-12.01*	-517.03***	40.27***	-0.05	7.80*
	(2.39)	(5.05)	(69.94)	(67.08)	(0.25)	(3.27)
Mean outcome	86.98	64.52	2691.80	1845.96	5.40	-14.91
R^2	0.74	0.88	0.98	0.93	0.64	0.82
F	30.20	129.42	740.00	139.33	23.99	88.04
Observations	504	504	504	504	504	504

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Proportion weights based on pseudo-hospital volume are used in estimation. Recall that in this extended-DID specification Post = 1 for merged hospitals AND their matched control in the post-treatment period. It should not be interpreted as a part of the treatment effect.

There are several reasons why the decrease in volume could occur. Patients who would have visited the hospital may now visit other hospitals, either because they now prefer a competitor or because the merged hospital no longer has enough capacity to serve them. The result is also complicated by the fact that hospitals over the post-merger period are increasingly treating patients on an outpatient basis (i.e. without assigning them to a bed) (Sinclair et al., 2005), meaning that these patients do not appear in our dataset. If the hospitals that physically restructured moved to outpatient care for the treatment of urgent conditions more quickly than the matched controls, it would be reflected in a relative decrease in volume, as we observe.

Though we cannot directly test these hypotheses, we find some evidence that outpatient care is increasing in merged hospitals faster that it is in never-merged hospitals. First, there is no rise in the occupancy rate at merged hospitals, indicating that capacity constraints do not cause the fall in volume. On average, both merged and never-merged hospitals experience a decrease in volume (relative to 1994) over the study period, suggesting that mergers do not simply reallocate patients from merged to never-merged hospitals. Diving deeper, we locate the merged hospitals with persistent decreases in volume and find the three closest never-merged hospitals. As Figure 4-2 shows, these hospitals also experience a decrease in volume.

In addition, if we repeat our estimation strategy and use the ADG Score (a measure of comorbidity) and the percentage of patients admitted by ambulance, we find that merged hospitals experience an increase in these measures relative to never-merged hospitals. This suggests that patients with less severe illnesses may increasingly be treated using outpatient care.

Altogether, it appears that conversion to outpatient care could play a role in our result, but this is difficult to investigate further given that we do not possess outpatient data. We include these comments simply to argue that the decrease in volume cannot easily be categorized as a benefit or drawback or mergers.

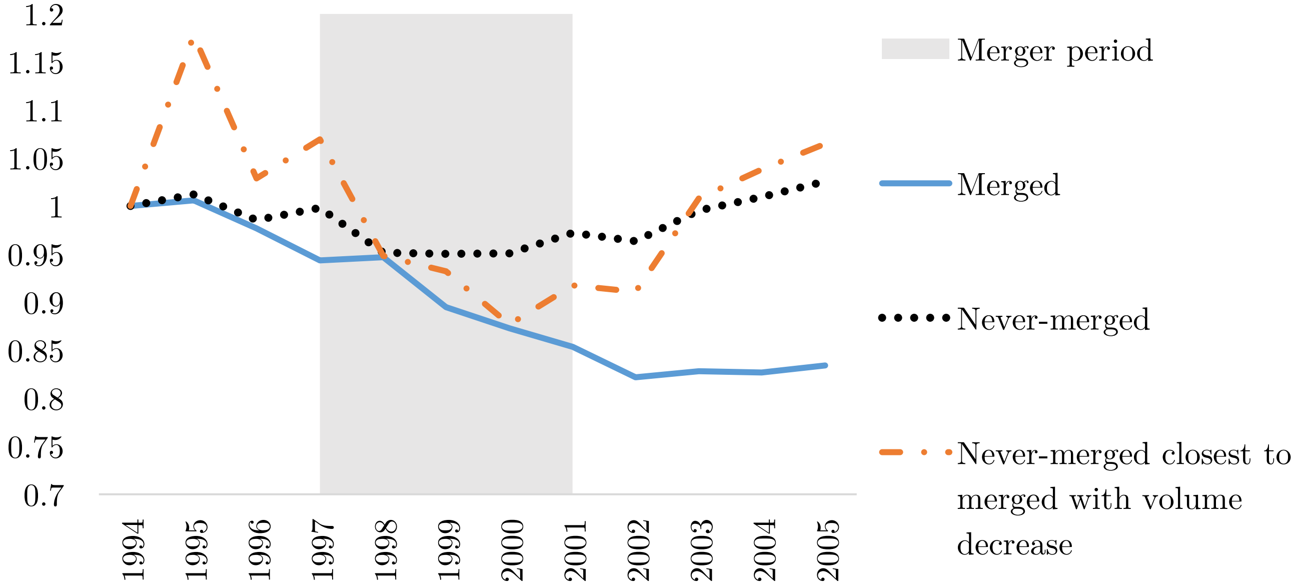


Figure 4-2. Changes in average volume by hospital type, discharges, 1994-2005

4.4.3 Comparison to full panel differences-in-differences

For a comparison to full-panel difference-in-differences, we estimate a simplified version of the outcome equations, by including all never-merged hospitals as the control group. The equation is as follows:

$$y_{it} = \alpha + \beta_1 \text{Treatment} + \beta_2 \text{Closure} + \beta_3 \text{Post}_{it} + \beta_4 \text{Post}_{it} \times \text{Closure} + \eta \text{Year} + \mu_i + e_{it} \qquad \text{(Eq. 3)}$$

In this specification, $Post_{it}$ equals 1 for merged hospitals in their post-merger period and is always equal to 0 for never-merged hospitals.

TABLE 4-8. HOSPITAL EFFECTS IN TERMS OF COEFFICIENTS FROM (3)

	Total effect pre-merger	Total effect post-merger	Treatment effect
Never-merged hospital	α	α	-
Merged hospital	$\alpha + \beta_1$	$\alpha + \beta_1 + \beta_3$	β_3
Merged hospital w/closure	$\alpha + \beta_1 + \beta_2$	$\alpha + \beta_1 + \beta_2 + \beta_3 + \beta_4$	$\beta_3 + \beta_4$

We use the propensity scores estimated earlier, but by incorporating them into the analytical weights used during estimation. The initial analytical weight for a merged hospital is given as:

$$\frac{1}{ps_M} \tag{Eq. 4}$$

The initial analytical weight for a never-merged hospital is given as:

$$\frac{1}{1 - ps_N} \tag{Eq. 5}$$

Since propensity scores are less than 1, this means that merged hospitals with higher propensity to merge receive less weight than merged hospitals with a lower propensity to merge. In other words, merged hospitals that are more similar to never-merged hospitals are weighted up. Similarly, a never-merged hospital with a high propensity to merge receives more weight—never-merged hospitals that are similar to merged hospitals are weighted down. To make this analysis as similar to the main analysis as possible, these initial score-based weights are combined with the volume-based weights used in the main analysis.

Calculating trends using these weights, we can get a sense of the impact of the weighting. Using the weights makes the treatment and control group more similar to each other, but the reduction in bias is noticeably lower for suburban status and total beds, both of which are predictors of merging.

TABLE 4-9. BIAS TESTS FROM MATCHING, SIMPLE MATCHING

Variable	Sample	Mean			% bias
		Treated	Control	% Bias	reduction
Teaching (%)	Unmatched	28.57	8.82	69%	85%
	Matched	16.57	14.82	11%	
Urban (%)	Unmatched	66.67	23.53	65%	83%
	Matched	42.99	38.22	11%	
Suburban (%)	Unmatched	4.76	31.37	-559%	56%
	Matched	7.50	26.02	-247%	
Average comorbidity	Unmatched	18.69	17.35	7%	85%
	Matched	18.02	17.83	1%	
HSMR	Unmatched	100.04	104.62	-5%	53%
	Matched	100.94	103.13	-2%	
Average ALC length of stay	Unmatched	8.37	8.68	-4%	-170%
	Matched	7.81	8.59	-10%	
Total beds (10s)	Unmatched	57.75	9.19	84%	14%
	Matched	43.97	12.29	72%	
Total margin (%)	Unmatched	2.97	24.85	-737%	86%
	Matched	9.84	20.20	-105%	

Notes: Variable definitions in Table 3-1. Characteristics are weighted based on propensity scores and volume.

Since analysis is at the pseudo-facility level, the weights must be aggregated for merged hospitals. The score-based weights are aggregated first and then combined with volume-based weights calculated at the pseudo-hospital level. Appendix A contains the full results of this estimation (Tables A-7 through A-9). A few results change, which is not surprising as this method is not likely to control for differences between merged and never-merged hospitals. The decrease in the HSMR is no longer significant, but there is a significant decrease in the standardized mortality rate for AMI patients. The increase in total margin in hospitals with acute care service closures is larger and more significant than when using our main matching strategy.

4.4.4 Common trends test

Since hospital mergers are announced ahead of time, there is some concern that anticipation effects could contaminate the matching process and the treatment effect. For example, if merging hospitals do not invest as much in quality in the pre-merger period (perhaps because they know some of their programs will be reorganized or closed) then quality could deteriorate and then correct after the merger, potentially causing a positive treatment effect to be identified. To investigate anticipation effects, we estimate the following equation:

$$\begin{aligned} y_{it} = {} & \alpha + \beta_1 \text{Merged} + \beta_2 \text{Merged with Closure}_t + \beta_3 \text{Post}_{it} + \beta_4 D_{it} \\ & + \beta_5 D_{it} \times \text{Merged with Closure} + \eta \textbf{Year} + \gamma_1 \text{Merger year}_{it} + \gamma_2 \text{Merger year-1}_i \\ & + \gamma_3 \text{Merger year}_{it} \times \text{Treatment} + \gamma_4 \text{Merger year-1}_{it} \times \text{Treatment} \\ & + \gamma_5 \text{Merger year}_{it} \times \text{Closure} + \gamma_6 \text{Merger year-1}_{it} \times \text{Closure} + \mu_{it} + e_{it} \end{aligned} \qquad \text{(Eq. 6)}$$

This is the same as equation 2 (used in the main specification) except the terms associated with the coefficients γ_1 through γ_6 are added. Merger year_{it} is equal to 1 if 1) the record is for a merged hospital in its merger year, or 2) the record is for a control hospital in a year in which the corresponding merged hospital is in its merger year. Similarly, Merger year-1_{it} is equal to 1 if 1) the record is for a merged hospital in the year prior to its merger year, or 2) the record is for a control hospital in a year prior to which the corresponding merged hospital is in its merger year. In this equation, γ_3 and γ_4 capture differences in the trend between the two years preceding the post-merger period and the other pre-period years, and γ_5 and γ_6 capture additional effects for hospitals with acute care service closures.

Appendix A contains the full results of this estimation (Tables A-10 through A-12). There are no significant estimates of γ_3, γ_4, γ_5, or γ_6 for any of the outcome equations. The significances of the treatment effects generally do not change, though the p-value associated with the total margin result increases from 0.018 to 0.053. Overall, it does not appear that our results are caused by anticipation effects.

4.5 Conclusion

Despite mixed evidence surrounding hospital mergers in public systems, governments continue to mandate hospital mergers. We test the claim that hospital mergers can improve quality and costs using a wave of hospital mergers occurring in Ontario between 1997 and 2001.

We examine a large set of quality indicators related to patient and hospital outcomes. Consistent with literature in the U.S. and U.K., we do not find significant effects on all quality measures, though in our case we do find a relative decrease in the hospital-standardized mortality ratio and the mean ALC length of stay, both indicators of an increase in quality. Also consistent with the literature in the U.K., volume in merged hospitals decreases relative to never-merged hospitals, but we find that this volume decrease is associated with merged hospitals that close acute care services at one or more sites at merger.

We add to existing literature by examining the differences between mergers that are more administrative and mergers that involve the closure of acute care services at one or more hospital sites. We find that physical restructuring of this nature does not lead to a greater increase in quality in merged hospitals. However, mergers involving physical restructuring are associated with an improvement in financial performance relative to mergers without physical restructuring.

Our results suggest that hospital mergers have the potential to improve quality but may not improve financial outcomes unless physical restructuring is involved. We also observe signs of decreased capacity (a reduction in compensation paid to UPP for both types of mergers and a decrease in volume for mergers involving acute care service closures), that are difficult to investigate further using our data set. We argue that these changes are consistent with a conversion to outpatient care in merged hospitals and recommend that this finding be investigated further before the Ontario government proceeds with further mergers involving the closure of acute care services.

Chapter 5

The Impact of Restructuring on Patient Choice and Welfare

5.1 Introduction

This chapter of the thesis uses patient-level data to further evaluate the Ontario merger wave by examining the impact of the mergers on patient welfare. The previous chapter presents evidence that the Ontario hospital mergers increased hospital quality and improved financial status for some hospitals, but this may not translate to an increase in welfare for all patients. The most obvious reason is because when mergers involve site closures, some patients have to travel farther to access medical care.

In the short-run, hospital mergers that are accompanied by service closures only impact patients through the removal of choices, the welfare impact of which can only be negative. In the long-run, hospitals can reallocate resources, and mergers may have a positive welfare impact if the welfare impact of that reallocation outweighs the welfare impact from the removal of choices. This chapter provides evidence on the short-run and long-run effects of the mergers. Specifically, we focus on three questions. First, how are patients impacted in the short-run, given that hospital restructuring involving closures removes choices? Second, how are patients impacted in the long-run, where hospital quality has adjusted as a result of the mergers? Third, is the welfare impact of hospital mergers larger for patients living in rural areas where choices are already limited?

To answer these questions, we use data on the Ontario merger wave occurring from 1997 through 2001. Unlike in the previous chapter, outcomes are calculated at the level of the hospital site, rather than the level of the hospital. This is because patients choose between hospital sites, not hospitals. In total, we examine 22 mergers involving 66 hospital sites and the closure of acute care at 12 hospital sites.[17] An attractive feature of this merger wave is that the merging hospitals are geographically diverse,

---

[17] There is one more merger than in the previous chapter because in the previous chapter, a hospital was excluded for closing a site several years after merging; that hospital is not excluded in this chapter. In addition, since this chapter analyzes data at the level of the hospital site and not at the level of the hospital, there are slightly more units (66 versus 61) than in the last chapter because some hospitals have multiple sites even prior to mergers. As a reminder, some of our "closed" hospital sites still provide some healthcare services (e.g., rehabilitative services) but all acute care programs are closed, and the hospital sites no longer appear in our data set.

allowing us to speculate about the differences in effects between urban, suburban and rural markets.

One of our main empirical challenges is that the mergers occur over several years, and it may take additional time after merging for quality changes to take effect. During this long time period, patient demographics will inevitably change, making it difficult to compare welfare calculated in the pre-period to welfare calculated in the post-period. In addition, the late 90s marks a conversion to outpatient care, which occurs when patients are treated without being assigned to a bed (Sinclair et al., 2005). Since patients treated using outpatient care do not appear in our data, the number of patients we do not observe is increasing over time. To overcome these challenges, we estimate a patient utility model using techniques proposed by McFadden (1974) using only the pre-period data but incorporate the post-merger characteristics of the hospitals to compute our counterfactuals. In the model, given the set of hospital sites available within reasonable travelling distance, patients choose to receive treatment at the hospital site that maximizes their utility. A patient's utility depends on their personal characteristics such as age, sex and health status, but also on the health condition for which they are admitted—the equation is estimated separately for each condition. In addition, patients derive utility from hospital characteristics such as hospital site quality (e.g., mortality rate, specialization) and general attributes (e.g., teaching status). We estimate patient preference parameters for these characteristics and attributes. As our dataset includes distance travelled to the hospital site, we are able to use these parameters to calculate willingness to travel (the extra distance patients are willing to travel to access a hospital site where the measure is one standard deviation higher) for each of the characteristics. We use the patient preference parameter estimates to simulate the utility level of the patients in the post-merger environment. Specifically, we examine two counterfactuals. In each, we use the existing preference parameters to find the new best option for each patient and recalculate the level of patient welfare. In the first counterfactual, the hospital sites removed by the merger wave are no longer in the choice sets, and the characteristics of the remaining hospital sites are those of the pre-merger period. In the second, the closed hospital sites are still removed, but the characteristics of the hospital sites are those of the post-merger period. In the first case, only mergers involving closures will have an impact, capturing the short-run effect of the mergers. In the second case, mergers not involving closures also have an impact. This captures potential resource reallocation that may occur in the long-run. For both counterfactuals, we estimate heterogeneous effects for urban, suburban and rural patients.

Another empirical challenge is the presence of idiosyncratic shocks to patient utility that are unobservable in the data. These shocks may be important determinants of utility for some patients, but the shocks are not identifiable at the individual level.

The optimal choice for a patient based on the observable components of the utility function (hereafter referred to as the observable best hospital site) may not correspond to the patient's actual choice observed in the data (hereafter referred to as the choice hospital site). When this is the case, constructing the counterfactual without taking the shock into account will cause the patient to be reallocated from the choice hospital site to the observable best hospital site, resulting in a mechanical welfare increase that is not due to the hospital mergers. Therefore, failing to account for this unobservable utility component leads to upward bias when simulating the welfare change. This has a larger impact in our second counterfactual, since we must re-optimize the choices of all patients, as opposed to only the patients who have their choice removed by the closures. For each hospital site-patient pair where the hospital site was not chosen (hereafter, the alternative hospitals), we draw a shock from the Type I extreme value distribution, and then for the choice hospital, we draw a shock from a portion of the Type I extreme value distribution such that the utility level from the choice hospital site is greater than the utility from all alternatives.

By controlling for idiosyncratic shocks and comparing our counterfactuals, we can speculate on the short- and long-run consequences of hospital mergers involving closures. First, we estimate our patient preference parameters for each of our four conditions and find that patients dislike distance and prefer a hospital site that does not have teaching status, is larger in terms of volume, and is more specialized in treating the condition for which the patient is admitted. We also find important differences between patient groups. For example, rural patients are less sensitive to distance and older patients are more sensitive. Using these preference parameters, the aggregate change in patient welfare associated with removing the hospital sites from the choice set is between -116.37% and -25.88% depending on the condition. The utility change from removing the hospital sites and adjusting the hospital quality measures is between -11.35% and 122.09% depending on condition.

At the individual level, the welfare impacts for patients vary by region and condition in the short- and long-run. For example, compared to urban patients, a greater proportion of rural patients are harmed in both the short- and long-run. These results suggest that resource allocation caused by restructuring involving closures can increase patient welfare on the average in the long-run, but whether this is true varies by subgroup.

This paper contributes to the literature using patient choice modelling to estimate hospital demand and in particular, to the small literature using these models to calculate changes in patient welfare, as discussed in Chapter 2. We contribute to this literature by applying the model to the Canadian setting and by estimating the distribution of unobservable shocks to the patient choice model, allowing us to control for these shocks in our counterfactuals. Though our method differs, this is similar to the method used in the context of the US daily newspapers market in Fan (2013). Another contribution is that in contrast to previous studies, we estimate a long-run welfare impact by using data from the period following a merger wave. We combine these post-merger data and two separate counterfactuals to calculate measures of patient welfare in the short- and long-run, using our diverse set of hospitals to draw conclusions about heterogeneous impacts on urban, suburban and rural patients.

The remainder of the paper proceeds as follows. Section 5.2 details the methodology used to estimate the impact of the mergers, and section 5.3 describes the data. Finally, section 5.4 describes the results, and section 5.5 concludes.

5.2 Methodology and model

To analyze the impact of mergers, we use a patient choice model based on McFadden's (1974) conditional logit analysis of choice behavior. This model requires patient level data. We estimate this model separately for each of our four conditions: AMI (heart attack), pneumonia, sepsis, and stroke.

Given that a patient is seeking treatment in an Ontario acute care hospital site, the patient chooses to be treated at the hospital site in his choice set associated with the highest ex-ante utility. Each hospital site has a set of characteristics that influence the level of utility received by the patient. The level of utility is also influenced by the patient's own characteristics and their distance from the hospital site.[18] This gives us the following utility equation for patient *i* receiving treatment at hospital site *j*:

$$\begin{aligned} u_{ij} &= V_{ij} + \varepsilon_{ij} \\ &= Q_j\beta_i^q + X_j\beta_i^x - D_{ij}\beta_i^q + \varepsilon_{ij} \qquad \text{with } i = 1, \ldots., I \quad \text{and} \quad j = 1, \ldots, J \end{aligned} \tag{Eq. 7}$$

where Q_j is a vector of hospital site quality measures that the patient may consider when choosing where to receive treatment. Some quality measures as specific to the condition experienced by the patient (e.g., the mortality rate for that condition) while other quality measures are not (e.g., the overall alternate-level-of-care length of stay). All quality measures are calculated at the level of the hospital site and are lagged. In other words, a patient seeking treatment for pneumonia will have generic lagged quality measures and lagged quality measures specific to pneumonia in their utility function, but not lagged quality measures specific to other conditions. Quality measures are lagged because it may take patients some time to learn about changes in quality. Additionally, the patients admitted during a given year impact the quality measures for that year, so that using current quality measures could cause endogeneity problems. The vector X_j consists of observed time-invariant hospital site characteristics such as hospital

---

[18] We do not observe the exact location of patients when they choose to visit the hospital site. As a proxy for distance travelled, we use the distance between the home of the patient and the hospital site. The home address of the patient is generally verified at the hospital site and will be up to date.

site location (urban, rural, suburban) and whether the hospital site has teaching status. These time-invariant characteristics are current as opposed to lagged since the composition of patients admitted during the period do not impact these characteristics. In our setting, since patients do not pay for hospital services received, distance travelled to the hospital is the observable cost for the patient to receive treatment.[19] This is given by the vector D_{ij}. Distance is specified as a vector because polynomial terms are included, a specification which we discuss further in the *Estimation and Results* section. Lastly, ε_{ij} represents the idiosyncratic preferences of patient *i* for hospital site *j*, which are unobservable to the researcher. For example, preferences caused by relationships with staff or physicians would be captured in ε_{ij}. In the case where a patient is far from home (e.g., on vacation) at the time of admission, this will also be reflected in ε_{ij}. For simplicity of exposition, we will refer to observable part of the patient utility function as V_{ij}.

Finally, the patient chooses the hospital site that provides the highest utility. If we assume that the idiosyncratic component, ε_{ij}, is independently and identically distributed according to the Type I extreme value distribution, we can compute the probability that patient *i* chooses hospital site *j*:

---

[19] Wait times are another cost, but they are not available in our data.

$$P_{ij} = \frac{exp(V_{ij})}{\sum_k exp(V_{ik})} \qquad \text{(Eq. 8)}$$

where the denominator is a sum of utility from observables over all alternative hospital sites in the choice set of the patient, $k \in J$. In theory, all patients could seek treatment at any hospital site in Ontario. Since the patients in the sample used for our estimation have conditions that require immediate treatment, we reduce the size of the choice set so that patients can travel to each hospital site within the set in a reasonable amount of time. If the assumptions of the model are correct, then eliminating irrelevant choices does not affect the results.

Summing over all patients with hospital *j* in their choice set, we obtain P_j which can be interpreted as hospital *j*'s expected market share.

$$P_j = \int_i P_{ij} dF(\beta) \qquad \text{(Eq. 9)}$$

where β is the vector of coefficients of the patient utility function. The log-likelihood for estimation is then given by

$$\ln L = \sum_k \ln(P_j) = \ln\left(\frac{exp(V_{ij})}{\sum_k exp(V_{ik})}\right) \qquad \text{(Eq. 10)}$$

The basic model specification given in equation 7 does not allow for interactions between patient and hospital site characteristics. As in Gutacker et al. (2016), we specify a model that allows preferences to vary across conditions by defining the coefficients according to:

$$\beta_i^n = \beta^n + \alpha^n Z_i \quad \text{where} \quad n = q, x, d \qquad \text{(Eq. 11)}$$

where the β^n coefficients reflect the preferences of the reference patient for quality measures *q*, characteristics *x* and distance term *d* and Z_i is a vector of observable patient characteristics. In our case, the vector of observable patient characteristics, Z_i, will include patient age, sex, comorbidity (captured by the ADG Score, described in Appendix B) and rurality indicators.

Once the patient preference parameters are estimated, we can compute willingness to travel (*WTT*) for the included quality measures. This measure, a measure analogous to willingness to pay, informs us on the extra distance patients are willing to travel to receive treatment at a hospital of better quality. *WTT* is computed as follows:

$$\begin{aligned} \text{WTT} &= \left.\frac{\partial d_{ij}}{\partial Q_{ij}}\right|_{u_{ij}} SD(Q) = \frac{-\,\partial u_{ij}\,\partial d_{ij}}{\partial Q_{ij}\,\partial u_{ij}} SD(Q) \\ &= \frac{-\beta_i^q}{\beta_i^{d1} + 2\beta_i^{d2}\mu_d + 3\beta_i^{d3}\mu_d^2} SD(Q) \end{aligned} \qquad \text{(Eq. 12)}$$

where *SD(Q)* is the one standard deviation increase in quality and μ_d is the average distance travelled by patients to their choice hospital. *WTT* is computed with respect to each quality measure used in estimation.

Finally, we can construct our counterfactuals and compute the change in patient utility between reality and the counterfactual scenarios. We use two counterfactuals. In the first, the closed hospital sites are removed from the choice set. In the second, the closed hospital sites are removed from the choice set and the hospital sites are assigned their post-merger quality characteristics. For the post-merger quality characteristics, we use the values from 2005, four years after the end of the merger wave. Using data from 2005 allows time for the characteristics to adjust but avoids policy contamination from the restructuring of the DHCs in 2007 (Gardner, 2006).

To compute the change in patient utility for each counterfactual, we use our model to calculate the predicted utility for each patient given their choice of hospital site. We then use our estimated preference parameters to find the best choice of the patient in the counterfactual scenario, and calculate the new predicted utility based on that choice. These calculations take utility from unobservable factors into account—we leave detailed discussion of our method to do so for the *Estimation and Results* section. For each patient, we can compare the ex-ante utility from the real healthcare environment to the ex-ante utility from the counterfactual environment. Aggregating over all patients gives the change in welfare for the entire economy (i.e. the province of Ontario).

$$\Delta = \sum_i \delta_i = \sum_i [u_{is} - u_{ic}] \qquad \text{(Eq. 13)}$$

where u_{ic} is the utility level associated with the actual choice of a patient while u_{is} is the utility in the counterfactual environment. Summing over all individual patient welfare changes, δ_i, we obtain the total welfare change, Δ, for the entire economy.

