UNIVERSITY OF CALGARY

Microeconometric Analyses of Health Care

by

Lawrence So

A DESSERTATION

SUBMITTED TO THE FACULTY OF GRADUATE STUDIES

IN PARTIAL FULFILMENT OF THE REQUIREMENTS FOR THE

DEGREE OF DOCTOR OF PHILOSOPHY

DEPARTMENT OF ECONOMICS

CALGARY, ALBERTA

SEPTEMBER, 2009

UNIVERSITY OF CALGARY

FACULTY OF GRADUATE STUDIES

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Abstract

The focus of this dissertation is the use of microeconometric methods to explore behavior in Canadian healthcare systems. Understanding this behavior is an essential pre-requisite to the implementation of effective policies to optimize health given budget constraints. The dissertation hones in on two issues which are of considerable importance: first, how physicians respond to changes in the prices they are paid for different services, and second, how immigrants' health evolves over the years following immigration.

Chapter 2 uses physician claims data to estimate the effect of relative fees on physicians' choice of services. Changes to physician fees may affect the types of services they provide, which in turn, may influence patient care and government expenditures. These are key concerns for policy makers deciding future health care budgets and fee changes in Fee-For-Service (FFS) systems. Benefits of the data include exogenous fee schedule changes, and the universe of physicians and services claimed in a single-payer, public health care system. The estimated model suggests that physicians substitute to services with increasing relative fees. There is significant difference in response to relative fees across physician specialties. General Practice is the least fee elastic while Dermatology is the most fee elastic. Thus, policy makers should consider the effect on type of services provided when adjusting the fee schedule.

Chapter 3 uses the comprehensive nature of physician claims data and exogenous variations from a government determined fee schedule to estimate the effect of service fees on the number of services provided. The scarcity of physician labor supply has been a difficult issue for policy makers to resolve. Fees for physician services have been used to encourage physicians to supply more of their services. However, there is little consensus in the literature on how physicians respond to fee increases. The estimated model shows that physicians supply more services as their fees increase. The positive response to a fee

increase is found for all physician specialties, but the size of the response differs across specialties.

Chapter 4 takes advantage of longitudinal data to estimate the association between immigrant status and individual rate changes in health, while controlling both for survey attrition, and for time-variant and time-invariant observables. Immigrants initially entering a host country tend to have a health advantage over native residents that diminishes over time. Most estimates, however, measure the disappearance of immigrants' health advantage using cross-sectional data. The estimates with longitudinal data suggest that immigrants' health advantage deteriorates over time only for their *perceived* health. Immigrants' health, measured in number of chronic conditions and BMI, do not follow the same steep increase over time. Thus, the findings suggest that immigrants maintain their health advantage over native residents over time.

Acknowledgments

I would like to gratefully thank my supervisor, Dr. Auld and co-supervisor, Dr. Hollis. They have been exceptional at providing me with numerous constructive and challenging suggestions that has improved the quality of this thesis. Their insight, patience, and positive feedback has helped me learn a lot from them. I would also like to acknowledge Dr. Quan for providing me with invaluable mentorship and guidance during my doctoral studies.

I would like to thank my parents, Dennis and Sadie So for their support. My parents have always been there for me and have made this thesis possible. To Helen Lee, thank you for being the person I can talk to throughout my graduate studies.

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Chapter 1

Introduction

This dissertation uses microeconometric methods to explore the behaviors of providers and users of Canadian healthcare systems. Understanding the behaviors are important to implementing effective policies that optimize health given budget constraints. The dissertation focuses on two issues which are of considerable importance: first, how physicians respond to changes in fees for their services. Chapter 2 determines whether physicians choose a different set of services when their relative fees change. Chapter 3 determines whether physicians provide more services in total when their fees are increased. Chapter 4 explores the second issue. It determines the difference between immigrants' and native residents' health over the years following immigration.

Chapters 2 and 3 stem from the empirical literature investigating Physician Induced Demand Theory. The literature determines whether physicians induce demand for their services by taking advantage of their superior information of patient health. The empirical literature is abundant and diverse. Studies vary in institutional settings, type of services, physician specialties, and payment mechanisms. Although this topic has been well-researched, the implications from the estimates remain unclear. Many studies find contradictory estimates within their studies, or are inconsistent with other studies. Further, many studies' estimates have been difficult to generalize physicians' supply behavior when the focus was on a single service or physician specialty.

Chapters 2 and 3 add to the literature with the use of physician claims data. The data is unique in its comprehensiveness, which accounts for all physicians and their services for the largest physician specialties in a Canadian health region. The data comes from a publicly-paid, single payer, Fee-For-Service system, which is key to identifying the empirical models in Chapters 2 and 3. Particularly, fees vary exogenously in this institutional setting because fees follow a publicly-governed schedule. The schedule prevents patients or physicians from influencing the fee amounts.

Policy-makers often manage health care expenditures by changing the fee schedule, which consequently changes relative fees. The change in relative fees may provide physicians an incentive to supply a different set of services, which may affect patient care. Chapter 2 develops a model to estimate the effect of relative fees on physicians' choice of services. The estimated model is a Negative Binomial regression with fixed effects, which is estimated with panel data of services by days. The Negative Binomial regression accounts for the count nature of the dependent variable, number of services provided daily. The fixed effects for services account for the difference in frequency of provision across services.

The estimates show that physicians tend to substitute to services with increasing relative fees. The responsiveness differs significantly across physician specialties, where 9 of the 11 physician specialties are found to have positive fee elasticities. Dermatology is the most fee elastic with an elasticity of 2.91, while General Practice is the least fee elastic with an elasticity of