5.3 Data and descriptive statistics

Compared to Chapter 4, patient information is used more extensively in this paper. Our dataset includes demographic information on patients such as age, gender, longitude and latitude of residence, and whether the residence is located in an urban, suburban or rural area. It also includes health information such as the admission and discharge dates, the condition for which the patient is treated, comorbidity status, whether arrival to the hospital was by ambulance, and to which hospital the patient was admitted. Using these patient records, we calculate the approximate distance travelled to the chosen hospital by taking the geodesic distance (i.e. the shortest distance between two points) between the residence of the patient and the location of the hospital. We then exclude all patients where the distance calculated exceeds 300 km. In these cases, it is likely that the patient was far from home when making their choice, and the calculated distance is therefore not correct. Some of these cases will also be dropped when we define the choice sets.[20]

We estimate preference parameters over variables that are similar to those as used in Chapter 4 and described in Table 3-1, with the main difference being that the variables are calculated at the level of the hospital site instead of the level of the

---

[20] This is because for a patient to be included in the estimation, their choice must be in the choice set. For example, if the choice set is specified as the closest 10 hospital sites, patients who did not choose one of the closest 10 hospital sites will be dropped.

hospital. Attributes include an indicator for whether the hospital site is a teaching institution, and the hospital network to which the hospital site belongs. The quality measures that are not specific to a condition are the hospital site-standardized mortality ratio, the average alternate-level-of-care length of stay, the volume of the hospital (discharges) and the annual high-risk occupancy rate. For the annual high-risk occupancy rate, we include the patients with conditions responsible for 80% of hospital deaths—the list is provided by the Canadian Institute for Health Information (2016) for the purpose of calculating standardized mortality ratios. For this study, which concerns emergency conditions, this is a more accurate reflection of hospital desirability for these patients than is the overall occupancy rate, which includes patients with non-emergency conditions. We also calculate in-sample condition-specific quality measures: 30-day standardized mortality, 30-day standardized readmission (exclusive to this chapter), and specialization in the condition of admission.[21]

Data are available from 1994 onwards, and since we use lagged quality measures in our model and the merger wave begins in 1997, this means that we estimate the model using patients admitted in 1995 and 1996. In our second counterfactual, we use hospital attributes and quality measures from 2005.

---

[21] In a sensitivity analysis, condition-specific length of stay is included. The results are contained in Appendix A.

5.3.1 Descriptive statistics

5.3.1.1 Patients

Table 5-1 shows the characteristics of the patients during the pre-merger period (i.e. in the estimation sample) for each of the four conditions. Characteristics are relatively constant across the two years. After removing observations of patients that have missing information and patients who went to hospital sites unexpectedly far from their FSA, the remaining sample consists of 36,269, 50,792, 12,662, and 19,702 patients admitted with AMI, pneumonia, sepsis and stroke. The reference patient admitted with a diagnosis of AMI is a 68.8-year-old male, has 3.4 ADGs (ADG Score of 20.35), has not been diagnosed with another of the conditions in the dataset, and has a length of stay of 9.8 days.[22] The reference patient is admitted once during our sample period and dies or is readmitted within 30 days of discharge 20.1% or 0.6% of the time. 42.0% of AMI patients are admitted via ambulance. The majority of patients admitted for AMI live in urban locations compared to suburban and rural locations (65.6%, 24.0% and 10.3%). The reference patient admitted with pneumonia is almost identical to the reference patient admitted with AMI. This reference patient is a 70.2-year-old male with 4.2 ADGs (ADG Score of 17.1) and a length of stay of 13.9 days. 23.3% of patients died following their discharge and 0.2% were readmitted following discharge. Just as for

---

[22] Length of stay is not included in the main specification but is used in a sensitivity in Appendix A.

AMI, the majority of patients live in urban areas (62.9%) followed by suburban areas (24.9%) and finally rural areas (12.3%). The reference sepsis patient is sicker than the other reference patients. This patient is a 68- year-old male, has 5.5 ADGs (ADG Score of 22.6) at the time of admission, and had a length of stay of 19.2 days. 33% of sepsis patients died and 0.2% were readmitted within the 30 days following discharge. Most patients admitted with sepsis live in urban areas but, relative to the two above mentioned conditions this percentage is much higher at 71.7%. 20.2% of patients with a sepsis diagnosis reside in suburban locations while 8.1% of them reside in rural locations. Just under half (48.9%) of patients arrived via ambulance and 26.7% of sepsis patients had multiple hospital stay during our sample period. Finally, the reference patient diagnosed with a stroke is the oldest at 71.3 years of age. He is a male who stayed in the hospital for 19.6 days and was recorded as having 3.7 ADGs (ADG Score of 19.3) at the time of admission. Just like for the other conditions, the percentage of patients living in urban locations is higher (76.2%) relative suburban and rural locations (17.3% and 6.5%). Just over half (50.8%) of the patients in our estimation sample diagnosed with a stroke arrived at the hospital by ambulance and 27.2% of patients had multiple stays during the sample period.

A very small portion of patients were admitted with more than one of the conditions observable in our data (e.g., a patient was both recorded as having received treatment for pneumonia and stroke). This is more common for sepsis patients, which is to be expected as sepsis can originate from pneumonia (Rautanen et al., 2015). Sepsis also causes organ damage and can cause a dramatic drop in blood pressure, both of which can lead to AMI (Schilling, 1997) or stroke (Rhee, Jones, & Hamad, 2019).

TABLE 5-1. DESCRIPTIVE STATISTICS, PATIENTS 1995-1996, BY CONDITION

	AMI		Pneumonia		Sepsis		Stroke	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
AMI	1.000	0.000	0.042	0.201	0.036	0.185	0.033	0.180
Pneumonia	0.059	0.236	1.000	0.000	0.189	0.392	0.052	0.221
Sepsis	0.012	0.111	0.047	0.212	1.000	0.000	0.016	0.126
Stroke	0.018	0.134	0.020	0.140	0.025	0.156	1.000	0.000
Death (30 days)	0.201	0.401	0.233	0.423	0.334	0.472	0.224	0.417
Readmitted (30 days)	0.006	0.078	0.002	0.040	0.002	0.041	0.004	0.061
Age	68.816	13.191	70.151	16.917	68.038	16.793	71.259	12.491
Admitted by ambulance	0.420	0.493	0.447	0.497	0.489	0.500	0.508	0.500
Distance travelled	0.938	1.512	0.936	1.498	1.028	1.814	1.302	2.504
Female	0.383	0.486	0.484	0.500	0.484	0.500	0.475	0.499
Johns Hopkins ADGs	3.420	1.971	4.212	2.255	5.514	2.489	3.663	2.119
ADG Score	20.335	9.216	17.120	11.712	22.595	11.989	19.329	9.566
Urban	0.656	0.475	0.629	0.483	0.717	0.450	0.762	0.426
Suburban	0.240	0.427	0.249	0.432	0.202	0.402	0.173	0.378
Rural	0.103	0.304	0.123	0.328	0.081	0.272	0.065	0.246
Length of stay	9.831	14.286	13.886	24.493	19.249	32.050	19.581	33.646
# of admissions	1.114	0.344	1.095	0.310	1.153	0.393	1.154	0.400
Multiple admissions	0.206	0.404	0.176	0.381	0.267	0.443	0.272	0.445
# of conditions	1.090	0.305	1.110	0.329	1.250	0.475	1.101	0.330
Observations	36269		50792		12662		19702	

Notes: Death and readmission must be related to the condition of admission and occur within 30 days of the discharge of the patient.

In terms of distance travelled to the chosen hospital site, three of the four conditions have an average distance of about 9 km (9.38 km for AMI, 9.36 km for pneumonia and 10.28 km for sepsis). On average, stroke patients travel farther than the other patients (13.02 km). We discuss this further when the model results are presented.

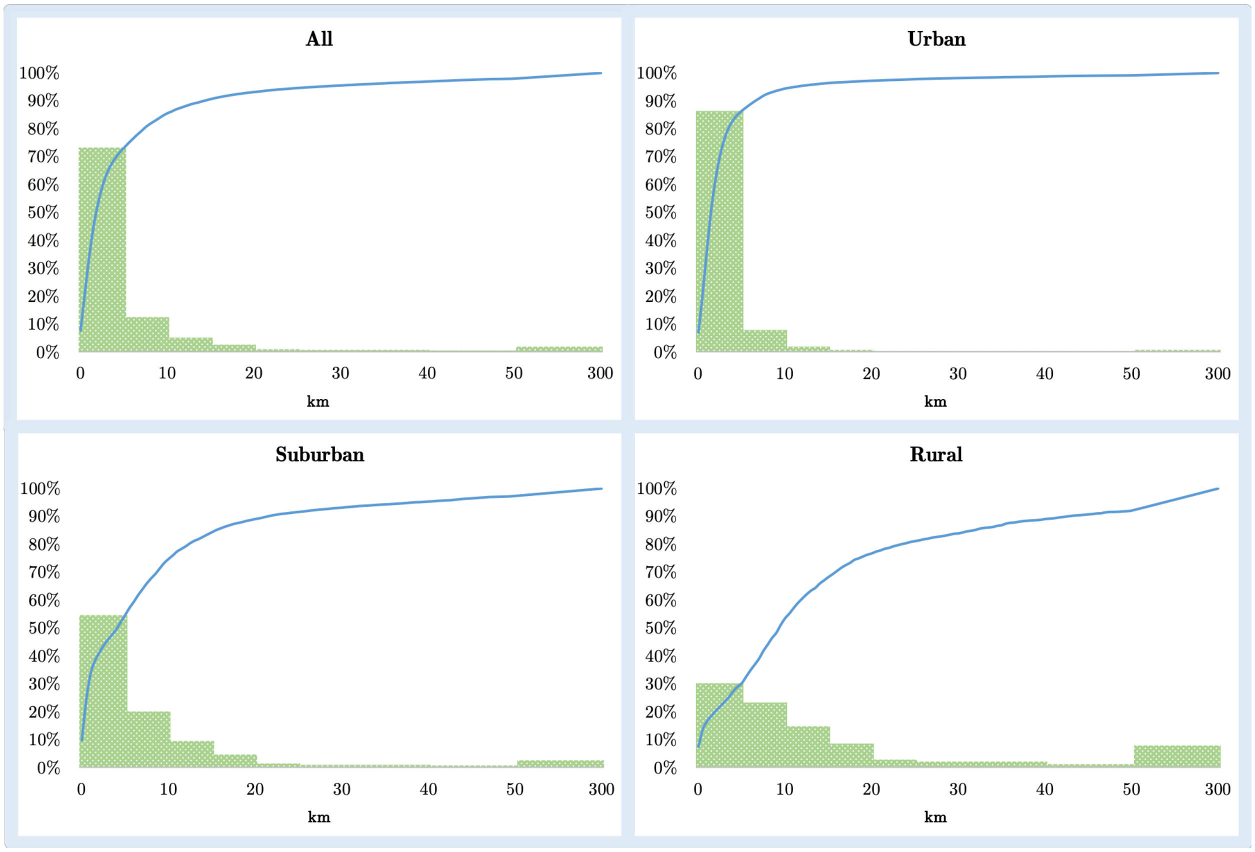


Figure 5-1. Discrete histogram with cumulative frequency, distance between patient residence and their chosen hospital site, by location

Figure 5-1 demonstrates that close to 73% of patients received treatment at a hospital located within 5 km from their residence, and this is mostly driven by patients that live in urban areas. More than 86% of urban patients travelled 5 km or less. That number was only 55% and 30% for patients living in suburban and rural areas respectively. 8%

of rural patients travelled between 50 km and 300 km to their chosen hospital. For urban patients, only 1% of patients travelled such a distance to their choice hospital. 3% of suburban patients travelled between 50 km to 300 km.

From Figure 5-2, 42% of patients in our sample chose to bypass the closest hospital site. Breaking this down by location of residence, the shape of the distribution remains the same, but urban patients are more likely to bypass the closest site than suburban and rural patients. This is expected because the number of hospital sites located in urban regions is higher than in other regions.

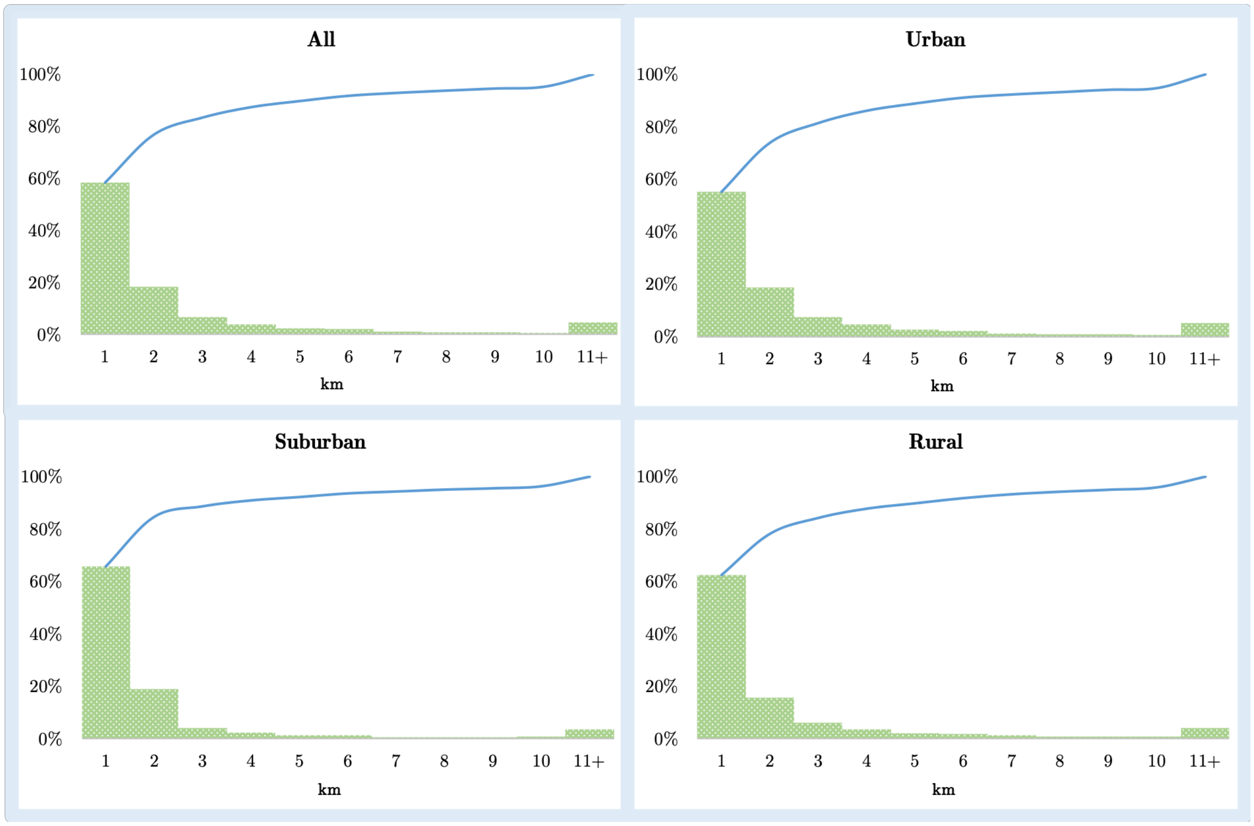


Figure 5-2. Discrete histogram with cumulative frequency, patients who choose the Nth closest hospital site, by location

These figures provide graphical evidence that there exists heterogeneity in patients' willingness to travel and that patients living in different regions of Ontario could be impacted differently by hospital mergers. The pattern remains if we instead break down patients according to condition of admission and arrival type (ambulance v. other arrival). These alternate figures are available in Appendix A.

5.3.1.2 Hospitals

Table 5-2 describes hospital site characteristics over the same time period as our patient information. As previously mentioned, we exclude a set of hospitals that merged during our merger wave on the grounds that the hospitals had formed a network several years prior to merging. In addition, we exclude a small number of hospitals that merged voluntarily just before the merger wave. The average hospital site treats around five thousand patients per year, has 133 beds staffed, and a high-risk occupancy rate of 39.11%.[23] Only 15.6% of all hospitals have a teaching status and the hospital-standardized mortality ratio (i.e. observed death divided by expected death multiplied by 100) is 103.44. 22.5% patients discharged were treated for AMI, 37.38% of patients were in the hospital for pneumonia and less than ten percent were treated for sepsis or stroke (6.55% and 7.85% respectively). The indirectly standardized mortality and

---

[23] This occupancy rate may seem low but recall that it only includes patients admitted with conditions responsible for 80% of in-hospital deaths.

readmission rates of all treated patients with AMI are 27.45% and 46.68% respectively. The average length of stay of treated patients for AMI was 8.7 days. For treated patients for pneumonia, the average length of stay was 11.4 days, the indirectly standardized mortality and readmission rates were 22.80% and 15.79 percent. Sepsis is the condition for which hospitals had the highest standardized mortality and readmission rates (38.91% and 82.02% respectively). The average length of stay for patients receiving treatment for sepsis was 13.6 days. The standardized mortality rate for stroke was 33.6% and the standardized readmission rate was 55.53%. Average hospital length of stay was 15.1 days. Taking a closer look at where hospitals are located, 37.7% of hospitals are located in urban areas, 28.5% are located in suburban regions while the remaining 33.8% are located in rural areas.

As shown in Table 5-3, the hospitals selected to close are similar on average to other hospitals in many characteristics, but there is some evidence of worse quality at hospitals that remain open. The HSMR, the mortality rate for AMI, and the readmission rate for pneumonia are significantly higher. A disproportionate number of urban hospitals are closed.

TABLE 5-2. DESCRIPTIVE STATISTICS, HOSPITAL SITES, 1995 AND 1996

	Mean	S.D.
Teaching institution	0.156	0.364
Lag total beds	133.228	147.752
Hospital location: urban	0.377	0.485
Hospital location: suburban	0.285	0.452
Hospital location: rural	0.338	0.474
Hospital lag ALC days	7.862	6.453
Lag HSMR	103.443	6.499
Lag occupancy ((%)	39.113	11.77
Lag volume (1000s)	5.068	5.532
Lag specialization - AMI (%)	22.499	9.768
Lag specialization - pneumonia (%)	37.379	16.751
Lag specialization - sepsis (%)	6.551	4.486
Lag specialization - stroke (%)	7.851	7.926
Lag (std) mortality rate - AMI (%)	27.447	35.956
Lag (std) mortality rate - pneumonia (%)	22.798	14.605
Lag (std) mortality rate - sepsis (%)	38.911	57.802
Lag (std) mortality rate -stroke (%)	33.607	45.729
Lag (std) readmission rate - AMI (%)	46.682	196.538
Lag (std) readmission rate - pneumonia (%)	15.787	74.777
Lag (std) readmission rate - sepsis (%)	82.022	485.762
Lag (std) readmission rate - stroke (%)	55.528	244.812
Lag mortality rate - AMI (%)	19.14	8.348
Lag mortality rate - pneumonia (%)	19.632	8.357
Lag mortality rate - sepsis (%)	21.33	14.733
Lag mortality rate - stroke (%)	15.553	10.482
Lag readmission rate - AMI	6.942	5.891
Lag readmission rate - pneumonia (%)	2.099	2.702
Lag readmission rate - sepsis (%)	2.221	3.906
Lag readmission rate - stroke (%)	4.4	5.067
Lag avg. LOS - AMI (days)	8.687	2.994
Lag avg. LOS - pneumonia (days)	11.454	4.249
Lag avg. LOS - sepsis (days)	13.631	8.046
Lag avg. LOS - stroke (days)	15.125	8.079
Observations	358	

TABLE 5-3. DESCRIPTIVE STATISTICS, HOSPITAL SITES, 1996, SELECTED TO CLOSE VERSUS NOT SELECTED TO CLOSE

	Not closed		Closed		
	Mean	S.D.	Mean	S.D.	Diff
Teaching institution	0.146	0.027	0.417	0.149	-0.271
Lag total beds	134.898	11.524	122.750	26.123	12.148
Hospital location: urban	0.360	0.036	0.750	0.131	-0.390**
Hospital location: suburban	0.281	0.034	0.167	0.112	0.114
Hospital location: rural	0.354	0.036	0.083	0.083	0.271**
Hospital lag ALC days	8.618	0.547	9.271	1.901	-0.652
Lag HSMR	103.267	0.526	98.568	1.822	4.699*
Lag occupancy ((%)	38.718	0.915	38.480	3.594	0.238
Lag volume (1000s)	5.125	0.434	5.394	1.286	-0.269
Lag specialization - AMI (%)	22.494	0.732	16.761	3.609	5.733
Lag specialization - pneumonia (%)	36.978	1.845	33.587	6.483	3.391
Lag specialization - sepsis (%)	6.378	0.326	6.560	1.400	-0.182
Lag specialization - stroke (%)	8.100	0.682	9.259	1.644	-1.159
Lag (std) mortality rate - AMI (%)	25.489	2.738	23.814	3.730	1.675
Lag (std) mortality rate - pneumonia (%)	36.250	3.636	28.104	4.870	8.146
Lag (std) mortality rate - sepsis (%)	35.839	3.911	25.890	7.774	9.949
Lag (std) mortality rate -stroke (%)	28.907	5.131	40.943	31.857	-12.036
Lag (std) readm. rate - AMI (%)	23.977	7.868	4.706	1.794	19.271*
Lag (std) readm. rate - pneumonia (%)	114.793	51.951	18.477	16.663	96.316
Lag (std) readm. rate - sepsis (%)	51.501	11.449	98.590	92.637	-47.089
Lag avg. LOS - AMI (days)	8.797	0.233	9.308	1.001	-0.511
Lag avg. LOS - Pneumonia (days)	11.812	0.358	14.827	1.979	-3.016
Lag avg. LOS - Sepsis (days)	14.290	0.726	14.991	2.497	-0.701
Lag avg. LOS - Stroke (days)	15.128	0.690	18.288	3.171	-3.160
Observations	178		12		

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

5.4 Estimation and Results

We estimate our conditional logistic regression model using the built-in STATA command clogit. As our baseline case, we specify the choice set as the closest 10 hospital sites. Other specifications are presented in Appendix A, but in all specifications, patients more than 300 km from their chosen hospital site are excluded. Similarly, potential alternative hospital sites that are located more than 300km away from the patients are not considered as part of the choice set even if these alternatives would be one of the 10 closest. Patients are also excluded when their choice is not contained within the choice set, as it is not possible to compute probabilities over alternatives when no choice is made, or when only their choice is in their set, as observables perfectly predict their choice. In our baseline specification, 4,493, 6,539, 2,118, and 4,305 patients admitted for AMI, pneumonia, sepsis and stroke are excluded, respectively, because their choice is not in the choice set or their choice set only contains their choice. This amounts to 12.39%, 12.87%, 16.72%, and 21.85% of patients. One of our sensitivity analyses shows that we can vary this choice set somewhat without changing our results.[24]

---

[24] At the time of writing we have re-estimated the model using 6, 9, 12 or 15 hospitals. Results available by request.

Our baseline specification includes both patients admitted by ambulance and those who are not. This means that the preferences of paramedics are also reflected in the estimates. As discussed in Tay (2003), the model is still valid even when preference parameters are influenced by the preferences of paramedics and healthcare practitioners. Though including patients admitted by an ambulance affects the interpretation of the model, excluding them excludes a large number of patients and would make our simulations less representative of the welfare change experienced by Ontario hospital patients. To test for the influence of paramedic preferences, we re-estimate the model by arrival type (ambulance or other) and the results are presented in Appendix A. Generally, both groups have similar preferences over hospital attributes and quality.

The factors that are important for choice are similar in magnitude and direction, evidence that patients not arriving by ambulance based their hospital choice on the same factors as paramedics. Thus, patients and paramedics appear to have similar objective functions and we choose to include patients admitted by ambulance in our baseline specification.

We leave the results of the model without interaction effects to Appendix A and focus on the results of the model with patient interaction effects. For these interaction effects, we use patient urban/rural status, age, ADG Score, and sex. We estimate the marginal utilities associated with a polynomial of distance, time invariant hospital attributes, and measures of quality and capacity. The time invariant attributes are hospital urban/rural status and teaching status. The measures are standardized mortality, standardized readmission, hospital volume, specialization, alternate-level-of-care days, the hospital standardized mortality ratio, and the high-risk occupancy rate. All measures are lagged to prevent patients admitted today having an influence on quality measures and causing an endogeneity issue. Standardized mortality, standardized readmission, and specialization are specific to the patient condition, while the other measures are calculated at the level of the hospital site.

Following marginal utilities, we present estimates of willingness to travel, which are more easily interpreted, and give a sense of the trade-offs patients would be willing to make between distance and quality. Lastly, we present the results of our two counterfactuals.

5.4.1 Estimates of marginal utility

Table 5-4 shows the estimates for the patient choice model. Since the table is large, AMI and pneumonia results are presented at the top of the table and sepsis and stroke results are presented below. In this table, marginal utilities presented beside an interaction term must be interpreted relative to the marginal utilities for the reference patient for the condition, described in detail in the *Data and descriptive statistics* section. The reference patient for each condition is a male, living in a suburban area, and has an ADG Score and age based on the mean or mode amongst patients with the same condition. We begin by discussing the preferences of the reference person followed by interaction effects.

Unsurprisingly, distance is an important determinant of choice for the reference patient, in the sense that the marginal ex-ante (dis)utility from distance is relatively large in magnitude and is highly significant. The marginal utilities are similar across conditions with the exception of stroke patients, who seem less sensitive to distance than patients with other conditions. One of our sensitivity analyses may help explain this fact. When we re-estimate the model using only patients who were not admitted by ambulance, the result that stroke patients are less sensitive intensifies. When we re-estimate the model using only patients who were admitted by ambulance, the result reverses, and stroke patients are the most sensitive to distance. Taken together, this is consistent with research showing that patients often delay going to the hospital site because they fail to recognize the symptoms of a stroke, particularly if the stroke is minor (Boulanger et al., 2018). Stroke patients who do not arrive via ambulance may travel further because they do not understand the severity of their condition.

TABLE 5-4. PATIENCE CHOICE ESTIMATION RESULTS

Base	Interaction	AMI		Pneumonia	
		Coefficient	s.e.	Coefficient	s.e.
Distance (10km)		-2.555***	(0.0688)	-2.517***	(0.0491)
	x Urban	-3.082***	(0.0799)	-2.205***	(0.0603)
	x Rural	0.872***	(0.0734)	0.849***	(0.0573)
	x Age	-0.0168***	(0.0021)	-0.0174***	(0.0011)
	x ADG Score	0.00607*	(0.0029)	0.0136***	(0.0017)
	x Female	-0.0326	(0.0569)	-0.0459	(0.0381)
Distance^2 (10km)		0.205***	(0.0130)	0.218***	(0.0095)
	x Urban	0.837***	(0.0270)	0.432***	(0.0162)
	x Rural	-0.129***	(0.0125)	-0.136***	(0.0102)
	x Age	0.00184***	(0.0004)	0.00140***	(0.0002)
	x ADG Score	0.000678	(0.0005)	-0.000497	(0.0003)
	x Female	0.00907	(0.0101)	0.00290	(0.0059)
Distance^3 (10km)		-0.00523***	(0.0006)	-0.00587***	(0.0005)
	x Urban	-0.0535***	(0.0024)	-0.0198***	(0.0010)
	x Rural	0.00382***	(0.0006)	0.00458***	(0.0005)
	x Age	-0.0000646***	(0.0000)	-0.0000304***	(0.0000)
	x ADG Score	-0.0000330	(0.0000)	0.00000333	(0.0000)
	x Female	-0.000309	(0.0004)	-0.0000280	(0.0002)
Hospital location: urban		0.271***	(0.0711)	0.0517	(0.0577)
	x Urban	0.677***	(0.1543)	0.541***	(0.1341)
	x Rural	0.183	(0.1389)	0.190	(0.1194)
Hospital location: rural		-2.238***	(0.1546)	-1.857***	(0.1140)
	x Urban	1.074**	(0.3472)	-0.248	(0.3076)
	x Rural	1.734***	(0.1730)	1.842***	(0.1304)
Lag volume (1000s)		0.102***	(0.0067)	0.0948***	(0.0057)
	x Urban	-0.0453***	(0.0065)	-0.0319***	(0.0057)
	x Rural	0.0183	(0.0116)	0.0290**	(0.0102)
	x Age	-0.000383**	(0.0001)	-0.000263***	(0.0001)
	x ADG Score	0.00111***	(0.0002)	0.000644***	(0.0001)
	x Female	-0.000115	(0.0032)	-0.00104	(0.0024)
Lag specialization (%)		0.0578***	(0.0038)	0.00421**	(0.0015)
	x Urban	-0.0155***	(0.0036)	0.00314	(0.0016)
	x Rural	-0.0146**	(0.0051)	0.000643	(0.0019)
	x Age	0.0000627	(0.0001)	0.0000741*	(0.0000)
	x ADG Score	-0.000620***	(0.0002)	0.000182***	(0.0001)
	x Female	-0.00165	(0.0031)	-0.000509	(0.0011)

table continued on next page

TABLE 5-4 CONTINUED

Lag (std) mortality		-0.00638***	(0.0016)	-0.0118***	(0.0019)
rate (%)	x Urban	-0.00965***	(0.0018)	0.00971***	(0.0019)
	x Rural	0.000965	(0.0019)	0.00910***	(0.0023)
	x Age	0.000252***	(0.0001)	0.000128**	(0.0000)
	x ADG Score	-0.0000426	(0.0001)	0.000310***	(0.0001)
	x Female	0.000976	(0.0016)	0.00137	(0.0015)
Lag (std) readmission		-0.00841***	(0.0015)	-0.000654	(0.0008)
rate (%)	x Urban	-0.00320	(0.0017)	-0.0255***	(0.0028)
	x Rural	0.00335*	(0.0015)	-0.00320**	(0.0010)
	x Age	0.000172***	(0.0000)	-0.0000447	(0.0000)
	x ADG Score	0.0000572	(0.0001)	-0.0000374	(0.0000)
	x Female	-0.00175	(0.0011)	-0.000466	(0.0009)
Lag ALC days (days)		-0.0286***	(0.0052)	-0.0241***	(0.0038)
	x Urban	0.0292***	(0.0050)	0.0429***	(0.0040)
	x Rural	0.00903	(0.0066)	0.0164**	(0.0052)
	x Age	0.0000571	(0.0002)	0.000236*	(0.0001)
	x ADG Score	0.0000451	(0.0002)	-0.000375**	(0.0001)
	x Female	0.00939*	(0.0041)	0.00103	(0.0030)
Lag HSMR		0.0521***	(0.0137)	0.00859	(0.0083)
	x Urban	-0.0302*	(0.0132)	-0.0176*	(0.0083)
	x Rural	0.0173	(0.0168)	-0.00133	(0.0118)
	x Age	-0.000231	(0.0004)	-0.000121	(0.0001)
	x ADG Score	-0.00192***	(0.0005)	-0.000562**	(0.0002)
	x Female	-0.0141	(0.0102)	-0.0122**	(0.0040)
Lag high-risk		-0.00400	(0.0030)	0.0207***	(0.0021)
occupancy	x Urban	-0.00790**	(0.0028)	-0.00648**	(0.0022)
	x Rural	-0.00318	(0.0040)	-0.0136***	(0.0030)
	x Age	0.0000588	(0.0001)	0.000177***	(0.0001)
	x ADG Score	0.000916***	(0.0001)	0.000185*	(0.0001)
	x Female	-0.00536*	(0.0024)	-0.00412*	(0.0018)
Teaching institution		-0.358***	(0.0366)	-0.122***	(0.0292)
Observations		341790		483765	
Pseudo R-squared		0.601		0.576	

table continued on next page

TABLE 5-4 CONTINUED

Base	Interaction	Sepsis		Stroke	
		Coefficient	s.e.	Coefficient	s.e.
Distance (10km)		-2.641***	(0.1118)	-1.997***	(0.0657)
	x Urban	-1.807***	(0.1045)	-2.936***	(0.086)
	x Rural	0.694***	(0.126)	0.520***	(0.1013)
	x Age	-0.0304***	(0.0023)	-0.0272***	(0.0023)
	x ADG Score	0.0132***	(0.0034)	-0.00442	(0.0031)
	x Female	-0.0931	(0.0815)	0.00217	(0.0565)
Distance^2 (10km)		0.227***	(0.021)	0.175***	(0.009)
	x Urban	0.302***	(0.026)	0.749***	(0.0244)
	x Rural	-0.0954***	(0.0200)	-0.0673***	(0.0133)
	x Age	0.00321***	(0.0004)	0.00228***	(0.0003)
	x ADG Score	-0.000213	(0.0006)	0.000364	(0.0004)
	x Female	0.00743	(0.0152)	0.00144	(0.0079)
Distance^3 (10km)		-0.00622***	(0.001)	-0.00410***	(0.0003)
	x Urban	-0.0118***	(0.0014)	-0.0426***	(0.002)
	x Rural	0.00238**	(0.0008)	0.00155***	(0.0004)
	x Age	-0.0000995***	(0.0000)	-0.0000484***	(0.0000)
	x ADG Score	-0.0000095	(0.0000)	-0.0000114	(0.0000)
	x Female	0.000224	(0.0007)	-0.0000536	(0.0002)
Hospital location:		0.325**	(0.1225)	0.282**	(0.1086)
urban	x Urban	0.586*	(0.2869)	2.285***	(0.2785)
	x Rural	-0.218	(0.2599)	0.0375	(0.1869)
		-2.456***	(0.2929)	-0.755**	(0.2489)
Hospital location: rural	x Urban	0.103	(0.7792)	-3.537**	(1.0813)
	x Rural	1.862***	(0.3277)	-0.355	(0.2917)
Lag volume (1000s)		0.105***	(0.0110)	0.138***	(0.0085)
	x Urban	-0.0423***	(0.0104)	-0.0629***	(0.0084)
	x Rural	0.0287	(0.0205)	-0.0348*	(0.0151)
	x Age	-0.000243	(0.0001)	-0.000341*	(0.0001)
	x ADG Score	0.000451*	(0.0002)	0.000376*	(0.0002)
	x Female	-0.00316	(0.0045)	-0.0000763	(0.0036)
Lag specialization (%)		0.112***	(0.0122)	0.0979***	(0.0041)
	x Urban	-0.0824***	(0.0108)	-0.0339***	(0.004)
	x Rural	-0.0203	(0.0185)	0.00135	(0.0071)
	x Age	-0.000996***	(0.0002)	-0.00101***	(0.0001)
	x ADG Score	0.000243	(0.0003)	-0.00121***	(0.0002)
	x Female	-0.00761	(0.0072)	-0.0035	(0.003)

table continued on next page

TABLE 5-4 CONTINUED

Lag (std) mortality rate (%)		0.00473*	(0.0019)	-0.000285	(0.0015)
	x Urban	-0.00939***	(0.0017)	-0.000177	(0.0017)
	x Rural	-0.00404*	(0.0017)	-0.00464*	(0.0018)
	x Age	-0.000103**	(0.0000)	0.0000571	(0.0001)
	x ADG Score	0.000084	(0.0000)	0.0000388	(0.0001)
	x Female	-0.00221	(0.0012)	-0.000147	(0.0014)
Lag (std) readmission rate (%)		-0.00267**	(0.0008)	-0.00106*	(0.0005)
	x Urban	-0.000313	(0.0011)	0.00151**	(0.0005)
	x Rural	0.00247**	(0.0008)	-0.0000803	(0.0008)
	x Age	0.00000942	(0.0000)	-0.0000196	(0.0000)
	x ADG Score	0.00000773	(0.0000)	0.0000155	(0.0000)
	x Female	0.000137	(0.0002)	0.0000458	(0.0003)
Lag ALC days (days)		-0.00602	(0.0090)	-0.0515***	(0.0075)
	x Urban	0.0469***	(0.0082)	0.0622***	(0.0075)
	x Rural	-0.00386	(0.0122)	0.0207	(0.0118)
	x Age	0.00105***	(0.0002)	0.00121***	(0.0002)
	x ADG Score	-0.00113***	(0.0002)	0.00000337	(0.0003)
	x Female	-0.000776	(0.0058)	0.0173***	(0.0049)
Lag HSMR		-0.00646	(0.0216)	0.0539**	(0.018)
	x Urban	-0.0108	(0.0205)	-0.0295	(0.0181)
	x Rural	0.0838**	(0.0304)	0.00948	(0.0267)
	x Age	0.000041	(0.0003)	-0.00143**	(0.0005)
	x ADG Score	-0.000373	(0.0004)	-0.00356***	(0.0006)
	x Female	-0.00745	(0.0097)	-0.00583	(0.0114)
Lag high-risk occupancy		0.0184***	(0.0055)	0.00322	(0.0038)
	x Urban	-0.0121*	(0.0047)	-0.00597	(0.0039)
	x Rural	-0.0144	(0.0076)	-0.00354	(0.0059)
	x Age	0.000280*	(0.0001)	0.000737***	(0.0001)
	x ADG Score	0.0000639	(0.0002)	0.00108***	(0.0002)
	x Female	0.0044	(0.0038)	-0.00416	(0.003)
Teaching institution		-0.189***	(0.0565)	-0.185***	(0.045)
Observations		112953		168448	
Pseudo R-squared		0.531		0.533	

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Estimation by conditional logistic regression for patients admitted with sepsis and stroke. Choice set size of 10 hospitals. Marginal utility reported. Patient and hospital characteristics included.

Figure 5-3 combines the marginal utility from all distance terms. Similar to what has been found in the U.K. for hospitals (Gutacker et al., 2016; Moscelli et al., 2016; Santos et al., 2017) marginal disutility from distance is increasing at first, then sharply decreasing after a certain inflection point. In a theoretical paper, Mainardi (2007) refers to this inflection point as the safety threshold, reflecting that travelling large distances to access care is unsafe but that the difference in safety is small for a low number of kilometres. Including a cubic term avoids the unrealistic relationship between distance and utility that is estimated when only *distance* and $distance^2$ are included. When the equation is estimated without the $distance^3$ term, the coefficient on distance is still negative, and the coefficient on $distance^2$ is still positive, so as the number of kilometers becomes large, the contribution of the $distance^2$ term becomes larger and the utility from distance becomes more and more positive. Including $distance^3$ allows for a more realistic relationship between utility and distance, as patients are penalized for travelling large distances. The figure with the cubic term omitted is included in Appendix A. For two of the conditions, excluding $distance^3$ also impacts the marginal utility of urban patients for rural hospitals in a way that is inconsistent with what we would expect. For example, AMI patients that live in urban centres prefer rural hospital sites more than suburban patients but also more than rural patients. The marginal utility of urban patients for a rural hospital site is almost twice as large as the marginal utility of rural patients for rural hospital sites. The marginal benefit received from obtaining treatment at a rural site is also larger than the (dis)utility from distance. A likelihood-ratio test confirms that the fit of the model is improved when the cubic term is included.

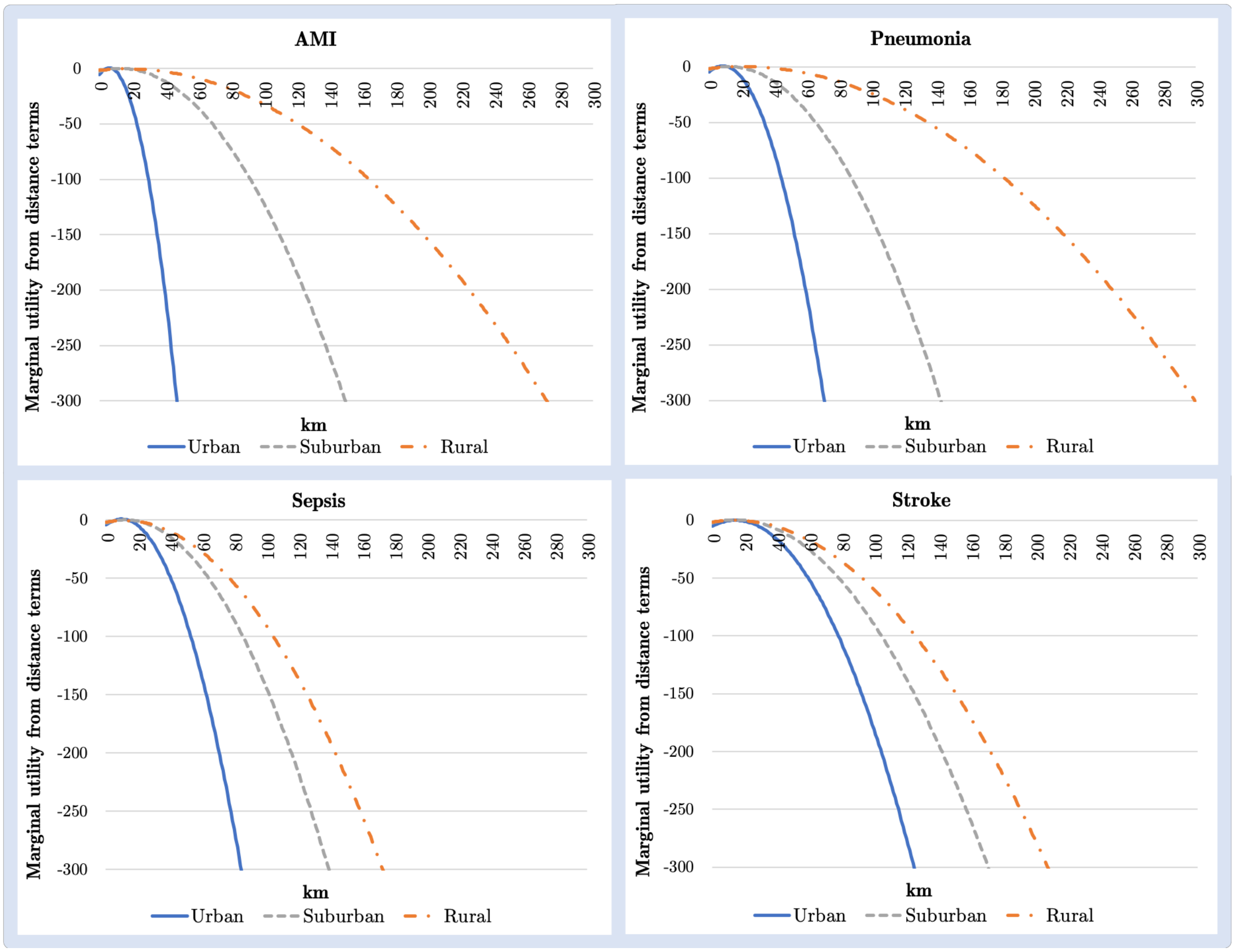


Figure 5-3. Marginal utility from distance (km) in the baseline specification, reference person, by condition and location

Some other results are also consistent across conditions. The reference patient prefers not to seek treatment at a rural hospital site and prefers a hospital site with larger overall volume and greater specialization in the condition for which they are admitted.

They would also prefer not to attend a teaching hospital site. Results are not consistent for mortality, readmission, ALC, HSMR and high-risk occupancy. When significant, the signs of the quality measures are in the expected direction (higher quality is associated with higher marginal utility).

Estimation of the interaction effects suggests that there are important differences between patient groups. Though patients in urban, suburban and rural markets all dislike distance, urban patients receive the most disutility and rural patients receive the least. Older patients are more sensitive to distance than the reference person, and patients with higher comorbidity as measured by the ADG score are less sensitive. This may be because patients with pre-existing health conditions may be more inclined to travel to specific hospitals where they know that their other conditions can be managed. Urban patients have a stronger preference for urban hospitals than the reference patient. Urban patients tend to dislike hospitals that treat more patients and that are more congested. The same is true for older patients, although high-risk occupancy does not matter for this subgroup. Other interaction effects are inconsistent in significance and sign.

5.4.2 Estimates of willingness to travel

We estimate willingness to travel with respect to each of our quality measures. Willingness to travel can be interpreted as the number of additional kilometers a patient would be willing to travel to access a hospital where the quality measure in question is one standard deviation higher. Since higher alternate-level-of-care days, hospital standardized mortality ratio, high-risk occupancy, standardized mortality rates and readmission rates are indicators of poor quality, we would expect the willingness to travel estimates to be negative. On the other hand, the willingness to travel estimates for volume and specialization might be positive, since patients seem to prefer hospitals with higher volume and specialization.

TABLE 5-5. WILLINGESS TO TRAVEL ESTIMATES, REFERENCE PATIENT

Condition	Quality Measures	S.D.(Q)	μ_d	WTT	s.e.	CI_L	CI_U
AMI	ALC days	6.183	0.839	-0.074	0.014	-0.101	-0.047
	HSMR	3.324		0.072	0.019	0.035	0.110
	High-risk occupancy	9.518		-0.016	0.012	-0.040	0.008
	Volume	6.335		0.272	0.019	0.235	0.309
	Specialization	8.473		0.205	0.014	0.177	0.233
	Std. mortality rate	37.642		-0.101	0.026	-0.151	-0.050
	Std. readmission rate	60.634		-0.214	0.038	-0.288	-0.139
Pneu.	ALC days	6.891	0.838	-0.071	0.011	-0.093	-0.049
	HSMR	7.226		0.065	0.026	-0.024	0.077
	High-risk occupancy	12.994		0.115	0.012	0.092	0.138
	Volume	6.362		0.258	0.016	0.227	0.289
	Specialization	14.134		0.025	0.009	0.007	0.044
	Std. mortality rate	18.324		-0.092	0.015	-0.121	-0.063
	Std. readmission rate	124.978		-0.035	0.043	-0.119	0.049

table continued on next page

TABLE 5-5 CONTINUED

Sepsis	ALC days	5.840	0.818	-0.014	0.021	-0.056	0.027
	HSMR	16.620		-0.044	0.146	-0.330	0.243
	High-risk occupancy	14.336		0.107	0.032	0.044	0.170
	Volume	5.375		0.229	0.025	0.180	0.278
	Specialization	4.678		0.213	0.024	0.167	0.259
	Std. mortality rate	48.754		0.094	0.037	0.022	0.166
	Std. readmission rate	55.034		-0.060	0.019	-0.096	-0.023
Stroke	ALC days	5.193	0.955	-0.146	0.022	-0.189	-0.103
	HSMR	2.958		0.087	0.029	0.029	0.145
	High-risk occupancy	10.601		0.019	0.022	-0.025	0.062
	Volume	5.587		0.419	0.027	0.366	0.472
	Specialization	8.322		0.444	0.021	0.404	0.485
	Std. mortality rate	20.745		-0.003	0.016	-0.035	0.029
	Std. readmission rate	50.944		-0.029	0.015	-0.059	0.000

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Willingness to travel computed with respect to each of the quality measures included in main specification. The reference patient is willing to travel X km to a hospital with a quality measure one standard deviation higher. μ_d is the mean distance for each of the reference patients, CI_L is the lower confidence interval and CI_U is the upper confidence internal. The standard deviation of each quality measure is reported in the 4th column. The standard error of each WTT measure is computed using the delta method.

As expected, the willingness to travel estimates are positive and significant for volume and specialization. For example, the reference patient for sepsis will travel an additional 2.29 kilometers to access a hospital that is one standard deviation larger in terms of volume. The reference patients for AMI, pneumonia and sepsis will travel farther to access a hospital with a lower standardized better mortality rate, which is consistent with the regression results. Estimates of the WTT for stroke patients is not significant. Similarly, for the WTT with respect to the readmission rate and ALC days, three of the four conditions have a significant and negative WTT (AMI, pneumonia and stroke), where higher ALC days is associated with worst hospital quality. The WTT estimates for the HSMR and the high-risk occupancy rate are not consistently significant. This is also consistent with the estimation results.

5.4.3 The impact of hospital mergers on patient welfare: two counterfactuals

Using our existing estimated preference parameters, we compute individual and aggregate measures of welfare for patients whose observed choice later closed as a result of the mergers. We also compute the individual welfare according to patient location (urban, suburban, rural).

In our first counterfactual, some hospital sites are removed, but the characteristics of the remaining hospital sites are the same. Therefore, the change in utility comes only from patients who had their choice removed. This counterfactual represents the short-run, in which hospital sites have closed, but quality has not had time to adjust. In the second counterfactual, since the quality of the remaining hospital sites has changed, any patient may experience a change in utility. This scenario represents the long-run, in which quality has changed as a result of the mergers and resources have been reallocated.

Patient *i*'s utility from hospital site *j* depends on observable hospital site and patient characteristics, but also on an unobservable idiosyncratic taste shock that must be accounted for when simulating the welfare change. As mentioned, failing to do so may result in a bias capturing the reallocation of patients from their actual choice to the observable best hospital site choice as opposed to the impact of hospital mergers. This is especially true for the long-run simulation where all patients could end up choosing a different hospital site than the one which we observe in the data. To account for this shock in the calculation of the utility level, we first draw a set of Type I extreme value idiosyncratic shocks, one for each patient-hospital site pair for all alternative hospitals. For each of these alternatives, we sum the utility level from observables, V_{ij}, and the idiosyncratic shock draw, ε_{ij}, to obtain the overall utility, u_{ij}. For each patient, we then find the highest utility level among alternatives. For this alternative, we know that it must be that $u_{i,max} < u_{ic}$ where u_{ic} is the utility from the choice hospital and $u_{i,max}$ is the maximum utility among all alternative from hospital sites in patient *i*'s choice set. Otherwise, the patient would have chosen this alternative instead. This will be true for all alternative hospital sites as patient *i*'s utility from any of the other alternatives is lower than $u_{i,max}$. Rearranging the above inequality, we can obtain a lower bound on the draw of the idiosyncratic shock associated with the choice hospital site for it to be rationalized by the data:

$$\begin{gathered} u_{i,max} < u_{ic} \\ V_{i,max} + \varepsilon_{I,max} < V_{ic} + \varepsilon_{ic} \\ V_{i,max} + \varepsilon_{I,max} - V_{ic} = \underline{\varepsilon} < \varepsilon_{ic} \end{gathered} \qquad \text{(Eq. 14)}$$

where $\underline{\varepsilon}$ is the minimum value of the Type I extreme value shock for that choice hospital site that could rationalize that patient *i* went to his choice hospital. We find the location of this value along the distribution and we randomly select the shock for the choice hospital site from the portion of the Type I extreme value distribution above the cut-off value given by $\underline{\varepsilon}$. We use this approach of bounding the distribution of the choice shock because for some patients, we cannot distinguish whether they made this choice because $V_{ia} > V_{ic}$ and $\varepsilon_{ia} > \varepsilon_{ic}$ or because $V_{ia} > V_{ic}$ and $\varepsilon_{ia} < \varepsilon_{ic}$ when $V_{ia} - V_{ic} > |\varepsilon_{ia} - \varepsilon_{ic}|$ for hospital sites *a* and *c*. But we observe which hospital site was their choice and we want to condition on this information.

Once we have an idiosyncratic shock for each patient-hospital site pair, including the choice, we compute the utility level associated with each pair. We must then find the new optimal choice of patients in a simulated world where hospital mergers have occurred and compute the difference in welfare. We simulate this process 100 times. In the first counterfactual hospitals have merged and closed sites, but hospitals have not had time to adapt therefore quality has not yet changed. In the second counterfactual, the hospitals have merged and closed sites, and quality has adjusted as a result of the mergers. Resources from closed hospital sites may have also been reallocated to other sites, potentially improving quality. These two counterfactuals give us a measure of the short-run and long-run impact of mergers, respectively. A shortcoming of this method is that changes to patient flows are not considered. In reality, hospitals remaining in a market after a merger may have to admit a greater number of patients than before, which could affect utility. The benefit of keeping the same patient pool across counterfactuals is that it allows us to be certain that any resulting welfare change is not caused by a change in the patient pool (i.e. sicker patients are admitted in 2005 relative to the pre-merger period) as a result of the shift in from inpatient to outpatient care that occurred over time.

Table 5-6 shows the impact of hospital mergers on patients whose choice is closed as a result of mergers (i.e. the short-run impact). Only 6.74%, 4.71%, 5.35% and 3.52% of AMI, pneumonia, sepsis, and stroke patients are impacted in the short-run, respectively. Since the decrease in welfare is driven by patients travelling farther to the hospital site, all groups are worse off on average. These patients are also going to a hospital site with different characteristics than their original choice hospital. Stroke patients are the least harmed by the mergers with the median patients facing a decrease in welfare of 91.25% and a total welfare reduction for all stroke patients admitted between 1995 and 1996 of 25.92%. Patients the most impacted by the policy are sepsis patients. The median patient sees its utility fall by 268.14% with a total welfare change of -116.37% when aggregating all sepsis patients together. The welfare loss of AMI and pneumonia patients is between those of sepsis and stroke patients. The median welfare loss for AMI and pneumonia is 91.25% and 193.77%, and the total welfare change for patients with these conditions is -60.53% and -87.19%.

TABLE 5-6. CONDITIONAL SHORT-RUN WELFARE IMPACT OF HOSPITAL MERGERS

		Obs.	Mean	Median	S.D.	Min	Max
AMI	Pre-merger utility	2142	2.13	2.15	2.19	-7.95	9.51
	Post-merger utility	2142	0.84	0.81	2.18	-8.86	8.15
	Welfare δ (level)	2142	-1.29	-1.19	0.33	-2.79	-0.76
	Welfare δ (%)	2142	-304.36	-91.25	1616.90	-51656.54	-10.42
	Welfare Δ (level)	2142	-2756.78	-2756.78	-	-2756.78	-2756.78
	Welfare Δ (%)	2142	-60.53	-60.53	-	-60.53	-60.53
Pneu.	Observed utility	3374	1.42	1.39	1.39	-8.01	6.09
	Simulated utility	3374	0.18	0.14	1.44	-9.11	5.02
	Welfare δ (level)	3374	-1.24	-1.18	0.27	-2.93	-0.71
	Welfare δ (%)	3374	-416.32	-193.77	2513.34	-103622.60	-16.81
	Welfare Δ (level)	3374	-4176.23	-4176.23	-	-4176.23	-4176.23
	Welfare Δ (%)	3374	-87.19	-87.19	-	-87.19	-87.19
Sepsis	Pre-merger utility	879	1.07	0.72	1.92	-4.60	10.39
	Post-merger utility	879	-0.17	-0.51	1.94	-5.63	8.81
	Welfare δ (level)	879	-1.24	-1.18	0.25	-2.83	-0.81
	Welfare δ (%)	879	-526.45	-268.14	1965.94	-38372.56	-10.89
	Welfare Δ (level)	879	-1090.69	-1090.69	-	-1090.69	-1090.69
	Welfare Δ (%)	879	-116.37	-116.37	-	-116.37	-116.37
Stroke	Pre-merger utility	1159	4.53	4.47	3.97	-11.36	22.24
	Post-merger utility	1159	3.35	3.32	4.03	-12.46	21.26
	Welfare δ (level)	1159	-1.17	-1.12	0.23	-2.37	-0.73
	Welfare δ (%)	1159	-130.10	-23.47	540.00	-10739.41	-4.30
	Welfare Δ (level)	1159	-1359.58	-1359.58	-	-1359.58	-1359.58
	Welfare Δ (%)	1159	-25.92	-25.92	-	-25.92	-25.92

Notes: This table reports the welfare change of patients whose observed choice closed in the following years as a result of the mergers. Pre-merger utility refers to patient utility from their actual choice observed in the data. It is computed from the estimated parameters and the observable patient and hospital characteristics. Post-merger utility refers to the simulated utility computed based on observables and the idiosyncratic shock. Welfare δ and Welfare Δ, both defined mathematically in Section 5.3, represent the per-capita welfare change and total welfare change, respectively.

As seen in Table 5-7 the distribution of welfare change in the short-run is relatively similar across location for each of the conditions. Although the decrease in welfare varies across location. This is true for each of the four conditions. We also see that there are differences between locations.

TABLE 5-7. CONDITIONAL SHORT-RUN WELFARE IMPACT OF HOSPITAL MERGERS BY REGION

	Location	Variables	Count	Mean	Median	S.D.	Min	Max
AMI	Rural	Welfare δ (level)	103	-44.72	-22.24	101.76	-772.62	-10.42
		Welfare δ (%)	103	-1.25	-1.24	0.17	-1.63	-0.78
	Suburban	Welfare δ (level)	114	-177.85	-40.28	694.26	-7234.91	-10.77
		Welfare δ (%)	114	-1.61	-1.46	0.56	-2.79	-0.83
	Urban	Welfare δ (level)	1925	-325.75	-112.42	1695.69	-51656.54	-12.44
		Welfare δ (%)	1925	-1.27	-1.18	0.31	-2.71	-0.76
Pneu.	Rural	Welfare δ (level)	235	-529.11	-180.05	2190.97	-23248.35	-31.56
		Welfare δ (%)	235	-1.21	-1.21	0.13	-1.57	-0.88
	Suburban	Welfare δ (level)	267	-737.09	-213.43	5235.02	-84257.77	-16.81
		Welfare δ (%)	267	-1.47	-1.25	0.50	-2.93	-0.79
	Urban	Welfare δ (level)	2872	-377.26	-192.01	2116.34	-103622.60	-18.03
		Welfare δ (%)	2872	-1.22	-1.17	0.23	-2.50	-0.71
Sepsis	Rural	Welfare δ (level)	39	-16.37	-15.46	4.63	-33.67	-10.89
		Welfare δ (%)	39	-1.35	-1.41	0.20	-1.68	-0.93
	Suburban	Welfare δ (level)	35	-401.50	-246.86	351.92	-1339.13	-43.54
		Welfare δ (%)	35	-1.24	-1.17	0.23	-1.76	-0.90
	Urban	Welfare δ (level)	805	-556.60	-279.92	2049.68	-38372.56	-28.19
		Welfare δ (%)	805	-1.24	-1.17	0.25	-2.83	-0.81
Stroke	Rural	Welfare δ (level)	20	-54.99	-32.13	65.52	-244.05	-7.56
		Welfare δ (%)	20	-1.14	-1.15	0.13	-1.46	-0.94
	Suburban	Welfare δ (level)	69	-290.40	-28.06	1124.30	-8826.11	-6.41
		Welfare δ (%)	69	-1.30	-1.07	0.44	-2.33	-0.80
	Urban	Welfare δ (level)	1070	-121.17	-22.93	483.27	-10739.41	-4.30
		Welfare δ (%)	1070	-1.17	-1.12	0.21	-2.37	-0.73

Notes: See previous table for definitions.

Table 5-8 compares the actual distance travelled by patients with the distance travelled by patients in the two counterfactuals. All patient groups travel farther on average in both counterfactuals. Table 5-9 reports the long-run effect of hospital mergers. In this counterfactual, quality has been updated and all patient choices are reoptimized. The direction of the long-run impact varies across conditions. Patients admitted for AMI are negatively impacted in the long-run by hospital mergers with the median patient facing a decreased utility of 9.89% and a total welfare reduction of 11.35%. On the other hand, patients with the other three conditions are positively impacted by hospital mergers in the long-run. Pneumonia patients face a welfare change of 55.23% at the median with a total welfare increase of 122.09%. For sepsis patients these numbers are 98.82% and 119.01% respectively. For stroke patients, the welfare change is smaller, at 11.38% at the median with a total welfare increase of 39.19%.

If we focus on the long-run merger impact, Table 5-10 shows that the results are not consistent across regions for the different conditions. At the median, patients living in rural areas are harmed in the long-run for all conditions except stroke. For two of the conditions, the long-run welfare change of suburban patients is positive and for the other two it is negative. Patients in urban areas all benefit from the hospital mergers in the long-run regardless of the condition of admission.

TABLE 5-8. PRE-MERGER AND CONDITIONAL POST-MERGER DISTANCES

	Location	Obs.	Mean	S.D.	Mean	S.D.	Diff
			Pre-merger		Post-merger (1995-1996)		
AMI	All	2142	0.534	0.664	0.666	0.527	0.132
	Rural	103	1.486	1.568	1.417	1.269	-0.069
	Suburban	114	1.028	1.630	1.478	1.032	0.449
	Urban	1925	0.454	0.367	0.578	0.291	0.124
Pneu.	All	3374	0.556	0.737	0.649	0.603	0.093
	Rural	235	1.377	1.747	1.347	1.533	-0.030
	Suburban	267	0.926	1.305	1.168	0.898	0.241
	Urban	2872	0.454	0.392	0.544	0.290	0.089
Sepsis	All	879	0.518	0.697	0.657	0.648	0.139
	Rural	39	1.484	2.485	1.470	2.348	-0.014
	Suburban	35	0.816	1.168	1.303	1.208	0.487
	Urban	805	0.458	0.359	0.589	0.286	0.131
Stroke	All	1159	0.545	0.667	0.672	0.631	0.127
	Rural	20	1.628	2.206	2.169	1.895	0.541
	Suburban	69	1.137	1.630	1.741	1.305	0.604
	Urban	1070	0.487	0.422	0.575	0.367	0.088
			Pre-merger		Post-merger (2005)		
AMI	All	35021	0.839	1.199	0.958	1.227	0.119
	Rural	3627	2.268	2.207	2.427	2.203	0.159
	Suburban	8532	1.168	1.313	1.406	1.364	0.238
	Urban	22862	0.489	0.549	0.558	0.513	0.068
Pneu.	All	48662	0.838	1.247	1.073	1.375	0.235
	Rural	6023	2.050	2.222	2.209	2.266	0.159
	Suburban	12372	1.104	1.320	1.693	1.591	0.589
	Urban	30267	0.488	0.603	0.594	0.581	0.106
Sepsis	All	11819	0.818	1.278	0.940	1.244	0.121
	Rural	938	2.395	2.443	2.473	2.393	0.078
	Suburban	2412	1.304	1.618	1.619	1.515	0.315
	Urban	8469	0.505	0.657	0.576	0.591	0.071
Stroke	All	17848	0.955	1.791	1.136	1.828	0.181
	Rural	1082	3.904	3.641	3.442	2.905	-0.461
	Suburban	2972	1.900	2.780	2.568	3.179	0.667
	Urban	13794	0.520	0.620	0.647	0.620	0.127

Notes: Distance in units of 10 km. The post-merger distance is simulated.

TABLE 5-9. CONDITIONAL LONG-RUN WELFARE IMPACT OF HOSPITAL MERGERS

		Obs.	Mean	Median	S.D.	Min	Max
AMI	Pre-merger utility	35017	2.08	2.07	2.63	-10.69	10.30
	Post-merger utility	35021	1.84	1.79	2.40	-8.09	19.44
	Welfare δ (level)	35017	-0.24	-0.21	1.46	-7.51	19.95
	Welfare δ (%)	35017	62.73	-10.97	1668.71	-70792.45	161859.88
	Welfare Δ (level)	35021	-8258.42	-8258.42	-	-8258.42	-8258.42
	Welfare Δ (%)	35021	-11.35	-11.35	-	-11.35	-11.35
Pneu.	Observed utility	48655	0.67	0.84	1.67	-8.73	6.09
	Simulated utility	48662	1.49	1.28	2.84	-8.50	47.16
	Welfare δ (level)	48655	0.82	0.37	2.44	-2.88	46.59
	Welfare δ (%)	48655	628.10	75.75	25842.86	-76731.48	5469564.50
	Welfare Δ (level)	48662	39923.56	39923.56	-	39923.56	39923.56
	Welfare Δ (%)	48662	122.11	122.11	-	122.11	122.11
Sepsis	Pre-merger utility	11816	0.70	0.45	2.36	-7.67	11.36
	Post-merger utility	11819	1.53	1.07	2.44	-7.12	17.83
	Welfare δ (level)	11816	0.83	0.68	1.53	-8.60	16.31
	Welfare δ (%)	11816	625.25	168.23	9034.88	-28869.35	782029.50
	Welfare Δ (level)	11819	9811.22	9811.22	-	9811.22	9811.22
	Welfare Δ (%)	11819	118.91	118.91	-	118.91	118.91
Stroke	Pre-merger utility	17846	4.36	4.51	4.02	-13.25	24.82
	Post-merger utility	17848	6.07	5.22	5.44	-10.09	49.37
	Welfare δ (level)	17846	1.71	0.50	3.97	-9.51	32.97
	Welfare δ (%)	17846	280.22	17.12	2655.03	-25541.24	159640.42
	Welfare Δ (level)	17848	30516.34	30516.34	-	30516.34	30516.34
	Welfare Δ (%)	17848	39.18	39.18	-	39.18	39.18

Notes: This table reports the welfare change of patients whose observed choice closed in the following years as a result of the mergers. Pre-merger utility refers to patient utility from their actual choice observed in the data. It is computed from the estimated parameters and the observable patient and hospital characteristics. Post-merger utility refers to the simulated utility computed based on observables and the idiosyncratic shock. Welfare δ and Welfare Δ, both defined mathematically in Section 5.3, represent the per-capita welfare change and total welfare change, respectively.

TABLE 5-10. CONDITIONAL LONG-RUN WELFARE IMPACT OF HOSPITAL MERGERS BY REGION

	Location	Variables	Count	Mean	Median	S.D.	Min	Max
AMI	Rural	Welfare δ (level)	3627	-18.69	1230.33	-7998.93	-20.70	70360.26
		Welfare δ (%)	3627	-0.86	1.18	-6.93	-0.83	14.29
	Suburban	Welfare δ (level)	8531	4.60	836.99	-17886.78	-21.59	32786.73
		Welfare δ (%)	8531	-0.90	1.87	-7.51	-0.77	19.95
	Urban	Welfare δ (level)	22859	97.35	1939.23	-70792.45	12.92	161859.90
		Welfare δ (%)	22859	0.11	1.18	-5.82	0.09	11.33
Pneu.	Rural	Welfare δ (level)	6022	31.05	1462.85	-21962.30	-8.03	88834.52
		Welfare δ (%)	6022	0.11	1.67	-2.32	-0.05	45.67
	Suburban	Welfare δ (level)	12371	786.27	12133.67	-76731.48	45.37	883230.30
		Welfare δ (%)	12371	1.10	2.52	-2.88	0.22	16.79
	Urban	Welfare δ (level)	30262	682.24	31829.08	-53126.36	111.13	5469565.00
		Welfare δ (%)	30262	0.85	2.50	-1.85	0.48	46.59
Sepsis	Rural	Welfare δ (level)	938	-17.35	81.24	-1180.37	-11.25	1296.70
		Welfare δ (%)	938	-0.84	1.11	-8.60	-0.78	3.40
	Suburban	Welfare δ (level)	2412	1065.58	8937.36	-5231.69	262.98	369424.80
		Welfare δ (%)	2412	1.67	2.48	-3.66	1.03	16.31
	Urban	Welfare δ (level)	8466	571.00	9543.50	-28869.35	177.75	782029.50
		Welfare δ (%)	8466	0.78	0.93	-1.84	0.68	8.43
Stroke	Rural	Welfare δ (level)	1082	81.55	879.42	-25541.24	3.34	3762.83
		Welfare δ (%)	1082	0.23	2.03	-4.84	0.06	25.70
	Suburban	Welfare δ (level)	2972	346.80	3656.57	-17846.21	-1.40	119224.40
		Welfare δ (%)	2972	0.26	3.17	-7.02	-0.13	29.97
	Urban	Welfare δ (level)	13792	281.45	2485.21	-9259.75	30.55	159640.40
		Welfare δ (%)	13792	2.14	4.13	-9.51	0.63	32.97

Notes: See previous table for definitions.

5.5 Conclusion

In this paper, we estimate the parameters of the utility function of patients admitted to the hospital for AMI, pneumonia, sepsis or stroke who decided to seek treatment at one of the acute care hospitals in Ontario in 1995 and 1996. We find that distance is an important factor in choice of hospital site, and that the reference patient prefers a site that is larger in terms of volume, is more specialized in the condition with which the patient is diagnosed, is not located in a rural market, and does not have teaching status. There also seem to be important differences in patient groups. For example, urban patients are more sensitive to distance and rural patients are less sensitive. We compute willingness to travel estimates with respect to quality and find that reference patients are willing to travel a significantly greater distance to access a higher volume hospital or a hospital more specialized in the condition of admission. This is consistent with the evidence previously shown in the literature (Gutacker et al., 2016).

Using counterfactuals, we measure the short- and long-run impact of the mergers. In the short-run counterfactual, some patients travel farther to access a hospital that would not be their first choice, so the welfare impact is negative on average and at the median for every patient subgroup. In the long-run counterfactual, the welfare impact again varies by patient subgroup. Rural patients are worse off, faring worse than urban patients at the average and at the median, with a much greater portion of rural patients being harmed by mergers.

In our long-run counterfactual, the average and median patient travels farther for all conditions and for each region (urban/suburban/rural), and yet, some patient groups experience a positive welfare impact at the average and median. This suggests that for some patient groups, the welfare impact from changes in quality outweighs the welfare impact from the increase in distance travelled. Taken together with Chapter 4, this suggests that though hospital mergers have the potential to improve quality and cost, certain patient groups, such as those living in rural areas, can still be harmed.

References

Abadie, A., & Gardeazabal, J. (2003). The economic costs of conflict: A case study of the Basque Country. *The American Economic Review*, *93*(1), 113–132.

Adams, E. K., Porell, F. W., & Robbins, J. D. (1996). Estimating the utilization impacts of hospital closures through hospital choice models: A comparison of disaggregate and aggregate models. *Socio-Economic Planning Sciences*, *30*(2), 139–153.

Allison, P. D. (2008). Convergence failures in logistic regression. *SAS Global Forum*, *360*, 1–11.

American Hospital Association. (2017). *AHA hospital statistics: A comprehensive reference for analysis and comparison of hospital trends*. Chicago, IL: Health Forum.

Austin, P. C., & Van Walraven, C. (2011). The Mortality Risk Score and the ADG Score: two points-based scoring systems for the Johns Hopkins Aggregated Diagnosis Groups (ADGs) to predict mortality in a general adult population cohort in Ontario, Canada. *Medical Care*, *49*(10), 940–947.

Barker, E. H., & Tranmer, J. (2019). *Untitled* (Working Paper). Kingston, ON.

Beckert, W., Christensen, M., & Collyer, K. (2012). Choice of NHS-funded hospital services in England. *The Economic Journal*, *122*(560), 400–417.

Beukers, P., Kemp, R., & Varkevisser, M. (2014). Patient hospital choice for hip replacement: Empirical evidence from the Netherlands. *European Journal of Health Economics*, *15*(9), 927–936.

Boulanger, J. M., Lindsay, M. P., Gubitz, G., Smith, E. E., Stotts, G., Foley, N., … Butcher, K. (2018). Canadian stroke best practice recommendations for acute stroke management: Prehospital, emergency department, and acute inpatient stroke care. *International Journal of Stroke*, *13*(9), 949–984.

Canadian Institute for Health Information. (2016). *Hospital Standardized Mortality Ratio: Technical Notes*. Ottawa, ON: Canadian Institute for Health Information.

Capps, C., Dranove, D., & Lindrooth, R. (2010). Hospital closure and economic efficiency. *Journal of Health Economics*, *29*(1), 87–109.

Capps, C., Dranove, D., & Satterthwaite, M. (2003). Competition and market power in option demand markets. *RAND Journal of Economics*, *34*(4), 737–763.

Chen, Y., & Gayle, P. G. (2019). Mergers and product quality: Evidence from the airline industry. *Industrial Journal of Industrial Organization*, *62*, 96–135.

Cooper, Z., Gibbons, S., Jones, S., & McGuire, A. (2011). Does hospital competition save lives? Evidence from the English NHS patient choice reforms. *The Economic Journal*, *121*(554), F228–F260.

Cuellar, A. E., & Gertler, P. J. (2005). How the expansion of hospital systems has affected consumers. *Health Affairs*, *24*(1), 213–219.

Curtis, B., Gregory, D., Parfrey, P., Kent, G., Jelinski, S., Kraft, S., … Barrett, B. (2005). Quality of medical care during and shortly after acute care restructuring in Newfoundland and Labrador. *Journal of Health Services Research & Policy*, *10*(2 Suppl), 38–47.

Dolan, D., Grainger, J., MacCallum, N., Creatura, D., Forrester, J., & Shiller, S. (2012). The Institute for Clinical Evaluative Sciences: 20 years and counting. *Healthcare Quarterly*, *15*(4), 19–21.

Dranove, D., & Lindrooth, R. (2003). Hospital consolidations and costs: Another look at the evidence. *Journal of Health Economics*, *22*(6), 983–997.

Fan, Y. (2013). Ownership consolidation and product quality: A study of the U.S. daily newspaper market. *The American Economic Review*, *103*(5), 1–37.

Gardner, B. (2006). *Speaking Notes: Community Driven Planning, Priority Setting and Governance*. Toronto, ON: The Wellesley Institute.

Gaynor, M., Ho, K., & Town, R. J. (2015). The industrial organization of health-care markets. *Journal of Economic Literature*, *53*(2), 235–284.

Gaynor, M., Laudicella, M., & Propper, C. (2012). Can governments do it better? Merger mania and hospital outcomes in the English NHS. *Journal of Health Economics*, *31*, 528–543.

Gaynor, M., & Town, R. (2012a). Competition in health care markets. In *Handbook of Health Economics* (Vol. 2, pp. 499–637). Amsterdam: Elsevier North-Holland.

Gaynor, M., & Town, R. (2012b). *The impact of hospital consolidation--update. The Synthesis Project*. Princeton, NJ: Robert Wood Johnson Foundation.

Gutacker, N., Siciliani, L., Moscelli, G., & Gravelle, H. (2016). Choice of hospital: Which type of quality matters? *Journal of Health Economics*, *50*, 230–246.

Hanlon, N. T. (1998). *Administering the Trim Line: Restructuring in Three Ontario Hospital Settings* (Unpublished doctoral dissertation). Department of Geography, Queen's University: Kingston, ON.

Hayford, T. B. (2012). The impact of hospital mergers on treatment intensity and health outcomes. *Health Services Research*, *47*(3 Pt 1), 1008–1029.

Health Services Restructuring Commission. (2000). *Looking Back, Looking Forward: The Ontario Health Services Restructuring Commission (1996-2000): A Legacy Report*. Ottawa, ON: Canadian Health Services Research Foundation.

Hill, J. (2008). Discussion of research using propensity-score matching: Comments on "A critical appraisal of propensity-score matching in the medical literature between 1996 and 2003" by Peter Austin, Statistics in Medicine. *Statistics in Medicine*, *27*, 2055–2061.

Ho, V., & Hamilton, B. H. (2000). Hospital mergers and acquisitions: Does market consolidation harm patients? *Journal of Health Economics*, *19*(5), 767–791.

Hodgkin, D. (1996). Specialized service offerings and patients' choice of hospital: The case of cardiac catheterization. *Journal of Health Economics*, *15*(3), 305–332.

Howard, D. H. (2006). Do report cards influence hospital choice? The case of kidney transportation. *INQUIRY: The Journal of Health Care Organization, Provision, and Financing*, *43*(2), 150–159.

Hutchings, A., Allen, P., Fulop, N., King, A., Protopsaltis, G., Normand, C., & Walters, R. (2003). The process and impact of trust mergers in the National Health Service: A Financial Perspective. *Public Money & Management*, *23*(2), 103–112.

Johns Hopkins Bloomberg School of Public Health. (2009). *The Johns Hopkins ACG System: Excerpt from Technical Reference Guide Version 9.0*. Baltimore, MD: Health Services Research & Development Center.

Kourentzes, N., Rostami-Tabar, B., & Barrow, D. K. (2017). Demand forecasting by temporal aggregation: Using optimal or multiple aggregation levels? *Journal of Business Research*, *78*, 1–9.

Kralj, B. (2009). *Measuring Rurality - RIO 2008 BASIC: Methodology and Results*. Toronto, ON: Ontario Medical Association.

Kwoka, J., & Kilpatrick, S. (2018). Nonprice effects of mergers: issues and evidence. *The Antitrust Bulletin*, *63*(2), 169–182.

Luft, H. S., Peltzman, D. J., Mcphee, S. J., Garnick, D. W., Mark, D. H., Phibbs, C. S., & Lichtenberg, E. (1990). Does quality influence choice of hospital? *The Journal of the American Medical Association*, *263*(21), 2899–2906.

MacKinnon, J. G., & Webb, M. D. (2016). Online appendix for: Wild bootstrap inference for wildly different cluster sizes. *Journal of Applied Econometrics*.

Mainardi, S. (2007). Unequal access to public healthcare facilities: Theory and measurement revisited. *Surveys in Mathematics and Its Applications*, *2*, 91–112.

Marchildon, G. P. (2013). *Health Systems in Transition: Canada* (2nd ed.). Toronto, ON: University of Toronto Press.

McFadden, D. (1974). Conditional logit analysis of qualitative choice behaviour. In P. Zarembka (Ed.), *Frontiers in Econometrics, Volume 4* (pp. 105–142). New York: Academic Press.

McNamara, P. E. (1999). Welfare effects of rural hospital closures: A nested logit analysis of the demand for rural hospital services. *American Journal of Agricultural Economics*, *81*(3), 686–691.

Moscelli, G., Siciliani, L., Gutacker, N., & Gravelle, H. (2016). Location, quality and choice of hospital: Evidence from England 2002-2013. *Regional Science and Urban Economics*, *60*, 112–124.

Moscone, F., Tosetti, E., & Vittadini, G. (2012). Social interaction in patients' hospital choice: Evidence from Italy. *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, *175*(2), 453–472.

Mutter, R. L., Romano, P. S., & Wong, H. S. (2011). The effects of US hospital consolidations on hospital quality. *International Journal of the Economics of Business*, *18*(1), 109–126.

Naing, N. N. (2000). Easy way to learn standardization: Direct and indirect methods. *The Malaysian Journal of Medical Sciences: MJMS*, *7*(1), 10–15.

Noether, M., & May, S. (2017). *Hospital Merger Benefits: Views from Hospital Leaders and Econometric Analysis*. Boston, MA: Charles River Associates.

Oliver, L. L., & Leibenluft, R. F. (2015). A mixed bag: Sorting out efficiencies arguments in hospital mergers. *Antitrust*, *30*(1), 18–24.

Ontario Health Coalition. (2014a). *From Mega-Mergers to Reduced Parking Fees: A Comparision of the Ontario Provincial Parties' Health Care Platforms*. North York, ON: Ontario Health Coalition.

Ontario Health Coalition. (2014b). *Ontario Hospital Beds Staffed and in Operation 1990 to 2014*. North York, ON: Ontario Health Coalition.

Ontario Health Coalition. (2019). *List of local hospital and health services cuts/mergers/privatization*. North York, ON: Ontario Health Coalition.

Pérez, C. E. (2002). Ontario hospitals--mergers, shorter stays and readmissions. *Health*

Reports, *14*(1), 25–36.

Perry, B. Y. J. H., & Cunningham, R. H. (2013). Effective defenses of hospital mergers in concentrated markets. *Antitrust*, *27*(2), 43–47.

Philipon, D. J., & Braithwaite, J. (2008). Health system organization and governance in Canada and Australia: A comparison of historical developments, recent policy changes and future implications. *Healthcare Policy*, *4*(1), e168–e186.

Prince, J. T., & Simon, D. H. (2017). The impact of mergers on quality provision: Evidence from the airline industry. *The Journal of Industrial Economics*, *LXV*(No. 2), 336–362.

Public Hospital Act, R.S.O. 1990. (2017). Toronto, ON: Government of Ontario.

Rhee, C., Jones, T. M., & Hamad, Y. (2019). Prevalence, underlying causes, and preventability of sepsis-associated mortality in US acute care hospitals. *JAMA Network Open*, *2*(2), e187571.

Romano, P. S., & Balan, D. J. (2011). A retrospective analysis of the clinical quality effects of the acquisition of Highland Park hospital by Evanston Northwestern healthcare. *International Journal of the Economics of Business*, *18*(1), 45–64.

Rosko, M., Wong, H. S., & Mutter, R. L. (2018). Characteristics of high- and low-efficiency hospitals. *Medical Care Research and Review*, *75*(4), 454–478.

Rubin, H. R., Pronovost, P., & Diette, G. B. (2001). The advantages and disadvantages of process-based measures of health care quality. *International Journal for Quality in Health Care*, *13*(6), 469–474.

Russell, L. (2016). Market Power Effects of College and University Mergers. *MIT Working Paper*, (1122374), 1–44.

Santos, R., Gravelle, H., & Propper, C. (2017). Does quality affect patients' choice of doctor? Evidence from England. *The Economic Journal*, *127*(600), 445–494.

Schilling, R. . (1997). Spherocytosis, splenectomy, strokes, and heart attacks. *The Lancet*, *350*(9092), 1677–1678.

Schmitt, M. (2017). Do hospital mergers reduce costs? *Journal of Health Economics*, *52*, 74–94.

Scott, J. A., & Dunkelberg, W. C. (2003). Bank Mergers and Small Firm Financing. *Journal of Money, Credit, and Banking*, *35*(6 (Part 1)), 999–1017.

Scott, S., Lund, J. N., Gold, S., Elliott, R., Vater, M., Chakrabarty, M. P., … Williams, J. P. (2014). An evaluation of POSSUM and P-POSSUM scoring in predicting post-operative mortality in a level 1 critical care setting. *BMC Anesthesiology*, *14*(1), 104.

Sinclair, D., Rochon, M., & Leatt, P. (2005). *Riding the Third Rail: The Story of Ontario's Health Services Restructuring Commission, 1996-2000*. Montreal, QC: McGill-Queen's University Press.

Spang, H. R., Arnould, R. J., & Bazzoli, G. J. (2009). The effect of non-rural hospital mergers and acquisitions: An examination of cost and price outcomes. *The Quarterly Review of Economics and Finance*, *49*, 323–342.

Spang, H. R., Bazzoli, G. J., & Arnould, R. J. (2001). Hospital mergers and savings for consumers: exploring new evidence. *Health Affairs*, *20*(4), 150–158.

Steven, A. B., Yazdi, A. A., & Dresner, M. (2016). Mergers and service quality in the airline industry: A silver lining for air traders? *Transportation Research Part E: Logistics and Transportation Review*, *89*, 1–13.

Tay, A. (2003). Assessing competition in hospital care markets: The Importance of accounting for quality differentiation. *The RAND Journal of Economics*, *34*(4), 786–814.

Taylor, G., Gravel, D., Matlow, A., Embree, J., LeSaux, N., Johnston, L., … others. (2016). Assessing the magnitude and trends in hospital acquired infections in Canadian hospitals through sequential point prevalence surveys. *Antimicrobial Resistance & Infection Control*, *5*, 19.

Tijani-eniola, O. (2016). Hospital mergers: Mixed impact on price, quality of patient care, and hospital performance. *International Journal of Scientific and Research Publications*, *6*(1), 361–363.

Varkevisser, M., & Van Der Geest, S. A. (2007). Why do patients bypass the nearest hospital? An empirical analysis for orthopaedic care and neurosurgery in the Netherlands. *European Journal of Health Economics*, *8*(3), 287–295.

Wan, F. (2019). Matched or unmatched analyses with propensity-score-matched data? *Statistics in Medicine*, *38*, 289–300.

Weil, T. (2010). Hospital mergers: a panacea? *Journal of Health Services Research & Policy*, *15*(4), 251–253.

Appendix A: Supplementary Tables and Figures

Tables and Figures Supplementary to Chapter 4

Effect of varying the number of matches

TABLE A-1. TREATMENT EFFECT FOR MERGED HOSPITALS, BY NUMBER OF MATCHES (β_4)

	Matches					
	1	2	3	4	5	6
Patient outcomes						
HSMR	-2.48***	-8.85***	-9.92***	-9.53***	-10.53***	-15.40***
Average ALC LOS (days)	-2.14***	-1.13*	-1.47**	-1.09*	-0.90	-0.66
Average distance (km)	-0.44	-0.64	-0.95*	-0.86*	-0.83*	-0.71
30-day (std) mortality (%)						
AMI	-0.49	-1.63	-1.48	-1.38	-2.55	-7.72*
Pneumonia	-0.40	0.73	0.84	1.13	2.31	3.26
Sepsis	4.07*	4.39*	3.67	4.20*	4.41*	6.14***
Stroke	2.27	1.57	0.89	0.83	0.84	1.59
Average LOS (days)						
AMI	-0.26	0.16	-0.08	0.00	0.02	0.24
Pneumonia	-0.43*	-0.67***	-0.72***	-0.68***	-0.82***	-1.17***
Sepsis	-0.31	-0.13	-0.05	-0.13	-0.09	0.16
Stroke	-0.35	-0.41	-0.62	-0.56	-0.71	-1.04***
Hospital outcomes						
Occupancy (%)	-9.47***	-10.71***	-10.07***	-10.00***	-10.88***	-10.26***
Total beds	-5.26	-19.10	0.94	-8.98	-23.73	-2.30
Volume (discharges) (10s)	-167.42***	-117.58***	-47.49	-72.52	-104.66*	-97.47*

table continued on next page

TABLE A-1 continued

Comp. to UPP ($00,000s)	-477.51***	-574.64***	-619.47***	-558.74***	-543.03***	-444.37***
% spent on admin (%)	0.24	0.10	0.16	0.21	0.27	0.27
Total margin (%)	1.18	-4.21	-4.61	-4.65	-3.81	-2.92

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Proportion weights based on pseudo-hospital volume are used in estimation.

TABLE A-2. TREATMENT EFFECT FOR MERGED HOSPITALS WITH ACUTE CARE SERVICE CLOSURES, BY NUMBER OF MATCHES ($\beta_4 + \beta_5$)

	Matches					
	1	2	3	4	5	6
Patient outcomes						
HSMR	-3.46***	-8.69***	-9.42***	-9.06***	-10.20***	-14.65***
Average ALC LOS (days)	-1.75**	-0.89	-1.42**	-1.10*	-0.93	-0.71
Average distance (km)	-0.24	-0.24	-0.52	-0.44	-0.39	-0.25
30-day (std) mortality (%)						
AMI	1.11	-0.89	-1.02	-0.74	-2.48	-8.56*
Pneumonia	-0.57	0.31	0.60	0.98	2.19	3.21
Sepsis	-0.37	-0.53	-0.94	-0.26	0.11	1.83
Stroke	0.49	-0.33	-0.83	-0.74	-0.70	-0.04
Average LOS (days)						
AMI	-0.15	0.25	0.01	0.10	0.13	0.35
Pneumonia	0.46	0.01	-0.04	0.02	-0.12	-0.48
Sepsis	0.06	0.00	0.15	0.07	0.19	0.46
Stroke	-0.10	-0.33*	-0.49*	-0.41*	-0.57*	-0.94**
Hospital outcomes						
Occupancy (%)	-10.67***	-12.88***	-12.27***	-12.33***	-13.12***	-12.56***
Total beds	-110.72**	-137.38***	-118.65***	-129.04***	-141.37***	-118.16***
Volume (discharges) (10s)	-667.82***	-628.95***	-561.86***	-589.55***	-619.22***	-614.52***
Comp. to UPP ($00,000s)	-477.51***	-574.64***	-619.47***	-558.74***	-543.03***	-444.37***
% spent on admin (%)	0.24	0.10	0.16	0.21	0.27	0.27
Total margin (%)	1.18	-4.21	-4.61	-4.65	-3.81	-2.92

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Proportion weights based on pseudo-hospital volume are used in estimation.

Condition-specific outcomes, alternate specifications

TABLE A-3. CONDITION-SPECIFIC PATIENT OUTCOMES, USING ONLY PATIENTS ADMITTED BY AMBULANCE

	30-day indirectly standardized mortality				Average length of stay			
	AMI	Pneu.	Sepsis	Stroke	AMI	Pneu.	Sepsis	Stroke
Merged	43.31	45.28*	60.56	-32.89	3.99	14.21**	13.93**	14.63*
	(34.33)	(22.87)	(41.69)	(38.14)	(3.90)	(4.50)	(5.38)	(6.81)
Merged w/Closure	-21.36*	-0.95	-3.09	21.83	-5.05**	-11.28***	-7.09*	-10.27**
	(8.28)	(12.50)	(21.10)	(12.67)	(1.57)	(2.73)	(3.14)	(3.44)
Post	-1.12	-0.77	-2.19	6.75***	0.43*	0.39	0.15	-0.25
	(1.35)	(1.48)	(3.05)	(1.98)	(0.21)	(0.32)	(0.37)	(0.50)
Post * Merged	-0.98	1.29	2.76	-1.28	-0.56**	-1.03***	-0.21	-0.26
	(1.31)	(1.71)	(2.61)	(1.94)	(0.20)	(0.30)	(0.40)	(0.45)
Post * Merged w/Closure	1.90	-0.35	-4.87	-3.50	0.24	0.68	0.29	-0.64
	(1.58)	(1.71)	(3.45)	(3.91)	(0.31)	(0.38)	(0.54)	(0.57)
Mean outcome	24.01	32.24	46.56	32.69	10.46	13.90	15.97	16.46
R^2	0.57	0.60	0.33	0.27	0.75	0.76	0.82	0.67
F	9.99	11.39	5.56	8.39	24.23	34.37	36.42	23.90
N	503	503	503	503	503	503	503	503

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Proportion weights based on pseudo-hospital volume are used in estimation.

Recall that in this extended-DID specification Post = 1 for merged hospitals AND their matched control in the post-treatment period. It should not be interpreted as a part of the treatment effect.

TABLE A-4. MORTALITY RATE OUTCOMES BY STANDARDIZATION METHOD

	AMI	Pneumonia	Sepsis	Stroke
	30-day indirectly standardized mortality			
Post * Merged	-1.38	1.13	4.20*	0.83
	(2.40)	(2.71)	(2.04)	(1.49)
Post * Merged w/Closure	0.63	-0.15	-4.46	-1.58
	(1.66)	(1.87)	(2.54)	(1.57)
Mean outcome	18.34	26.75	38.66	26.25
R^2	0.28	0.53	0.43	0.42
F	6.91	6.92	8.34	9.65
	30-day directly standardized mortality			
Post * Merged	-0.33	0.12	4.72***	2.35***
	(0.48)	(0.48)	(0.95)	(0.68)
Post * Merged w/Closure	1.69*	-0.27	-3.46*	-0.68
	(0.66)	(0.62)	(1.38)	(0.77)
Mean outcome	20.40	23.60	36.16	22.37
R^2	0.69	0.70	0.78	0.60
F	18.39	24.98	36.26	21.50
	Crude mortality			
Post * Merged	-0.30	0.47	2.74**	1.32
	(0.55)	(0.58)	(0.97)	(0.72)
Post * Merged w/Closure	1.05	-0.71	-2.71*	-0.97
	(0.76)	(0.65)	(1.27)	(0.80)
Mean outcome	19.62	24.65	36.61	24.18
R^2	0.58	0.69	0.61	0.55
F	11.22	20.45	13.29	13.87
Observations	503	503	503	503

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Proportion weights based on pseudo-hospital volume are used in estimation.

TABLE A-5. CONDITION-SPECIFIC PATIENT OUTCOMES, PATIENTS ADMITTED WITHIN 6 MONTHS OF MERGER DATE EXCLUDED

	30-day indirectly standardized mortality				Average length of stay			
	AMI	Pneu.	Sepsis	Stroke	AMI	Pneu.	Sepsis	Stroke
Merged	32.83	0.00	-70.92*	-43.89	4.79*	6.41**	-1.74	6.95
	(29.80)	(59.42)	(28.21)	(47.99)	(2.05)	(2.20)	(4.59)	(3.61)
Merged w/Closure	-32.02	-5.92	8.99	7.29	-1.90*	-5.86***	-0.83	-9.40***
	(20.71)	(18.22)	(12.47)	(10.29)	(0.94)	(1.67)	(1.93)	(2.11)
Post (for Treatment or Match)	-1.67	-1.87	-2.43	3.89*	0.34*	0.55	-0.26	0.34
	(7.16)	(2.25)	(2.11)	(1.67)	(0.16)	(0.32)	(0.42)	(0.48)
Post * Merged	-3.52	0.88	3.83*	1.06	0.00	-0.78***	0.22	-0.65
	(3.18)	(2.13)	(1.87)	(1.47)	(0.16)	(0.23)	(0.40)	(0.40)
Post * Merged w/Closure	0.78	0.36	-3.61	-1.10	0.12	0.61	0.14	0.05
	(1.84)	(1.93)	(2.53)	(1.62)	(0.24)	(0.32)	(0.55)	(0.48)
Mean outcome	18.78	25.63	38.29	26.12	9.87	12.78	15.72	14.40
R^2	0.24	0.52	0.43	0.37	0.85	0.83	0.80	0.66
F	5.73	7.58	9.13	10.14	54.19	74.67	58.19	26.08
N	503	503	503	503	503	503	503	503

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Proportion weights based on pseudo-hospital volume are used in estimation.

Recall that in this extended-DID specification Post = 1 for merged hospitals AND their matched control in the post-treatment period. It should not be interpreted as a part of the treatment effect.

Full-panel differences-in-differences

TABLE A-6. NON-DIAGNOSIS SPECIFIC PATIENT OUTCOMES, FULL-PANEL DID

	HSMR	Average ALC length of stay	Average distance travelled
Merged	-5.25^{***}	0.28	18.43^{***}
	(1.52)	(2.59)	(1.60)
Merged with Closure	1.82	-2.80^{*}	-16.91^{***}
	(1.90)	(1.34)	(1.38)
Post (for Treatment)	-0.88	0.82	-1.30^{**}
	(0.77)	(0.72)	(0.44)
Post * Merged with Closure	-1.01	-1.66^{*}	-0.52
	(0.58)	(0.85)	(0.49)
Mean outcome	84.62	11.38	17.40
R^2	0.67	0.64	0.86
F	322.16	33.74	105.10
Observations	2297	2297	2297

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Analytical weights based on propensity scores and pseudo-hospital volume are used in estimation.

In this specification, Post = 1 only for the treatment group and the corresponding coefficient can be interpreted as part of the treatment effect.

TABLE A-7. DIAGNOSIS SPECIFIC PATIENT OUTCOMES, FULL-PANEL DID

	30-day indirectly standardized mortality				Average length of stay			
	AMI	Pneu.	Sepsis	Stroke	AMI	Pneu.	Sepsis	Stroke
Merged	2.30	8.31^{**}	-14.53^{*}	-13.61	-2.23^{*}	1.56^{*}	3.55^{**}	-3.08^{***}
	(3.43)	(3.05)	(7.31)	(15.90)	(0.92)	(0.73)	(1.15)	(1.77)
Merged	3.31	-7.63^{***}	2.61	-22.34	2.82^{***}	1.77^{**}	1.37	3.96^{***}
w/Closure	(3.01)	(1.87)	(4.34)	(17.61)	(0.82)	(0.60)	(0.74)	(1.00)
Post (for	-2.75^{*}	-0.66	1.84	-4.92	-0.38	-0.13	0.22	-0.66
Treatment)	(1.15)	(0.90)	(2.25)	(4.47)	(0.26)	(0.22)	(0.36)	(0.42)
Post * Merged	1.98	-0.31	-1.49	-3.13	0.50	0.38	0.23	0.05
w/Closure	(1.26)	(1.08)	(2.41)	(4.55)	(0.31)	(0.27)	(0.45)	(0.51)
Mean outcome	16.68	21.82	37.38	28.68	10.03	12.84	15.86	14.12
R^2	0.17	0.27	0.27	0.06	0.69	0.80	0.77	0.58
N	2182	2282	2091	2078	2179	2280	2090	2068

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Analytical weights based on propensity scores and pseudo-hospital volume are used in estimation.

In this specification, Post = 1 only for the treatment group and the corresponding coefficient can be interpreted as part of the treatment effect.

TABLE A-8. HOSPITAL OUTCOMES, FULL-PANEL DID

	Occupancy	Total Beds (10s)	Volume (10s)	Compensation to UPP ($00,000)	% on admin	Total margin
Merged	-9.06	-18.48*	-735.18***	-2127.92***	0.29	-65.88***
	(5.71)	(7.32)	(131.86)	(217.62)	(0.33)	(6.82)
Merged w/Closure	38.53*** (4.79)	66.27*** (6.94)	1860.52*** (139.89)	3061.21*** (212.25)	-1.99*** (0.33)	8.40 (7.06)
Post	-11.05***	-7.63*	-67.04	-46.43	0.25	-8.58**
	(1.88)	(3.14)	(41.63)	(52.07)	(0.18)	(3.04)
Post * Merged w/Closure	0.32 (2.14)	-10.94** (3.77)	-372.95*** (65.04)	132.66 (84.40)	-0.15 (0.19)	13.85*** (3.49)
Mean outcome	90.02	37.31	1638.03	1389.35	5.34	-22.84
R^2	0.64	0.88	0.97	0.94	0.51	0.81
F	50.17	140.77	821.88	196.86	43.70	137.44
N	1476	1476	1476	1476	1476	1476

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Analytical weights based on propensity scores and pseudo-hospital volume are used in estimation.

In this specification, Post = 1 only for the treatment group, and the corresponding coefficient can be interpreted as part of the treatment effect.

Common trends tests

TABLE A-9. NON-DIAGNOSIS SPECIFIC PATIENT OUTCOMES, COMMON TRENDS TEST

	HSMR	Average ALC length of stay	Avg distance travelled
Merged	120.00^{*}	-27.90	11.06
	(57.32)	(49.15)	(10.66)
Merged with Closure	-74.67^{***}	-36.27	-14.51^{***}
	(21.92)	(25.99)	(2.64)
Post (for Treatment or Match)	6.90^{**}	1.63	1.29^{**}
	(2.13)	(1.74)	(0.42)
Post * Merger	-9.18^{***}	-1.50	-0.98
	(2.38)	(1.61)	(0.56)
Post * Merged with Closure	0.68	0.24	0.04
	(1.42)	(0.93)	(0.66)
Year Before Merger	0.45	-0.42	0.03
	(1.03)	(0.94)	(0.78)
Two Years Before Merger	1.29	0.21	0.53
	(0.95)	(1.14)	(0.73)
Year Before Merger * Merged with Closure	0.92	0.85	0.91
	(1.38)	(1.04)	(1.41)
Two Years Before Merger * Merged with Closure	0.12	1.33	-0.69
	(1.22)	(1.36)	(0.86)
Mean outcome	94.78	93.92	9.19
R^2	0.45	0.44	0.72
F	14.09	26.40	45.37
Observations	504	504	504

Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Proportion weights based on pseudo-hospital volume are used in estimation. In this specification, significant effects in the highlighted rows would provide evidence of anticipation effects.

TABLE A-10. DIAGNOSIS SPECIFIC PATIENT OUTCOMES, COMMON TRENDS TEST

	30-day indirectly (std) mortality				Average length of stay			
	AMI	Pneu.	Sepsis	Stroke	AMI	Pneu.	Sepsis	Stroke
Merged	26.82	18.14	-180.83**	25.36	-1.93	3.09	-1.41	9.91
	(29.03)	(46.30)	(62.74)	(71.83)	(3.59)	(5.04)	(5.23)	(8.79)
Merged w/Closure	-22.28	7.42	7.44	-1.26	-3.96***	-6.34***	-3.53***	-10.40***
	(12.62)	(31.63)	(14.16)	(8.59)	(1.13)	(1.82)	(2.59)	(2.03)
Post (for Treatment or Match)	-6.10	-2.08	-1.57	5.53**	0.32*	0.33	-0.05	-0.09
	(6.37)	(2.65)	(2.45)	(1.96)	(0.15)	(0.32)	(0.35)	(0.40)
Post * Merged	-1.43	1.40	5.81*	0.47	-0.02	-0.73**	0.07	-0.67
	(2.52)	(2.81)	(2.39)	(1.71)	(0.18)	(0.25)	(0.43)	(0.45)
Post * Merged w/Closure	0.71	-0.65	-5.27	0.19	0.20	0.64	0.29	0.32
	(1.96)	(1.96)	(3.04)	(1.91)	(0.27)	(0.35)	(0.50)	(0.53)
Year Before Merger	1.11	0.94	3.74	-2.75	0.15	-0.17	-0.01	-0.21
	(1.87)	(1.59)	(3.19)	(1.94)	(0.31)	(0.31)	(0.69)	(0.71)
Two Years Before Merger	-1.45	0.42	4.54	0.85	-0.27	-0.10	0.97	-0.38
	(3.11)	(1.59)	(2.96)	(2.66)	(0.19)	(0.29)	(0.52)	(0.55)
Year Before Merger * Merged with Closure	-1.98	-2.71	-0.42	4.54	0.14	-0.23	-0.13	-0.04
	(3.57)	(2.19)	(4.60)	(2.77)	(0.48)	(0.48)	(0.92)	(0.89)
Two Years Before Merger * Merged with Closure	2.38	0.13	-3.89	4.63	0.39	-0.06	0.60	0.96
	(3.66)	(2.29)	(4.35)	(3.18)	(0.35)	(0.44)	(0.71)	(0.86)
Mean outcome	18.78	25.63	38.29	26.12	9.87	12.78	15.72	14.40
R^2	18.34	26.75	38.66	26.25	9.88	12.81	15.78	14.41
F	0.28	0.53	0.44	0.43	0.84	0.84	0.81	0.68
N	504	504	504	504	504	504	504	504

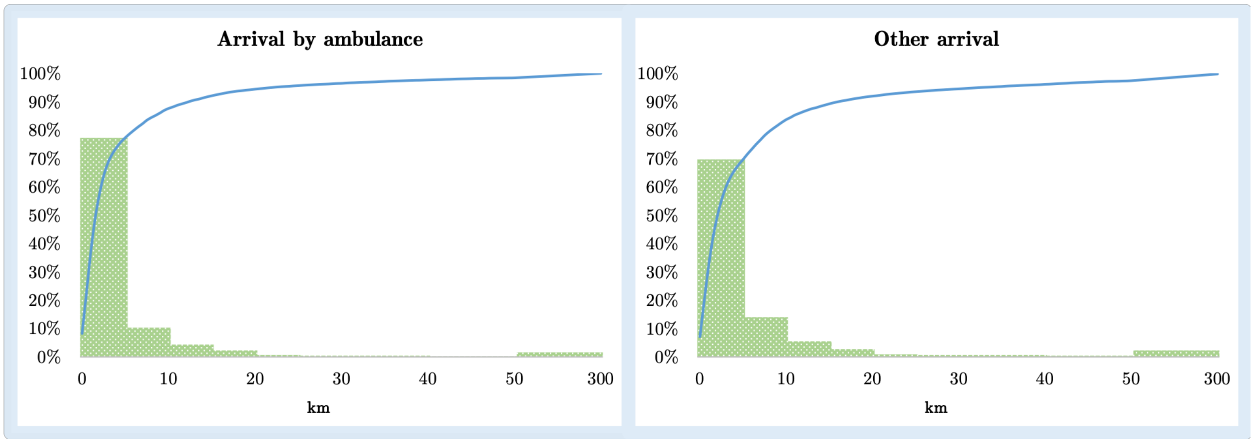
Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Proportion weights based on pseudo-hospital volume are used in estimation. In this specification, significant effects in the highlighted rows would provide evidence of anticipation effects.

TABLE A-11. HOSPITAL OUTCOMES, COMMON TRENDS TEST

	Occupancy	Total Beds (10s)	Volume (10s)	Compensation to UPP ($00,000)	% on admin	Total margin
Merged	-41.22	-44.34	-406.76	1323.99	-3.56	3.24
	(32.50)	(40.18)	(971.08)	(2326.27)	(1.96)	(42.08)
Merged w/Closure	-50.48***	-16.96	-1798.79***	-3152.74***	-2.19*	6.12
	(13.07)	(15.94)	(236.43)	(461.24)	(0.87)	(11.95)
Post (for Treatment or Match)	4.02*	3.31	164.10**	260.99*	0.11	2.58
	(1.93)	(4.94)	(55.63)	(107.46)	(0.18)	(2.12)
Post * Merged	-9.95***	-2.15	-49.90	-570.66***	0.20	-3.74
	(2.22)	(4.76)	(44.95)	(90.04)	(0.19)	(3.05)
Post * Merged w/Closure	-3.26	-14.67*	-529.21***	21.03	0.11	7.34
	(2.94)	(5.79)	(85.61)	(72.22)	(0.27)	(3.79)
Year Before Merger	0.34	-2.90	47.05	-51.58	-0.13	2.76
	(2.37)	(6.67)	(66.51)	(76.38)	(0.27)	(4.95)
Two Years Before Merger	0.04	-3.69	71.55	-8.29	0.08	1.69
	(2.76)	(6.46)	(68.37)	(65.54)	(0.27)	(4.55)
Year Before Merger * Merged with Closure	-0.05	-7.32	39.91	-8.91	0.50	-3.21
	(3.88)	(7.46)	(119.37)	(105.85)	(0.34)	(5.79)
Two Years Before Merger * Merged with Closure	-4.75	-7.75	-105.30	-88.86	0.32	0.93
	(4.15)	(7.48)	(121.60)	(114.14)	(0.37)	(5.36)
Mean outcome	86.98	64.52	2691.80	1845.96	5.40	-14.91
R^2	0.74	0.89	0.98	0.93	0.65	0.83
F	28.11	128.69	675.32	129.71	22.18	81.47
Observations	504	504	504	504	504	504

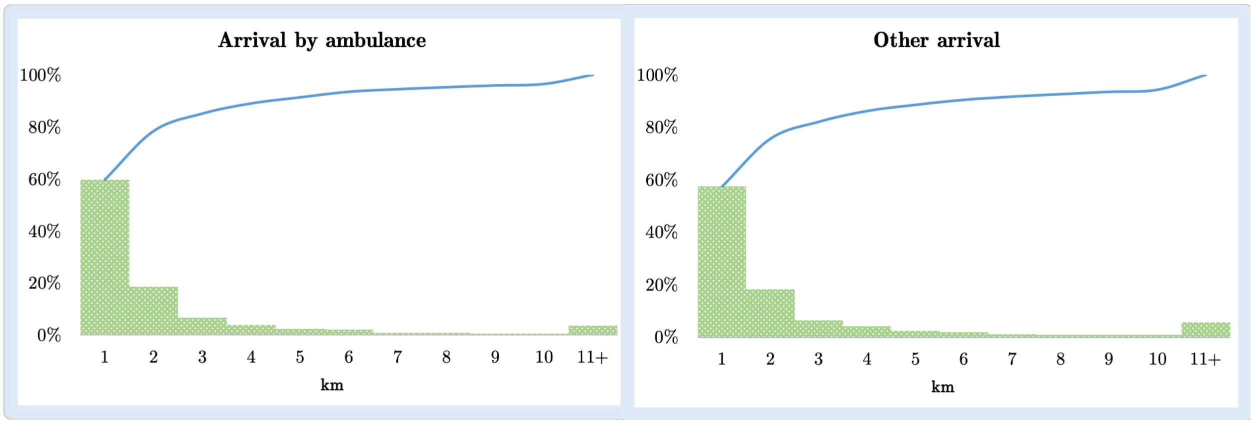
Notes: * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Robust standard errors reported in parentheses. Variable definitions in Table 3-1. Hospital controls, hospital fixed effects, and year fixed effects included. Analytical weights based on propensity scores and pseudo-hospital volume are used in estimation. In this specification, significant effects in the highlighted rows would provide evidence of anticipation effects.

Tables and Figures Supplementary to Chapter 5


Arrival by ambulance

Other arrival

Figure A-1. Histogram with cumulative frequency, distance between patient residence and their chosen hospital, by arrival type


Arrival by ambulance

Other arrival

Figure A-2. Histogram with cumulative frequency, patients who choose the Nth closest hospital, by arrival type

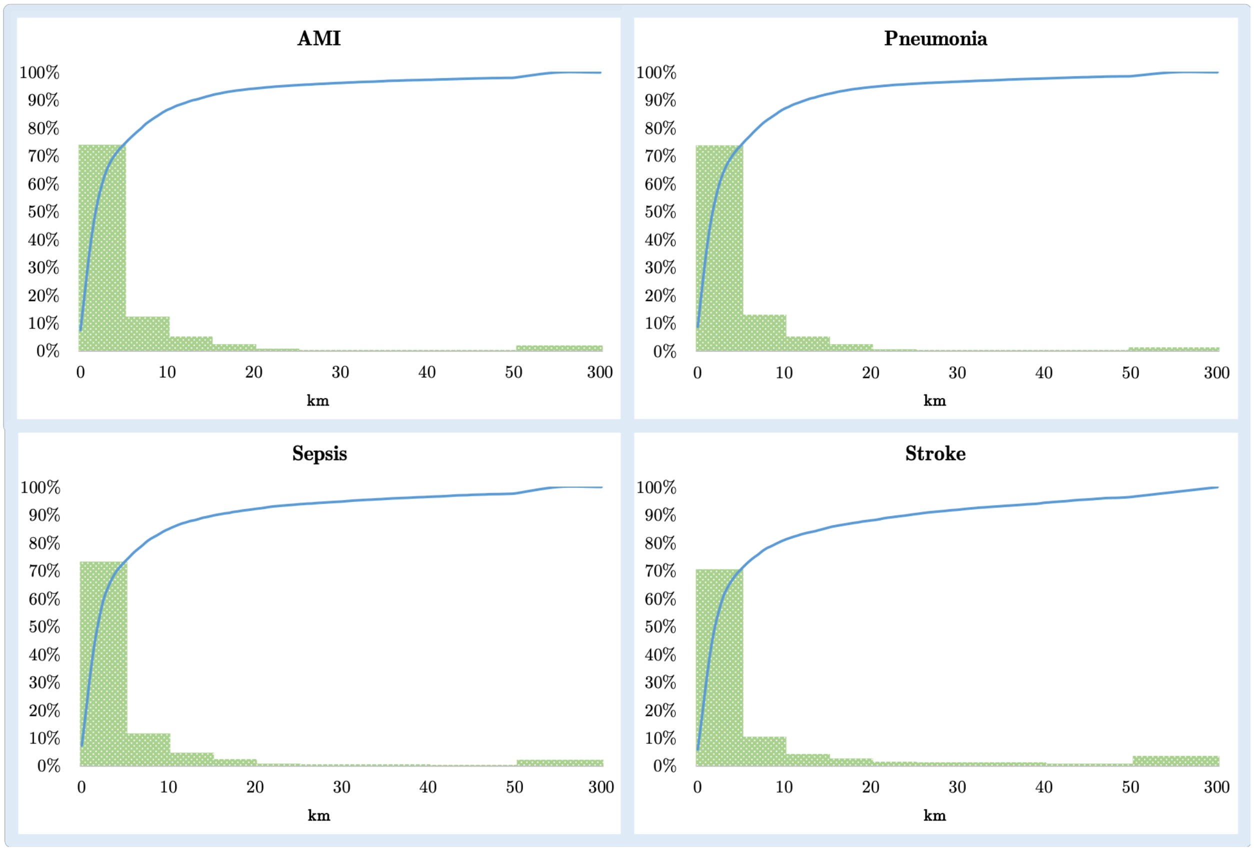


Figure A-3. Histogram with cumulative frequency, distance between patient residence and their chosen hospital, by condition

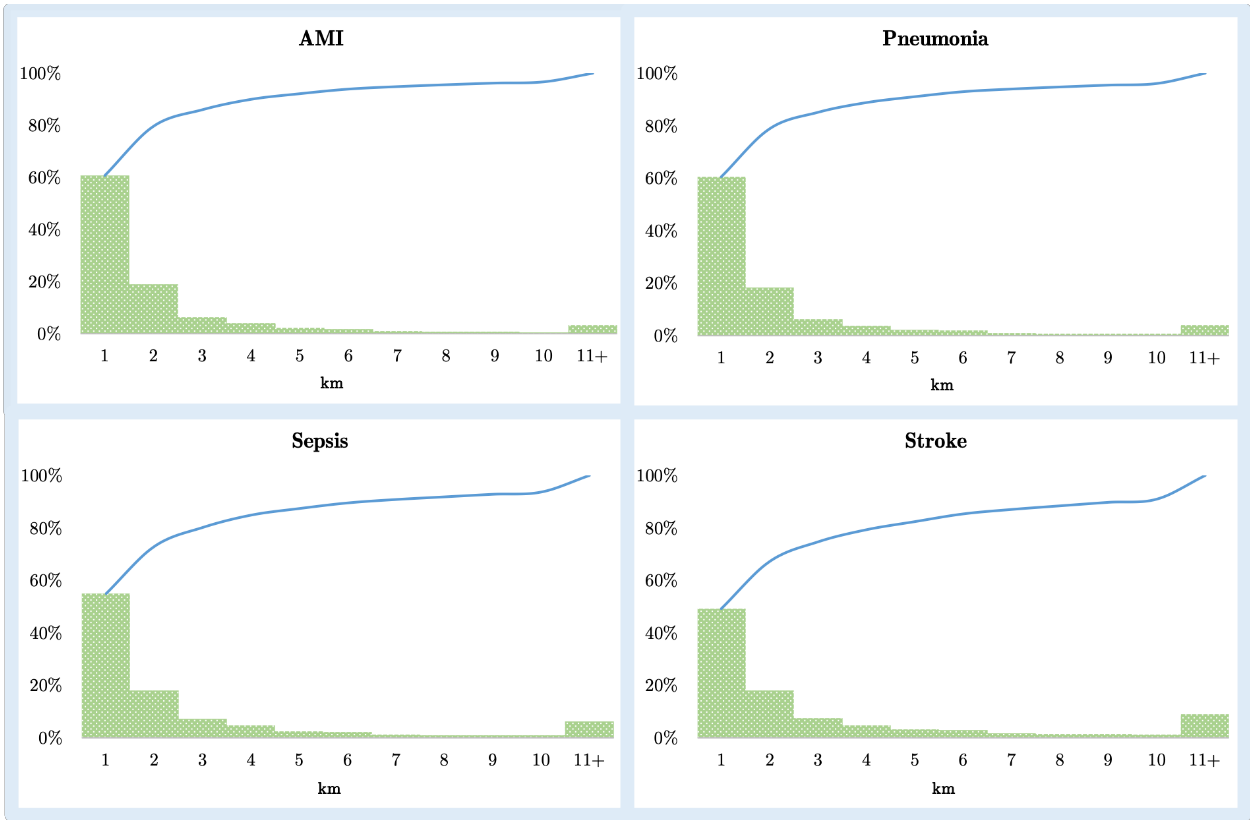


Figure A-4. Histogram with cumulative frequency, patients who choose the Nth closest hospital, by condition

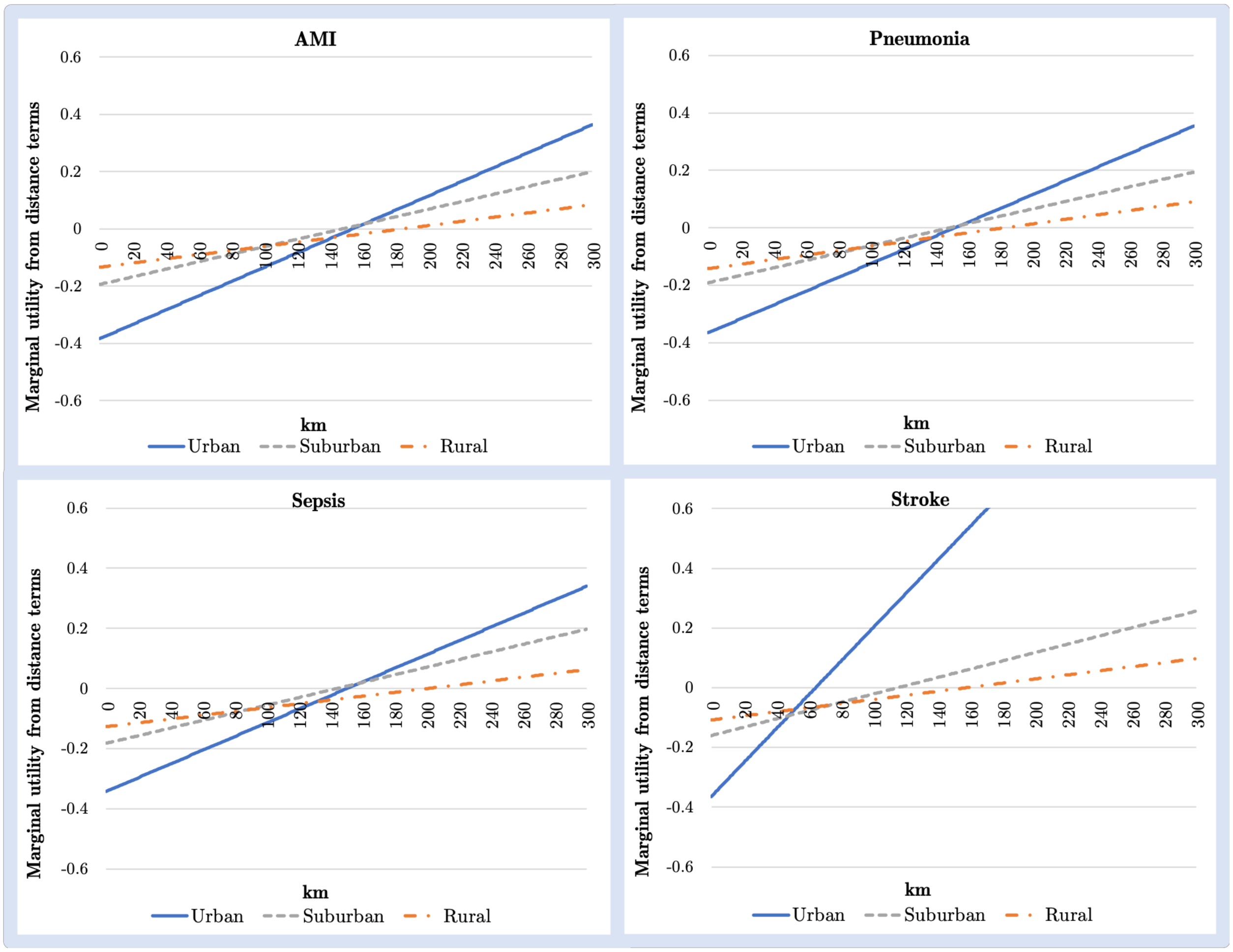


Figure A-5. Marginal utility from distance (km), specification with no distance-cubed term, reference person, by condition and location

TABLE A-12. PATIENT CHOICE ESTIMATION RESULTS, NO PATIENT CHARACTERISTICS

Variables	AMI	Pneumonia	Sepsis	Stroke
Distance (10km)	-3.780***	-3.268***	-3.329***	-2.880***
	(0.032)	(0.024)	(0.046)	(0.032)
Distance^2 (10km)	0.461***	0.329***	0.382***	0.270***
	(0.008)	(0.005)	(0.010)	(0.005)
Distance^3 (10km)	-0.0190***	-0.0107***	-0.0140***	-0.00680***
	(0.000)	(0.000)	(0.001)	(0.000)
Teaching status	-0.577***	-0.436***	-0.427***	-0.462***
	(0.032)	(0.026)	(0.051)	(0.039)
Hospital location: urban	1.105***	0.872***	1.325***	2.104***
	(0.045)	(0.037)	(0.076)	(0.069)
Hospital location: rural	-1.400***	-0.901***	-1.448***	-1.481***
	(0.064)	(0.051)	(0.116)	(0.111)
Lag volume (1000s)	0.0708***	0.0673***	0.0716***	0.0705***
	(0.002)	(0.001)	(0.002)	(0.002)
Lag specialization (%)	0.0440***	0.00657***	0.0507***	0.0653***
	(0.001)	(0.001)	(0.004)	(0.001)
Lag (std) mortality rate (%)	-0.0102***	-0.00195**	-0.000488	-0.00122
	(0.001)	(0.001)	(0.001)	(0.001)
Lag (std) readmission rate (%)	-0.00857***	-0.00343***	-0.000235	-0.0000874
	(0.001)	(0.000)	(0.000)	(0.000)
Lag HSMR (%)	-0.00221	-0.0118***	-0.0312***	-0.0103
	(0.005)	(0.002)	(0.005)	(0.006)
Lag ALC (days)	-0.0102***	0.00430**	0.00681*	0.00821**
	(0.002)	(0.002)	(0.003)	(0.003)
Lag high-risk occupancy (%)	0.0000945	0.0167***	0.0122***	0.00441**
	(0.001)	(0.001)	(0.002)	(0.001)
Observations	341790	483765	112953	168448
Pseudo R-squared	0.578	0.553	0.502	0.485

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Estimation by conditional logistic regression for patients admitted with sepsis and stroke. Choice set size of 10 hospitals. Marginal utility reported.

TABLE A-13. PATIENT CHOICE ESTIMATION RESULTS, AMI PATIENTS, ALTERNATE DISTANCE SPECIFICATIONS

Base	Interaction	Distance^2		Distance^3	
		Coefficient	s.e.	Coefficient	s.e.
Distance (10km)		-1.976***	(0.0350)	-2.555***	(0.0690)
	x Urban	-2.002***	(0.0420)	-3.082***	(0.0800)
	x Rural	0.455***	(0.0370)	0.872***	(0.0730)
	x Age	-0.0134***	(0.0010)	-0.0168***	(0.0020)
	x ADG Score	0.0147***	(0.0020)	0.00607*	(0.0030)
	x Female	0.0111	(0.0320)	-0.0326	(0.0570)
Distance^2 (10km)		0.0640***	(0.0020)	0.205***	(0.0130)
	x Urban	0.0646***	(0.0020)	0.837***	(0.0270)
	x Rural	-0.0223***	(0.0020)	-0.129***	(0.0120)
	x Age	0.000440***	(0.0000)	0.00184***	(0.0000)
	x ADG Score	-0.000438***	(0.0000)	0.000678	(0.0010)
	x Female	0.000353	(0.0020)	0.00907	(0.0100)
Distance^3 (10km)				-0.00523***	(0.0010)
	x Urban			-0.0535***	(0.0020)
	x Rural			0.00382***	(0.0010)
	x Age			-0.0000646***	(0.0000)
	x ADG Score			-0.000033	(0.0000)
	x Female			-0.000309	(0.0000)
Hospital location: urban		0.0969	(0.0690)	0.271***	(0.0710)
	x Urban	0.489**	(0.1870)	0.677***	(0.1540)
	x Rural	0.424**	(0.1380)	0.183	(0.1390)
		-2.199***	(0.1560)	-2.238***	(0.1550)
Hospital location: rural	x Urban	2.854***	(0.4300)	1.074**	(0.3470)
	x Rural	1.711***	(0.1740)	1.734***	(0.1730)
Lag volume (1000s)		0.112***	(0.0070)	0.102***	(0.0070)
	x Urban	-0.0542***	(0.0060)	-0.0453***	(0.0060)
	x Rural	0.00927	(0.0120)	0.0183	(0.0120)
	x Age	-0.000354**	(0.0000)	-0.000383**	(0.0000)
	x ADG Score	0.000943***	(0.0000)	0.00111***	(0.0000)
	x Female	-0.00108	(0.0030)	-0.000115	(0.0030)
Lag specialization (%)		0.0534***	(0.0040)	0.0578***	(0.0040)
	x Urban	-0.00898*	(0.0040)	-0.0155***	(0.0040)
	x Rural	-0.0124*	(0.0050)	-0.0146**	(0.0050)
	x Age	0.0000451	(0.0000)	0.0000627	(0.0000)
	x ADG Score	-0.000551***	(0.0000)	-0.000620***	(0.0000)

table continued on next page

TABLE A-13 CONTINUED

	x Female	-0.00071	(0.0030)	-0.00165	(0.0030)
Lag (std) mortality rate (%)		-0.00632***	(0.0020)	-0.00638***	(0.0020)
	x Urban	-0.00828***	(0.0020)	-0.00965***	(0.0020)
	x Rural	0.000732	(0.0020)	0.000965	(0.0020)
	x Age	0.000260***	(0.0000)	0.000252***	(0.0000)
	x ADG Score	-0.0000153	(0.0000)	-0.0000426	(0.0000)
	x Female	0.00116	(0.0020)	0.000976	(0.0020)
Lag (std) readmission rate (%)		-0.00790***	(0.0010)	-0.00841***	(0.0010)
	x Urban	-0.00329*	(0.0020)	-0.0032	(0.0020)
	x Rural	0.00296*	(0.0010)	0.00335*	(0.0010)
	x Age	0.000154***	(0.0000)	0.000172***	(0.0000)
	x ADG Score	0.0000559	(0.0000)	0.0000572	(0.0000)
	x Female	-0.00163	(0.0010)	-0.00175	(0.0010)
Lag ALC days (days)		-0.0276***	(0.0050)	-0.0286***	(0.0050)
	x Urban	0.0298***	(0.0050)	0.0292***	(0.0050)
	x Rural	0.0098	(0.0060)	0.00903	(0.0070)
	x Age	0.0000549	(0.0000)	0.0000571	(0.0000)
	x ADG Score	0.0000509	(0.0000)	0.0000451	(0.0000)
	x Female	0.00816*	(0.0040)	0.00939*	(0.0040)
Lag HSMR		0.0397**	(0.0140)	0.0521***	(0.0140)
	x Urban	-0.0238	(0.0130)	-0.0302*	(0.0130)
	x Rural	0.0297	(0.0170)	0.0173	(0.0170)
	x Age	-0.000133	(0.0000)	-0.000231	(0.0000)
	x ADG Score	-0.00201***	(0.0010)	-0.00192***	(0.0010)
	x Female	-0.0141	(0.0100)	-0.0141	(0.0100)
Lag high-risk occupancy		-0.00508	(0.0030)	-0.004	(0.0030)
	x Urban	-0.00599*	(0.0030)	-0.00790**	(0.0030)
	x Rural	-0.00134	(0.0040)	-0.00318	(0.0040)
	x Age	0.0000958	(0.0000)	0.0000588	(0.0000)
	x ADG Score	0.000911***	(0.0000)	0.000916***	(0.0000)
	x Female	-0.00550*	(0.0020)	-0.00536*	(0.0020)
Teaching institution		-0.428***	(0.0360)	-0.358***	(0.0370)
Observations		341790		341790	
Pseudo R-squared		0.586		0.601	

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Estimation by conditional logistic regression for patients admitted with sepsis and stroke. Choice set size of 10 hospitals. Marginal utility reported. Patient and hospital characteristics included.

TABLE A-14. PATIENT CHOICE ESTIMATION RESULTS, PNEUMONIA PATIENTS, ALTERNATE DISTANCE SPECIFICATIONS

Base	Interaction	Distance^2		Distance^3	
		Coefficient	s.e.	Coefficient	s.e.
Distance (10km)		-1.832***	(0.0240)	-2.517***	(0.0490)
	x Urban	-1.798***	(0.0340)	-2.205***	(0.0600)
	x Rural	0.391***	(0.0290)	0.849***	(0.0570)
	x Age	-0.0151***	(0.0010)	-0.0174***	(0.0010)
	x ADG Score	0.0155***	(0.0010)	0.0136***	(0.0020)
	x Female	-0.0610**	(0.0240)	-0.0459	(0.0380)
Distance^2 (10km)		0.0603***	(0.0010)	0.218***	(0.0100)
	x Urban	0.0584***	(0.0020)	0.432***	(0.0160)
	x Rural	-0.0196***	(0.0010)	-0.136***	(0.0100)
	x Age	0.000527***	(0.0000)	0.00140***	(0.0000)
	x ADG Score	-0.000472***	(0.0000)	-0.000497	(0.0000)
	x Female	0.00305**	(0.0010)	0.0029	(0.0060)
Distance^3 (10km)				-0.00587***	(0.0000)
	x Urban			-0.0198***	(0.0010)
	x Rural			0.00458***	(0.0000)
	x Age			-0.0000304***	(0.0000)
	x ADG Score			0.00000333	(0.0000)
	x Female			-0.000028	(0.0000)
Hospital location: urban		-0.0758	(0.0570)	0.0517	(0.0580)
	x Urban	0.366*	(0.1430)	0.541***	(0.1340)
	x Rural	0.392***	(0.1190)	0.19	(0.1190)
		-1.844***	(0.1140)	-1.857***	(0.1140)
Hospital location: rural	x Urban	0.885*	(0.4500)	-0.248	(0.3080)
	x Rural	1.839***	(0.1300)	1.842***	(0.1300)
Lag volume (1000s)		0.0994***	(0.0060)	0.0948***	(0.0060)
	x Urban	-0.0370***	(0.0060)	-0.0319***	(0.0060)
	x Rural	0.0225*	(0.0100)	0.0290**	(0.0100)
	x Age	-0.000264***	(0.0000)	-0.000263***	(0.0000)
	x ADG Score	0.000608***	(0.0000)	0.000644***	(0.0000)
	x Female	-0.00121	(0.0020)	-0.00104	(0.0020)
Lag specialization (%)		0.00383*	(0.0020)	0.00421**	(0.0020)
	x Urban	0.00371*	(0.0020)	0.00314	(0.0020)
	x Rural	0.000584	(0.0020)	0.000643	(0.0020)
	x Age	0.0000547	(0.0000)	0.0000741*	(0.0000)
	x ADG Score	0.000196***	(0.0000)	0.000182***	(0.0000)

table continued on next page

TABLE A-14 CONTINUED

	x Female	-0.000484	(0.0010)	-0.000509	(0.0010)
Lag (std) mortality rate (%)		-0.0129***	(0.0020)	-0.0118***	(0.0020)
	x Urban	0.0116***	(0.0020)	0.00971***	(0.0020)
	x Rural	0.00983***	(0.0020)	0.00910***	(0.0020)
	x Age	0.000130**	(0.0000)	0.000128**	(0.0000)
	x ADG Score	0.000313***	(0.0000)	0.000310***	(0.0000)
	x Female	0.00141	(0.0010)	0.00137	(0.0010)
Lag (std) readmission rate (%)		-0.0000476	(0.0010)	-0.000654	(0.0010)
	x Urban	-0.0289***	(0.0030)	-0.0255***	(0.0030)
	x Rural	-0.00427***	(0.0010)	-0.00320**	(0.0010)
	x Age	-0.0000414	(0.0000)	-0.0000447	(0.0000)
	x ADG Score	-0.0000451	(0.0000)	-0.0000374	(0.0000)
	x Female	-0.000698	(0.0010)	-0.000466	(0.0010)
Lag ALC days (days)		-0.0244***	(0.0040)	-0.0241***	(0.0040)
	x Urban	0.0439***	(0.0040)	0.0429***	(0.0040)
	x Rural	0.0188***	(0.0050)	0.0164**	(0.0050)
	x Age	0.000226*	(0.0000)	0.000236*	(0.0000)
	x ADG Score	-0.000345**	(0.0000)	-0.000375**	(0.0000)
	x Female	0.000752	(0.0030)	0.00103	(0.0030)
Lag HSMR		0.00328	(0.0080)	0.00859	(0.0080)
	x Urban	-0.0129	(0.0080)	-0.0176*	(0.0080)
	x Rural	0.00133	(0.0120)	-0.00133	(0.0120)
	x Age	-0.0000963	(0.0000)	-0.000121	(0.0000)
	x ADG Score	-0.000618***	(0.0000)	-0.000562**	(0.0000)
	x Female	-0.0123**	(0.0040)	-0.0122**	(0.0040)
Lag high-risk occupancy		0.0184***	(0.0020)	0.0207***	(0.0020)
	x Urban	-0.00409	(0.0020)	-0.00648**	(0.0020)
	x Rural	-0.0110***	(0.0030)	-0.0136***	(0.0030)
	x Age	0.000201***	(0.0000)	0.000177***	(0.0000)
	x ADG Score	0.000175*	(0.0000)	0.000185*	(0.0000)
	x Female	-0.00393*	(0.0020)	-0.00412*	(0.0020)
Teaching institution		-0.197***	(0.0290)	-0.122***	(0.0290)
Observations		483765		483765	
Pseudo R-squared		0.565		0.576	

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Estimation by conditional logistic regression for patients admitted with sepsis and stroke. Choice set size of 10 hospitals. Marginal utility reported. Patient and hospital characteristics included.

TABLE A-15. PATIENT CHOICE ESTIMATION RESULTS, SEPSIS PATIENTS, ALTERNATE DISTANCE SPECIFICATIONS

Base	Interaction	Distance^2		Distance^3	
		Coefficient	s.e.	Coefficient	s.e.
Distance (10km)		-1.972***	(0.0610)	-2.641***	(0.1120)
	x Urban	-1.729***	(0.0640)	-1.807***	(0.1050)
	x Rural	0.424***	(0.0640)	0.694***	(0.1260)
	x Age	-0.0225***	(0.0010)	-0.0304***	(0.0020)
	x ADG Score	0.0181***	(0.0020)	0.0132***	(0.0030)
	x Female	-0.0432	(0.0450)	-0.0931	(0.0820)
Distance^2 (10km)		0.0651***	(0.0030)	0.227***	(0.0210)
	x Urban	0.0589***	(0.0040)	0.302***	(0.0260)
	x Rural	-0.0220***	(0.0030)	-0.0954***	(0.0200)
	x Age	0.000836***	(0.0000)	0.00321***	(0.0000)
	x ADG Score	-0.000555***	(0.0000)	-0.000213	(0.0010)
	x Female	0.0023	(0.0020)	0.00743	(0.0150)
Distance^3 (10km)				-0.00622***	(0.0010)
	x Urban			-0.0118***	(0.0010)
	x Rural			0.00238**	(0.0010)
	x Age			-0.0000995***	(0.0000)
	x ADG Score			-0.0000095	(0.0000)
	x Female			0.000224	(0.0010)
Hospital location:		0.119	(0.1190)	0.325**	(0.1220)
urban	x Urban	0.54	(0.2890)	0.586*	(0.2870)
	x Rural	0.0977	(0.2610)	-0.218	(0.2600)
		-2.214***	(0.2800)	-2.456***	(0.2930)
Hospital location: rural	x Urban	-0.475	(1.9340)	0.103	(0.7790)
	x Rural	1.638***	(0.3170)	1.862***	(0.3280)
Lag volume (1000s)		0.115***	(0.0110)	0.105***	(0.0110)
	x Urban	-0.0514***	(0.0100)	-0.0423***	(0.0100)
	x Rural	0.0176	(0.0210)	0.0287	(0.0210)
	x Age	-0.000267*	(0.0000)	-0.000243	(0.0000)
	x ADG Score	0.000398*	(0.0000)	0.000451*	(0.0000)
	x Female	-0.00453	(0.0040)	-0.00316	(0.0040)
Lag specialization (%)		0.118***	(0.0120)	0.112***	(0.0120)
	x Urban	-0.0855***	(0.0110)	-0.0824***	(0.0110)
	x Rural	-0.0322	(0.0190)	-0.0203	(0.0190)
	x Age	-0.00100***	(0.0000)	-0.000996***	(0.0000)
	x ADG Score	0.000233	(0.0000)	0.000243	(0.0000)

table continued on next page

TABLE A-15 CONTINUED

	x Female	-0.00761	(0.0072)	-0.0035	(0.003)
Lag (std) mortality rate (%)		0.00473*	(0.0019)	-0.000285	(0.0015)
	x Urban	-0.00939***	(0.0017)	-0.000177	(0.0017)
	x Rural	-0.00404*	(0.0017)	-0.00464*	(0.0018)
	x Age	-0.000103**	(0.0000)	0.0000571	(0.0001)
	x ADG Score	0.000084	(0.0000)	0.0000388	(0.0001)
	x Female	-0.00221	(0.0012)	-0.000147	(0.0014)
Lag (std) readmission rate (%)		-0.00267**	(0.0008)	-0.00106*	(0.0005)
	x Urban	-0.000313	(0.0011)	0.00151**	(0.0005)
	x Rural	0.00247**	(0.0008)	-0.0000803	(0.0008)
	x Age	0.00000942	(0.0000)	-0.0000196	(0.0000)
	x ADG Score	0.00000773	(0.0000)	0.0000155	(0.0000)
	x Female	0.000137	(0.0002)	0.0000458	(0.0003)
Lag ALC days (days)		-0.00602	(0.0090)	-0.0515***	(0.0075)
	x Urban	0.0469***	(0.0082)	0.0622***	(0.0075)
	x Rural	-0.00386	(0.0122)	0.0207	(0.0118)
	x Age	0.00105***	(0.0002)	0.00121***	(0.0002)
	x ADG Score	-0.00113***	(0.0002)	0.00000337	(0.0003)
	x Female	-0.000776	(0.0058)	0.0173***	(0.0049)
Lag HSMR		-0.00646	(0.0216)	0.0539**	(0.018)
	x Urban	-0.0108	(0.0205)	-0.0295	(0.0181)
	x Rural	0.0838**	(0.0304)	0.00948	(0.0267)
	x Age	0.000041	(0.0003)	-0.00143**	(0.0005)
	x ADG Score	-0.000373	(0.0004)	-0.00356***	(0.0006)
	x Female	-0.00745	(0.0097)	-0.00583	(0.0114)
Lag high-risk occupancy		0.0184***	(0.0055)	0.00322	(0.0038)
	x Urban	-0.0121*	(0.0047)	-0.00597	(0.0039)
	x Rural	-0.0144	(0.0076)	-0.00354	(0.0059)
	x Age	0.000280*	(0.0001)	0.000737***	(0.0001)
	x ADG Score	0.0000639	(0.0002)	0.00108***	(0.0002)
	x Female	0.0044	(0.0038)	-0.00416	(0.003)
Teaching institution		-0.189***	(0.0565)	-0.185***	(0.045)
Observations		112953		168448	
Pseudo R-squared		0.531		0.533	

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Estimation by conditional logistic regression for patients admitted with sepsis and stroke. Choice set size of 10 hospitals. Marginal utility reported. Patient and hospital characteristics included.

TABLE A-16. PATIENT CHOICE ESTIMATION RESULTS, STROKE PATIENTS, ALTERNATE DISTANCE SPECIFICATIONS

Base	Interaction	Distance^2		Distance^3	
		Coefficient	s.e.	Coefficient	s.e.
Distance (10km)		-1.295***	(0.0370)	-1.997***	(0.0660)
	x Urban	-2.202***	(0.0520)	-2.936***	(0.0860)
	x Rural	0.304***	(0.0510)	0.520***	(0.1010)
	x Age	-0.0256***	(0.0010)	-0.0272***	(0.0020)
	x ADG Score	-0.00337	(0.0020)	-0.00442	(0.0030)
	x Female	0.0196	(0.0340)	0.00217	(0.0570)
Distance^2 (10km)		0.0436***	(0.0020)	0.175***	(0.0090)
	x Urban	0.176***	(0.0050)	0.749***	(0.0240)
	x Rural	-0.0113***	(0.0020)	-0.0673***	(0.0130)
	x Age	0.000999***	(0.0000)	0.00228***	(0.0000)
	x ADG Score	0.0000844	(0.0000)	0.000364	(0.0000)
	x Female	-0.000119	(0.0020)	0.00144	(0.0080)
Distance^3 (10km)				-0.00410***	(0.0000)
	x Urban			-0.0426***	(0.0020)
	x Rural			0.00155***	(0.0000)
	x Age			-0.0000484***	(0.0000)
	x ADG Score			-0.0000114	(0.0000)
	x Female			-0.0000536	(0.0000)
Hospital location: urban		0.123	(0.1040)	0.282**	(0.1090)
	x Urban	2.696***	(0.3330)	2.285***	(0.2790)
	x Rural	0.412*	(0.1830)	0.0375	(0.1870)
		-0.809**	(0.2520)	-0.755**	(0.2490)
Hospital location: rural	x Urban	-88.64***	(3.8400)	-3.537**	(1.0810)
	x Rural	-0.352	(0.2940)	-0.355	(0.2920)
Lag volume (1000s)		0.140***	(0.0080)	0.138***	(0.0080)
	x Urban	-0.0670***	(0.0080)	-0.0629***	(0.0080)
	x Rural	-0.0489**	(0.0150)	-0.0348*	(0.0150)
	x Age	-0.000289*	(0.0000)	-0.000341*	(0.0000)
	x ADG Score	0.000349*	(0.0000)	0.000376*	(0.0000)
	x Female	-0.000804	(0.0040)	-0.0000763	(0.0040)
Lag specialization (%)		0.0986***	(0.0040)	0.0979***	(0.0040)
	x Urban	-0.0337***	(0.0040)	-0.0339***	(0.0040)
	x Rural	0.00165	(0.0070)	0.00135	(0.0070)
	x Age	-0.000934***	(0.0000)	-0.00101***	(0.0000)
	x ADG Score	-0.00121***	(0.0000)	-0.00121***	(0.0000)

table continued on next page

TABLE A-16 CONTINUED

	x Female	-0.00339	(0.0030)	-0.0035	(0.0030)
Lag (std) mortality rate (%)		0.000641	(0.0010)	-0.000285	(0.0010)
	x Urban	0.000324	(0.0020)	-0.000177	(0.0020)
	x Rural	-0.00558**	(0.0020)	-0.00464*	(0.0020)
	x Age	0.0000533	(0.0000)	0.0000571	(0.0000)
	x ADG Score	0.0000226	(0.0000)	0.0000388	(0.0000)
	x Female	-0.000192	(0.0010)	-0.000147	(0.0010)
Lag (std) readmission rate (%)		-0.00101	(0.0010)	-0.00106*	(0.0010)
	x Urban	0.00137**	(0.0000)	0.00151**	(0.0010)
	x Rural	0.000327	(0.0010)	-0.0000803	(0.0010)
	x Age	-0.0000209	(0.0000)	-0.0000196	(0.0000)
	x ADG Score	0.0000171	(0.0000)	0.0000155	(0.0000)
	x Female	0.0000628	(0.0000)	0.0000458	(0.0000)
Lag ALC days (days)		-0.0551***	(0.0070)	-0.0515***	(0.0070)
	x Urban	0.0691***	(0.0070)	0.0622***	(0.0080)
	x Rural	0.0231*	(0.0120)	0.0207	(0.0120)
	x Age	0.00120***	(0.0000)	0.00121***	(0.0000)
	x ADG Score	-6.52E-06	(0.0000)	0.00000337	(0.0000)
	x Female	0.0156***	(0.0050)	0.0173***	(0.0050)
Lag HSMR		0.0437*	(0.0180)	0.0539**	(0.0180)
	x Urban	-0.0303	(0.0180)	-0.0295	(0.0180)
	x Rural	0.0205	(0.0260)	0.00948	(0.0270)
	x Age	-0.00144**	(0.0000)	-0.00143**	(0.0000)
	x ADG Score	-0.00339***	(0.0010)	-0.00356***	(0.0010)
	x Female	-0.0037	(0.0110)	-0.00583	(0.0110)
Lag high-risk occupancy		0.00361	(0.0040)	0.00322	(0.0040)
	x Urban	-0.00455	(0.0040)	-0.00597	(0.0040)
	x Rural	0.00062	(0.0060)	-0.00354	(0.0060)
	x Age	0.000760***	(0.0000)	0.000737***	(0.0000)
	x ADG Score	0.00105***	(0.0000)	0.00108***	(0.0000)
	x Female	-0.00416	(0.0030)	-0.00416	(0.0030)
Teaching institution		-0.251***	(0.0450)	-0.185***	(0.0450)
Observations		168448		168448	
Pseudo R-squared		0.514		0.533	

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Estimation by conditional logistic regression for patients admitted with sepsis and stroke. Choice set size of 10 hospitals. Marginal utility reported. Patient and hospital characteristics included.

TABLE A-17. PATIENT CHOICE ESTIMATION RESULTS, AMI PATIENTS, BY ARRIVAL TYPE

	Interaction	Ambulance Arrival		Other Arrival	
		Coefficient	s.e.	Coefficient	s.e.
Distance (10km)		-2.688***	(0.0854)	-2.337***	(0.1206)
	x Urban	-2.683***	(0.0990)	-3.729***	(0.1370)
	x Rural	0.906***	(0.0911)	0.795***	(0.1320)
	x Age	-0.0176***	(0.0026)	-0.0135***	(0.0038)
	x ADG Score	0.0149***	(0.0036)	-0.00768	(0.0051)
	x Female	-0.0851	(0.0735)	0.0540	(0.0939)
Distance^2 (10km)		0.232***	(0.0163)	0.160***	(0.0224)
	x Urban	0.705***	(0.0326)	1.061***	(0.0472)
	x Rural	-0.135***	(0.0159)	-0.113***	(0.0219)
	x Age	0.00221***	(0.0005)	0.00108	(0.0007)
	x ADG Score	-0.000529	(0.0006)	0.00258**	(0.0009)
	x Female	0.0174	(0.0134)	-0.000976	(0.0165)
Distance^3 (10km)		-0.00623***	(0.0008)	-0.00370***	(0.0010)
	x Urban	-0.0433***	(0.0027)	-0.0719***	(0.0044)
	x Rural	0.00408***	(0.0007)	0.00328***	(0.0009)
	x Age	-0.0000804***	(0.0000)	-0.0000350	(0.0000)
	x ADG Score	0.00000746	(0.0000)	-0.000100*	(0.0000)
	x Female	-0.000695	(0.0006)	0.000166	(0.0007)
Hospital location: urban		0.137	(0.0900)	0.504***	(0.1171)
	x Urban	0.738***	(0.2033)	0.574*	(0.2391)
	x Rural	-0.0646	(0.1881)	0.409	(0.2139)
		-2.301***	(0.1900)	-2.162***	(0.2737)
Hospital location: rural	x Urban	1.436***	(0.4355)	0.721	(0.5849)
	x Rural	1.847***	(0.2141)	1.613***	(0.3017)
Lag volume (1000s)		0.101***	(0.0087)	0.104***	(0.0107)
	x Urban	-0.0484***	(0.0084)	-0.0407***	(0.0103)
	x Rural	0.0213	(0.0153)	0.0167	(0.0183)
	x Age	-0.000454**	(0.0002)	-0.000363	(0.0002)
	x ADG Score	0.00135***	(0.0002)	0.000856***	(0.0002)
	x Female	0.00261	(0.0045)	-0.00273	(0.0047)
Lag specialization (%)		0.0516***	(0.0047)	0.0736***	(0.0064)
	x Urban	-0.0134**	(0.0045)	-0.0231***	(0.0059)
	x Rural	-0.0119	(0.0064)	-0.0221**	(0.0085)
	x Age	0.0000176	(0.0002)	-0.000147	(0.0002)
	x ADG Score	-0.000788***	(0.0002)	-0.000615*	(0.0003)

table continued on next page

TABLE A-17 CONTINUED

	x Female	-0.00394	(0.0041)	-0.000375	(0.0049)
Lag (std) mortality rate (%)		-0.00468*	(0.0020)	-0.00842**	(0.0030)
	x Urban	-0.0114***	(0.0024)	-0.00726*	(0.0029)
	x Rural	0.00122	(0.0025)	0.000361	(0.0032)
	x Age	0.000248**	(0.0001)	0.000291**	(0.0001)
	x ADG Score	-0.000156	(0.0001)	0.0000754	(0.0001)
	x Female	0.00123	(0.0021)	0.000789	(0.0025)
Lag (std) readmission rate (%)		-0.00886***	(0.0019)	-0.00864***	(0.0026)
	x Urban	-0.00186	(0.0021)	-0.00550*	(0.0027)
	x Rural	0.00416*	(0.0018)	0.000769	(0.0024)
	x Age	0.000147**	(0.0001)	0.000318***	(0.0001)
	x ADG Score	0.000141*	(0.0001)	-0.0000962	(0.0001)
	x Female	-0.00207	(0.0013)	-0.000124	(0.0019)
Lag ALC days (days)		-0.0197**	(0.0066)	-0.0410***	(0.0088)
	x Urban	0.0305***	(0.0064)	0.0271***	(0.0082)
	x Rural	0.00771	(0.0083)	0.00765	(0.0110)
	x Age	0.000374	(0.0002)	-0.000256	(0.0003)
	x ADG Score	-0.000177	(0.0003)	0.000335	(0.0003)
	x Female	0.00451	(0.0055)	0.0175**	(0.0064)
Lag HSMR		0.0535**	(0.0174)	0.0512*	(0.0229)
	x Urban	-0.0533**	(0.0168)	0.00281	(0.0214)
	x Rural	0.0210	(0.0210)	0.0107	(0.0283)
	x Age	-0.000222	(0.0005)	-0.000434	(0.0006)
	x ADG Score	-0.00113	(0.0007)	-0.00302***	(0.0008)
	x Female	-0.0211	(0.0135)	-0.00827	(0.0157)
Lag high-risk occupancy		-0.00392	(0.0039)	-0.00476	(0.0050)
	x Urban	-0.00812*	(0.0036)	-0.00748	(0.0045)
	x Rural	-0.00401	(0.0050)	-0.000991	(0.0064)
	x Age	0.000126	(0.0001)	0.0000721	(0.0001)
	x ADG Score	0.00111***	(0.0002)	0.000691***	(0.0002)
	x Female	-0.00462	(0.0033)	-0.00628	(0.0037)
Teaching institution		-0.295***	(0.0488)	-0.447***	(0.0559)
Observations		197617		144173	
Pseudo R-squared		0.602		0.603	

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Estimation by conditional logistic regression for patients admitted with sepsis and stroke. Choice set size of 10 hospitals. Marginal utility reported. Patient and hospital characteristics included.

TABLE A-18. PATIENT CHOICE ESTIMATION RESULTS, PNEUMONIA PATIENTS, BY ARRIVAL TYPE

	Interaction	Ambulance Arrival		Other Arrival	
		Coefficient	s.e.	Coefficient	s.e.
Distance (10km)		-2.583***	(0.0637)	-2.368***	(0.0834)
	x Urban	-1.755***	(0.0748)	-3.024***	(0.1044)
	x Rural	0.841***	(0.0763)	0.826***	(0.0969)
	x Age	-0.0160***	(0.0015)	-0.0171***	(0.0020)
	x ADG Score	0.0167***	(0.0025)	0.00883**	(0.0028)
	x Female	0.0340	(0.0531)	-0.163**	(0.0621)
Distance^2 (10km)		0.225***	(0.0125)	0.205***	(0.0171)
	x Urban	0.311***	(0.0192)	0.704***	(0.0312)
	x Rural	-0.131***	(0.0140)	-0.138***	(0.0179)
	x Age	0.00140***	(0.0003)	0.000973***	(0.0003)
	x ADG Score	-0.000599	(0.0004)	-0.000402	(0.0004)
	x Female	-0.00803	(0.0095)	0.0229*	(0.0094)
Distance^3 (10km)		-0.00593***	(0.0006)	-0.00607***	(0.0009)
	x Urban	-0.0128***	(0.0011)	-0.0387***	(0.0024)
	x Rural	0.00414***	(0.0006)	0.00518***	(0.0010)
	x Age	-0.0000318**	(0.0000)	-0.0000163	(0.0000)
	x ADG Score	-0.000000448	(0.0000)	0.00000652	(0.0000)
	x Female	0.000295	(0.0004)	-0.000617*	(0.0003)
Hospital location:		0.00575	(0.0736)	0.112	(0.0934)
urban	x Urban	0.529**	(0.1740)	0.501*	(0.1997)
	x Rural	0.119	(0.1579)	0.268	(0.1844)
		-1.677***	(0.1404)	-2.174***	(0.1975)
Hospital location: rural	x Urban	0.533	(0.3878)	-0.742	(0.5214)
	x Rural	1.567***	(0.1626)	2.294***	(0.2214)
Lag volume (1000s)		0.0967***	(0.0075)	0.0921***	(0.0088)
	x Urban	-0.0410***	(0.0075)	-0.0189*	(0.0087)
	x Rural	0.0299*	(0.0136)	0.0280	(0.0157)
	x Age	-0.000391***	(0.0001)	-0.000356**	(0.0001)
	x ADG Score	0.000947***	(0.0001)	0.000227	(0.0001)
	x Female	-0.00444	(0.0033)	0.00309	(0.0035)
Lag specialization (%)		0.00432*	(0.0020)	0.00438	(0.0025)
	x Urban	0.00249	(0.0021)	0.00371	(0.0025)
	x Rural	0.00286	(0.0025)	-0.00290	(0.0030)
	x Age	0.000132**	(0.0000)	0.00000278	(0.0001)
	x ADG Score	0.000145*	(0.0001)	0.000223**	(0.0001)

table continued on next page

TABLE A-18 CONTINUED

	x Female	-0.000581	(0.0015)	-0.0000306	(0.0018)
Lag (std) mortality rate (%)		-0.0117***	(0.0024)	-0.0106***	(0.0031)
	x Urban	0.00831**	(0.0025)	0.0106***	(0.0030)
	x Rural	0.00918**	(0.0030)	0.00876*	(0.0039)
	x Age	0.000195***	(0.0001)	-0.0000197	(0.0001)
	x ADG Score	0.000310***	(0.0001)	0.000264**	(0.0001)
	x Female	0.00238	(0.0019)	0.000543	(0.0022)
Lag (std) readmission rate (%)		0.000149	(0.0010)	-0.00237	(0.0014)
	x Urban	-0.0168***	(0.0033)	-0.0371***	(0.0047)
	x Rural	-0.00446***	(0.0013)	-0.000543	(0.0017)
	x Age	-0.0000370	(0.0000)	-0.0000166	(0.0001)
	x ADG Score	0.0000424	(0.0000)	-0.000151*	(0.0001)
	x Female	-0.000893	(0.0012)	0.000657	(0.0016)
Lag ALC days (days)		-0.0201***	(0.0049)	-0.0284***	(0.0062)
	x Urban	0.0345***	(0.0052)	0.0514***	(0.0063)
	x Rural	0.0125	(0.0066)	0.0192*	(0.0084)
	x Age	0.0000471	(0.0001)	0.000336*	(0.0002)
	x ADG Score	-0.000453*	(0.0002)	-0.000430*	(0.0002)
	x Female	-0.00148	(0.0040)	0.00255	(0.0046)
Lag HSMR		0.00376	(0.0108)	0.0179	(0.0130)
	x Urban	-0.0173	(0.0108)	-0.0204	(0.0130)
	x Rural	-0.0109	(0.0153)	0.0109	(0.0182)
	x Age	-0.000218	(0.0001)	-0.000203	(0.0002)
	x ADG Score	-0.000371	(0.0002)	-0.000904***	(0.0003)
	x Female	-0.0110*	(0.0055)	-0.0127*	(0.0059)
Lag high-risk occupancy		0.0165***	(0.0028)	0.0275***	(0.0033)
	x Urban	-0.000871	(0.0029)	-0.0149***	(0.0033)
	x Rural	-0.0140***	(0.0038)	-0.0132**	(0.0047)
	x Age	0.000153*	(0.0001)	0.000131	(0.0001)
	x ADG Score	0.0000524	(0.0001)	0.000258*	(0.0001)
	x Female	-0.00321	(0.0024)	-0.00625*	(0.0026)
Teaching institution		-0.0221	(0.0397)	-0.216***	(0.0435)
Observations		265726		218039	
Pseudo R-squared		0.573		0.584	

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Estimation by conditional logistic regression for patients admitted with sepsis and stroke. Choice set size of 10 hospitals. Marginal utility reported. Patient and hospital characteristics included.

TABLE A-19. PATIENT CHOICE ESTIMATION RESULTS, SEPSIS PATIENTS, BY ARRIVAL TYPE

	Interaction	Ambulance Arrival		Other Arrival	
		Coefficient	s.e.	Coefficient	s.e.
Distance (10km)		-2.652***	(0.1540)	-2.423***	(0.2154)
	x Urban	-1.672***	(0.1462)	-2.396***	(0.1814)
	x Rural	0.845***	(0.1710)	0.298	(0.2639)
	x Age	-0.0175***	(0.0031)	-0.0407***	(0.0043)
	x ADG Score	0.0173***	(0.0044)	0.00361	(0.0061)
	x Female	-0.173	(0.1091)	0.0545	(0.1543)
Distance^2 (10km)		0.228***	(0.0311)	0.215***	(0.0469)
	x Urban	0.422***	(0.0410)	0.255***	(0.0394)
	x Rural	-0.112***	(0.0295)	-0.0492	(0.0478)
	x Age	0.00167**	(0.0006)	0.00463***	(0.0008)
	x ADG Score	-0.0011	(0.0008)	0.00157	(0.0012)
	x Female	0.0383	(0.0214)	-0.054	(0.0332)
Distance^3 (10km)		-0.00592***	(0.0016)	-0.00863**	(0.0027)
	x Urban	-0.0225***	(0.0029)	-0.00525*	(0.0022)
	x Rural	0.00302*	(0.0013)	0.000936	(0.0025)
	x Age	-0.0000462	(0.0000)	-0.000160***	(0.0000)
	x ADG Score	0.0000274	(0.0000)	-0.0000503	(0.0001)
	x Female	-0.00161	(0.0010)	0.00439*	(0.0018)
Hospital location: urban		0.490**	(0.1570)	0.0665	(0.1966)
	x Urban	0.411	(0.3313)	1.265*	(0.5484)
	x Rural	-0.764*	(0.3408)	0.624	(0.4202)
		-2.145***	(0.3763)	-2.671***	(0.4528)
Hospital location: rural	x Urban	0.124	(0.9802)	-4.394	(2.3822)
	x Rural	1.461***	(0.4208)	2.193***	(0.5132)
Lag volume (1000s)		0.0863***	(0.0145)	0.116***	(0.0176)
	x Urban	-0.0389**	(0.0134)	-0.0395*	(0.0167)
	x Rural	0.0339	(0.0264)	0.0294	(0.0335)
	x Age	-0.000418*	(0.0002)	-0.000243	(0.0002)
	x ADG Score	0.00106***	(0.0003)	0.0000206	(0.0003)
	x Female	-0.00467	(0.0064)	-0.000893	(0.0065)
Lag specialization (%)		0.0840***	(0.0156)	0.141***	(0.0199)
	x Urban	-0.0608***	(0.0137)	-0.111***	(0.0178)
	x Rural	-0.00867	(0.0232)	-0.0345	(0.0317)
	x Age	-0.00124***	(0.0003)	-0.000557	(0.0004)
	x ADG Score	0.000796	(0.0004)	-0.000219	(0.0005)

table continued on next page

TABLE A-19 CONTINUED

	x Female	-0.00471	(0.0098)	-0.0115	(0.0111)
Lag (std) mortality rate (%)		0.00162	(0.0025)	0.00978**	(0.0030)
	x Urban	-0.00976***	(0.0022)	-0.0109***	(0.0028)
	x Rural	-0.00227	(0.0021)	-0.00829**	(0.0030)
	x Age	-0.000108*	(0.0000)	-0.000141*	(0.0001)
	x ADG Score	0.000144*	(0.0001)	0.0000268	(0.0001)
	x Female	-0.0007	(0.0016)	-0.00349	(0.0019)
Lag (std) readmission rate (%)		-0.00194	(0.0010)	-0.00395**	(0.0014)
	x Urban	-0.00154	(0.0015)	0.000336	(0.0018)
	x Rural	0.00202*	(0.0010)	0.00320*	(0.0014)
	x Age	0.0000131	(0.0000)	0.00000999	(0.0000)
	x ADG Score	-2.46E-06	(0.0000)	0.0000247	(0.0000)
	x Female	-0.0000943	(0.0003)	0.000477	(0.0004)
Lag ALC days (days)		0.0153	(0.0117)	-0.0249	(0.0143)
	x Urban	0.0319**	(0.0107)	0.0633***	(0.0129)
	x Rural	-0.0193	(0.0156)	0.0199	(0.0199)
	x Age	0.00111***	(0.0002)	0.000601*	(0.0003)
	x ADG Score	-0.00168***	(0.0003)	-0.000853*	(0.0004)
	x Female	-0.0054	(0.0079)	0.00331	(0.0086)
Lag HSMR		-0.00519	(0.0284)	-0.00797	(0.0340)
	x Urban	-0.0243	(0.0270)	0.00491	(0.0325)
	x Rural	0.0663	(0.0395)	0.120*	(0.0494)
	x Age	0.000199	(0.0004)	-0.000221	(0.0004)
	x ADG Score	-0.0000593	(0.0005)	-0.000729	(0.0005)
	x Female	-0.00271	(0.0132)	-0.00909	(0.0142)
Lag high-risk occupancy		0.0237**	(0.0075)	0.0154	(0.0082)
	x Urban	-0.0152*	(0.0065)	-0.0111	(0.0070)
	x Rural	-0.00897	(0.0099)	-0.0226	(0.0121)
	x Age	0.000199	(0.0002)	0.000286	(0.0002)
	x ADG Score	-0.000309	(0.0002)	0.000298	(0.0002)
	x Female	0.00637	(0.0052)	0.00121	(0.0055)
Teaching institution		0.127	(0.0784)	-0.485***	(0.0836)
Observations		56685		56268	
Pseudo R-squared		0.509		0.569	

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Estimation by conditional logistic regression for patients admitted with sepsis and stroke. Choice set size of 10 hospitals. Marginal utility reported. Patient and hospital characteristics included.

TABLE A-20. PATIENT CHOICE ESTIMATION RESULTS, STROKE PATIENTS, BY ARRIVAL TYPE

	Interaction	Ambulance Arrival		Other Arrival	
		Coefficient	s.e.	Coefficient	s.e.
Distance (10km)		-1.812***	(0.0814)	-2.687***	(0.1576)
	x Urban	-2.545***	(0.1098)	-2.940***	(0.1708)
	x Rural	0.317*	(0.1270)	0.820***	(0.2263)
	x Age	-0.0176***	(0.0031)	-0.0366***	(0.0042)
	x ADG Score	-0.00308	(0.0041)	0.0000709	(0.0060)
	x Female	0.109	(0.0714)	-0.0425	(0.1311)
Distance^2 (10km)		0.161***	(0.0108)	0.275***	(0.0325)
	x Urban	0.642***	(0.0311)	0.770***	(0.0458)
	x Rural	-0.0445**	(0.0166)	-0.116**	(0.0379)
	x Age	0.00144**	(0.0004)	0.00365***	(0.0007)
	x ADG Score	0.000152	(0.0006)	-0.000579	(0.0011)
	x Female	-0.0101	(0.0098)	0.0134	(0.0288)
Distance^3 (10km)		-0.00380***	(0.0003)	-0.00887***	(0.0019)
	x Urban	-0.0367***	(0.0025)	-0.0422***	(0.0034)
	x Rural	0.000944	(0.0006)	0.0034	(0.0019)
	x Age	-0.0000319*	(0.0000)	-0.000138***	(0.0000)
	x ADG Score	-0.00000411	(0.0000)	0.0000529	(0.0001)
	x Female	0.000243	(0.0003)	-0.000841	(0.0017)
Hospital location:		0.119	(0.1402)	0.538**	(0.1777)
urban	x Urban	1.421***	(0.3127)	4.180***	(0.6206)
	x Rural	-0.0444	(0.2474)	-0.0687	(0.3048)
		-0.646*	(0.3227)	-0.795*	(0.3917)
Hospital location: rural	x Urban	-3.232**	(1.1227)	-15.14	(743.4102)
	x Rural	-0.451	(0.3799)	-0.269	(0.4618)
Lag volume (1000s)		0.146***	(0.0111)	0.129***	(0.0135)
	x Urban	-0.0761***	(0.0110)	-0.0472***	(0.0134)
	x Rural	-0.0222	(0.0197)	-0.0195	(0.0250)
	x Age	-0.000688**	(0.0002)	-0.0000803	(0.0002)
	x ADG Score	0.000364	(0.0003)	0.000273	(0.0002)
	x Female	-0.00326	(0.0052)	0.00124	(0.0051)
Lag specialization (%)		0.100***	(0.0055)	0.0910***	(0.0064)
	x Urban	-0.0314***	(0.0055)	-0.0322***	(0.0062)
	x Rural	0.0114	(0.0092)	-0.0103	(0.0117)
	x Age	-0.000564**	(0.0002)	-0.00142***	(0.0002)
	x ADG Score	-0.00145***	(0.0002)	-0.00101***	(0.0002)

table continued on next page

TABLE A-20 CONTINUED

	x Female	-0.00587	(0.0042)	0.00162	(0.0044)
Lag (std) mortality rate (%)		-0.00302	(0.0021)	0.00272	(0.0022)
	x Urban	-0.00402	(0.0024)	0.00299	(0.0024)
	x Rural	-0.00207	(0.0024)	-0.00695*	(0.0031)
	x Age	0.0000983	(0.0001)	-0.0000256	(0.0001)
	x ADG Score	0.000192	(0.0001)	-0.0000861	(0.0001)
	x Female	-0.00122	(0.0020)	0.000653	(0.0022)
Lag (std) readmission rate (%)		-0.00209*	(0.0009)	-0.00273*	(0.0013)
	x Urban	0.000351	(0.0005)	0.00399**	(0.0013)
	x Rural	0.000963	(0.0010)	0.0000513	(0.0019)
	x Age	-0.0000117	(0.0000)	-0.0000268*	(0.0000)
	x ADG Score	0.00000256	(0.0000)	0.0000134	(0.0000)
	x Female	0.00194*	(0.0008)	-0.000521	(0.0004)
Lag ALC days (days)		-0.0568***	(0.0104)	-0.0413***	(0.0114)
	x Urban	0.0514***	(0.0104)	0.0701***	(0.0115)
	x Rural	0.0265	(0.0156)	0.0322	(0.0187)
	x Age	0.000602*	(0.0003)	0.00137***	(0.0003)
	x ADG Score	-0.000255	(0.0004)	-0.0000608	(0.0003)
	x Female	0.0194**	(0.0069)	0.0113	(0.0070)
Lag HSMR		0.0973***	(0.0249)	0.00651	(0.0278)
	x Urban	-0.0541*	(0.0249)	-0.000425	(0.0276)
	x Rural	-0.03	(0.0352)	0.0462	(0.0458)
	x Age	-0.000129	(0.0007)	-0.00234***	(0.0006)
	x ADG Score	-0.00462***	(0.0009)	-0.00269***	(0.0008)
	x Female	-0.0139	(0.0164)	0.000632	(0.0161)
Lag high-risk occupancy		0.00342	(0.0052)	0.00114	(0.0059)
	x Urban	-0.000023	(0.0053)	-0.0130*	(0.0058)
	x Rural	0.00522	(0.0078)	-0.0218*	(0.0091)
	x Age	0.000706***	(0.0002)	0.000844***	(0.0002)
	x ADG Score	0.00118***	(0.0002)	0.00120***	(0.0002)
	x Female	-0.00706	(0.0042)	-0.00143	(0.0044)
Teaching institution		-0.147*	(0.0631)	-0.235***	(0.0653)
Observations		79428		89020	
Pseudo R-squared		0.51		0.568	

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Estimation by conditional logistic regression for patients admitted with sepsis and stroke. Choice set size of 10 hospitals. Marginal utility reported. Patient and hospital characteristics included.

TABLE A-21. PATIENT CHOICE ESTIMATION RESULTS, LENGTH OF STAY INCLUDED

	Interaction	AMI		Pneumonia	
		Coefficient	s.e.	Coefficient	s.e.
Distance (10km)		-2.536***	(0.0690)	-2.507***	(0.0490)
	x Urban	-3.064***	(0.0800)	-2.232***	(0.0600)
	x Rural	0.853***	(0.0740)	0.835***	(0.0570)
	x Age	-0.0167***	(0.0020)	-0.0170***	(0.0010)
	x ADG Score	0.00565	(0.0030)	0.0135***	(0.0020)
	x Female	-0.0343	(0.0570)	-0.0323	(0.0380)
Distance^2 (10km)		0.204***	(0.0130)	0.217***	(0.0100)
	x Urban	0.825***	(0.0270)	0.436***	(0.0160)
	x Rural	-0.127***	(0.0130)	-0.135***	(0.0100)
	x Age	0.00182***	(0.0000)	0.00135***	(0.0000)
	x ADG Score	0.000696	(0.0010)	-0.000493	(0.0000)
	x Female	0.00893	(0.0100)	0.00114	(0.0060)
Distance^3 (10km)		-0.00528***	(0.0010)	-0.00587***	(0.0000)
	x Urban	-0.0523***	(0.0020)	-0.0200***	(0.0010)
	x Rural	0.00381***	(0.0010)	0.00457***	(0.0000)
	x Age	-0.0000633***	(0.0000)	-0.0000289***	(0.0000)
	x ADG Score	-0.0000319	(0.0000)	0.00000323	(0.0000)
	x Female	-0.000288	(0.0000)	0.0000199	(0.0000)
Hospital location: urban		0.315***	(0.0710)	0.0724	(0.0580)
	x Urban	0.620***	(0.1530)	0.504***	(0.1340)
	x Rural	0.109	(0.1390)	0.17	(0.1190)
		-2.256***	(0.1540)	-1.844***	(0.1140)
Hospital location: rural	x Urban	0.967**	(0.3460)	-0.28	(0.3080)
	x Rural	1.731***	(0.1730)	1.847***	(0.1300)
Lag volume (1000s)		0.107***	(0.0070)	0.0993***	(0.0060)
	x Urban	-0.0320***	(0.0070)	-0.0389***	(0.0060)
	x Rural	0.0189	(0.0120)	0.0374***	(0.0110)
	x Age	-0.000429**	(0.0000)	-0.00013	(0.0000)
	x ADG Score	0.000536**	(0.0000)	0.000619***	(0.0000)
	x Female	-0.000434	(0.0040)	0.00352	(0.0030)

table continued on next page

TABLE A-21 CONTINUED

Lag specialization (%)		0.0537***	(0.0040)	0.00360*	(0.0020)
	x Urban	-0.0047	(0.0040)	0.00381*	(0.0020)
	x Rural	-0.00992	(0.0050)	0.00204	(0.0020)
	x Age	0.000101	(0.0000)	0.0000747*	(0.0000)
	x ADG Score	-0.000703***	(0.0000)	0.000183***	(0.0000)
	x Female	-0.00227	(0.0030)	-0.00051	(0.0010)
Lag (std) mortality rate (%)		-0.00631***	(0.0020)	-0.0117***	(0.0020)
	x Urban	-0.00988***	(0.0020)	0.00900***	(0.0020)
	x Rural	0.000654	(0.0020)	0.00964***	(0.0020)
	x Age	0.000262***	(0.0000)	0.000166***	(0.0000)
	x ADG Score	-0.0000425	(0.0000)	0.000301***	(0.0000)
	x Female	0.000869	(0.0020)	0.00256	(0.0010)
Lag (std) readmission rate (%)		-0.0111***	(0.0020)	-0.0015	(0.0010)
	x Urban	-0.00109	(0.0020)	-0.0253***	(0.0030)
	x Rural	0.00542***	(0.0020)	-0.00261**	(0.0010)
	x Age	0.000175***	(0.0000)	-0.0000541	(0.0000)
	x ADG Score	0.0000963	(0.0000)	-0.000034	(0.0000)
	x Female	-0.00172	(0.0010)	-0.000718	(0.0010)
Lag ALC days (days)		-0.0270***	(0.0050)	-0.0218***	(0.0040)
	x Urban	0.0261***	(0.0050)	0.0397***	(0.0040)
	x Rural	0.00609	(0.0070)	0.0173**	(0.0050)
	x Age	0.0000736	(0.0000)	0.000304**	(0.0000)
	x ADG Score	0.000125	(0.0000)	-0.000383**	(0.0000)
	x Female	0.00947*	(0.0040)	0.00325	(0.0030)
Lag HSMR		0.0517***	(0.0140)	0.00355	(0.0080)
	x Urban	-0.0261*	(0.0130)	-0.0125	(0.0080)
	x Rural	0.0202	(0.0170)	-0.005	(0.0120)
	x Age	-0.000227	(0.0000)	-0.000123	(0.0000)
	x ADG Score	-0.00201***	(0.0010)	-0.000573**	(0.0000)
	x Female	-0.014	(0.0100)	-0.0118**	(0.0040)
Lag high-risk occupancy		-0.00281	(0.0030)	0.0191***	(0.0020)
	x Urban	-0.00751**	(0.0030)	-0.00485*	(0.0020)
	x Rural	-0.00398	(0.0040)	-0.0140***	(0.0030)
	x Age	0.000061	(0.0000)	0.000176**	(0.0000)
	x ADG Score	0.000883***	(0.0000)	0.000184*	(0.0000)
	x Female	-0.00551*	(0.0020)	-0.00432*	(0.0020)
Teaching institution		-0.217***	(0.0380)	-0.109***	(0.0320)

table continued on next page

TABLE A-21 CONTINUED

Avg length of stay		-0.123***	(0.0150)	-0.0215***	(0.0060)
	x Urban	-0.00518	(0.0140)	0.0309***	(0.0060)
	x Rural	0.0503*	(0.0200)	-0.00558	(0.0100)
	x Age	0.000334	(0.0000)	-0.000532***	(0.0000)
	x ADG Score	0.00394***	(0.0010)	0.000115	(0.0000)
	x Female	0.00219	(0.0100)	-0.0176***	(0.0040)
Observations		341790		483765	
Pseudo R-squared		0.602		0.576	

table continued on next page

TABLE A-21 CONTINUED

	Interaction	Sepsis		Stroke	
		Coefficient	s.e.	Coefficient	s.e.
Distance (10km)		-2.643***	(0.1120)	-2.011***	(0.0660)
	x Urban	-1.819***	(0.1050)	-2.928***	(0.0870)
	x Rural	0.686***	(0.1260)	0.536***	(0.1020)
	x Age	-0.0300***	(0.0020)	-0.0275***	(0.0020)
	x ADG Score	0.0129***	(0.0030)	-0.00445	(0.0030)
	x Female	-0.0636	(0.0820)	0.00515	(0.0570)
Distance^2 (10km)		0.228***	(0.0210)	0.176***	(0.0090)
	x Urban	0.304***	(0.0260)	0.749***	(0.0240)
	x Rural	-0.0950***	(0.0200)	-0.0684***	(0.0130)
	x Age	0.00317***	(0.0000)	0.00230***	(0.0000)
	x ADG Score	-0.000178	(0.0010)	0.000375	(0.0000)
	x Female	0.00426	(0.0150)	0.00133	(0.0080)
Distance^3 (10km)		-0.00624***	(0.0010)	-0.00412***	(0.0000)
	x Urban	-0.0119***	(0.0010)	-0.0426***	(0.0020)
	x Rural	0.00237**	(0.0010)	0.00157***	(0.0000)
	x Age	-0.0000985***	(0.0000)	-0.0000487***	(0.0000)
	x ADG Score	-0.0000107	(0.0000)	-0.000012	(0.0000)
	x Female	0.000312	(0.0010)	-0.0000557	(0.0000)
Hospital location: urban		0.348**	(0.1230)	0.295**	(0.1090)
	x Urban	0.564*	(0.2880)	2.306***	(0.2790)
	x Rural	-0.261	(0.2610)	0.00742	(0.1870)
		-2.397***	(0.2920)	-0.730**	(0.2500)
Hospital location: rural	x Urban	0.0372	(0.7770)	-3.557**	(1.0810)
	x Rural	1.799***	(0.3270)	-0.377	(0.2920)
Lag volume (1000s)		0.110***	(0.0120)	0.135***	(0.0090)
	x Urban	-0.0483***	(0.0110)	-0.0582***	(0.0090)
	x Rural	0.0139	(0.0210)	-0.0322*	(0.0150)
	x Age	-0.00019	(0.0000)	-0.000450**	(0.0000)
	x ADG Score	0.000402*	(0.0000)	0.000379*	(0.0000)
	x Female	0.00158	(0.0050)	0.000875	(0.0040)

table continued on next page

TABLE A-21 CONTINUED

Lag specialization (%)		0.117***	(0.0130)	0.0972***	(0.0040)
	x Urban	-0.0883***	(0.0120)	-0.0341***	(0.0040)
	x Rural	-0.0297	(0.0190)	0.00228	(0.0070)
	x Age	-0.000891***	(0.0000)	-0.00100***	(0.0000)
	x ADG Score	0.000158	(0.0000)	-0.00121***	(0.0000)
	x Female	0.000116	(0.0080)	-0.00367	(0.0030)
Lag (std) mortality		0.00456*	(0.0020)	-0.000279	(0.0010)
rate (%)	x Urban	-0.00927***	(0.0020)	0.000324	(0.0020)
	x Rural	-0.00406*	(0.0020)	-0.00469**	(0.0020)
	x Age	-0.0000892**	(0.0000)	0.0000518	(0.0000)
	x ADG Score	0.0000796	(0.0000)	0.0000402	(0.0000)
	x Female	-0.00149	(0.0010)	-0.0000782	(0.0010)
Lag (std) readmission		-0.00297***	(0.0010)	-0.000881	(0.0010)
rate (%)	x Urban	-0.0000033	(0.0010)	0.00127**	(0.0000)
	x Rural	0.00279***	(0.0010)	-0.000256	(0.0010)
	x Age	0.00000942	(0.0000)	-0.0000161	(0.0000)
	x ADG Score	0.00000763	(0.0000)	0.0000152	(0.0000)
	x Female	0.000124	(0.0000)	-0.000015	(0.0000)
Lag ALC days (days)		-0.00787	(0.0090)	-0.0583***	(0.0080)
	x Urban	0.0495***	(0.0080)	0.0758***	(0.0080)
	x Rural	-0.00215	(0.0120)	0.0284*	(0.0130)
	x Age	0.00102***	(0.0000)	0.000951***	(0.0000)
	x ADG Score	-0.00112***	(0.0000)	-0.0000228	(0.0000)
	x Female	-0.0022	(0.0060)	0.0196***	(0.0050)
Lag HSMR		-0.0103	(0.0220)	0.0568**	(0.0180)
	x Urban	-0.00792	(0.0210)	-0.0360*	(0.0180)
	x Rural	0.0871**	(0.0310)	0.00462	(0.0270)
	x Age	0.0000794	(0.0000)	-0.00130**	(0.0000)
	x ADG Score	-0.000393	(0.0000)	-0.00341***	(0.0010)
	x Female	-0.00297	(0.0100)	-0.0063	(0.0110)
Lag high-risk		0.0175**	(0.0060)	0.00476	(0.0040)
occupancy	x Urban	-0.0110*	(0.0050)	-0.00853*	(0.0040)
	x Rural	-0.0123	(0.0080)	-0.00544	(0.0060)
	x Age	0.000278*	(0.0000)	0.000784***	(0.0000)
	x ADG Score	0.000072	(0.0000)	0.00108***	(0.0000)
	x Female	0.00376	(0.0040)	-0.00456	(0.0030)
Teaching institution		-0.168**	(0.0590)	-0.154***	(0.0450)

table continued on next page

TABLE A-21 CONTINUED

Avg length of stay		-0.01	(0.0090)	0.0113	(0.0060)
	x Urban	0.0121	(0.0080)	-0.0226***	(0.0060)
	x Rural	0.0269*	(0.0110)	-0.0136	(0.0090)
	x Age	-0.000213	(0.0000)	0.000490*	(0.0000)
	x ADG Score	0.000163	(0.0000)	0.0000489	(0.0000)
	x Female	-0.0165**	(0.0050)	-0.00506	(0.0050)
Observations		112953		168448	
Pseudo R-squared		0.532		0.534	

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses. Estimation by conditional logistic regression for patients admitted with sepsis and stroke. Choice set size of 10 hospitals. Marginal utility reported. Patient and hospital characteristics included.

Appendix B: Johns Hopkins Aggregated Diagnostic Groups (ADGs)

The Johns Hopkins ACG® System was developed to predict healthcare utilization and costs on the patient level (Gutacker et al., 2016). This system allows healthcare providers, health plans, and public-sector agencies to predict the level of resources needed to provide appropriate care to a given patient. The basic principle of the ADG system is that patients can be put into overlapping diagnostic groups based on five criteria: duration (acute, recurring, chronic), severity (minor or major, and stable or unstable), diagnostic certainty (symptoms-based or documented), etiology (infectious, injury, other), and the need for specialty care involvement. Patients can be placed in up to 32 diagnostic categories, each of which predicts healthcare utilization to some degree.

Since the level of resources needed are based on the illness of a patient, many researchers use ADGs as a measure of comorbidity of a patient, using it to predict outcomes such as mortality (Johns Hopkins Bloomberg School of Public Health, 2009). The simplest measure involves summing all of the diagnostic categories to which a patient belongs, but this treats all categories as equal. The ADG Score, on the other hand, uses weights to construct a weighted total for each patient, ignoring some diagnostic groups that are useful for predicting administrative costs but do not predict mortality.

For regression on the patient level creating a single measure is not always necessary, as each ADG can enter the regression equation separately. But a single measure is useful when performing analysis on the aggregate or when interactions between comorbidity and other factors are desired. In this thesis, individual ADGs are used in the indirect standardization of mortality rates (described in the next section). When average comorbidity is desired (in matching or as an outcome in the production function), the ADG Score is calculated at the patient level and then aggregated to the appropriate level of analysis (hospital or pseudo-hospital) by averaging.

Table B-1 shows the ADG categories, and the weights used to construct the weighted total for each patient. To get a sense of how common various ADG categories are, the percentage of patients in each category is calculated, using the sample utilized in Chapter 4.

TABLE B-1. PERCENTAGE OF PATIENTS IN EACH ADG CATEGORY, SAMPLE USED IN CHAPTER 4

ADG category	%, main sample	%, ambulance only	Weight for ADG Score
Time Limited: Minor	4.58	4.66	0
Time Limited: Minor-Primary Infections	46.78	48.10	0
Time Limited: Major	32.13	37.00	6
Time Limited: Major-Primary Infections	24.40	25.59	4
Allergies	0.16	0.15	-6
Asthma	2.68	2.16	0
Likely to Recur: Discrete	13.96	14.81	0
Like to Recur: Discrete-Infections	13.18	16.56	0
Likely to Recur: Progressive	53.53	55.27	8
Chronic Medical: Stable	51.57	54.15	4
Chronic Medical: Unstable	65.87	69.64	12
Chronic Specialty: Stable-Orthopedic	1.26	1.47	-3
Chronic Speciality: Stable-Ear, Nose, Throat	0.18	0.18	0
Chronic Specialty: Stable-Eye	0.71	0.86	3
Chronic Specialty: Unstable-Orthopedic	0.79	0.84	-2
Chronic Specialty: Unstable-Ear, Nose Throat	0.05	0.06	-4
Chronic Speciality: Unstable-Eye	1.37	1.43	1
Dermatologic	1.05	1.17	-4
Injuries/Adverse Effects: Minor	2.61	3.37	-1
Injuries/Adverse Effects: Major	17.18	19.50	2
Psychosocial: Time Limited, Minor	2.77	2.97	-1
Psychosocial: Recurrent or Persistent, Stable	5.64	6.50	-3
Psychosocial: Recurrent or Persistent, Unstable	13.26	17.85	16
Signs/Symptoms: Minor	12.52	14.00	3
Signs/Symptoms: Uncertain	31.89	34.93	2
Signs/Symptoms: Major	36.20	40.55	2
Discretionary	6.59	6.51	-2
See and Reassure	2.16	2.18	1
Prevention/Administrative	33.78	39.17	-2
Malignancy	12.50	10.89	13
Pregnancy	0.00	0.00	-19
Dental	0.00	0.00	-1

Weights from (Austin & Van Walraven, 2011).

Appendix C: Calculation of Standardized Ratios and Rates

Condition-specific rates

Standardization of outcomes such as mortality rates allow outcomes to be more easily compared between hospitals. There are two main categories of standardization: direct and indirect. In the main body of this thesis, rates and ratios are indirectly standardized, though Appendix A contains some tables showing the results when direct standardization or no standardization is used.

Direct standardization

In essence, direct standardization methods impose a fixed population on each hospital, essentially calculating the rate of an outcome as if all hospitals faced the same composition of patients. By comparison, indirect standardization imposes fixed risks on each hospital. Given the patient composition of each hospital, the expected number of bad outcomes is calculated, and this number is compared to the actual number of bad outcomes.

The directly standardized rates, used in regression in Appendix A, are calculated as follows:

1. All patients are divided into age-sex groups. The groups for males and females, respectively, are: under 35, 35-44, 45-54, 55-64, 65-74, 75-84, 85-94, 95-104, and 105 and older.
2. The proportion of patients in each group is calculated, for the whole sample. These are the proportion weights.
3. Within each hospital-year, the crude rate within each age-sex group is calculated.
4. The directly standardized rate for each hospital-year is calculated as a weighted average of the crude mortality rates for the hospital-year, where the weight for each rate is the associated proportion weight from Step 2.

Since this method only controls for differences in age and sex groups between hospitals, the mortality rates calculated using the method are highly similar to the crude mortality rates, and likely are not incorporating enough information. Since it is cumbersome to include more dimensions using this method, indirect standardization, which easily incorporates many dimensions, is used for the rates analyzed in the main body of the thesis.

Indirect standardization

While direct standardization imposes a fixed population on each hospital, indirect standardization imposes fixed risks on each hospital. The indirectly standardized rates, used in the main body of the thesis, are calculated as follows:

1. A logistic regression predicting the negative outcome (death) is run on the entire sample. The regressors are age, whether the patient is female, whether the patient is admitted via ambulance, length of stay, and each of the Johns Hopkins ADG categories.
2. Within each hospital-year, the coefficients from the logistic regression are used to predict the number of negative outcomes.
3. The crude mortality rates for each hospital-year are multiplied by the actual number of negative outcomes over the predicted number of negative outcomes for the hospital year. This means that if a hospital has more actual negative outcomes than predicted, the indirectly standardized rate is higher than the crude rate.

The Hospital-Standardized Mortality Ratio (HSMR)

The hospital-standardized mortality ratio is calculated via indirect standardization according to steps published by CIHI (2016). Rather than using indirect standardization to adjust a crude mortality rate, the ratio is calculated as the ratio of actual to predicted deaths, multiplied by 100. This makes the statistic easy to interpret as hospitals with more deaths than predicted have an HSMR over 100 while hospitals with fewer deaths than predicted have an HSMR under 100. Full details are included in CIHI (2016) but the basic steps are as follows:

1. Patients are divided into 72 diagnostic categories, based on the diagnoses that cause 80% of in-hospital deaths. A patient is included in a category if the diagnosis identified as most responsible for their hospital stay is in one of the categories. There are some special cases where CIHI recommends switching the most responsible diagnosis of the patient based on comorbidities or procedures performed. Patients only have one most responsible diagnosis and can only be in one category. Patients with a most responsible diagnosis outside of the categories are excluded from the calculation of the HSMR.
2. A baseline year is chosen, and for each category, logistic regression predicting in-hospital death is run on the records of the patients in the category. The regressors are age, whether the patient is male, whether the admission was an emergency admission, various length of stay groups, and various comorbidity groups based on the Charlson Index.

3. Based on the coefficients from the logistic regressions, predicted deaths are calculated for each hospital-year. The HSMR for each hospital-year is 100 times the ratio of actual to predicted deaths.

For this thesis, the baseline year is 1996, to coincide with the year previous to the merger year. The method outlined by The Canadian Institute for Health Information is followed as closely as possible, but there is one exception. According to instructions, in cases where the most responsible diagnosis is coronary artery disease but AMI also occurred, the most responsible diagnosis should be switched to AMI. However, our data set does not include procedures performed and we are unable to make this switch.

Appendix D: Research Ethics Approval

At Queen’s University in Kingston, Ontario, research ethics approval is not required for health studies not involving human or animal participants. Nevertheless, the projects described in Chapter 4 and 5 were reviewed by the Queen’s University Health Sciences & Affiliated Teaching Hospital Research Ethics Board (HSREB). The TRAQ number is 6021439 and clearance is valid until June 28, 2020.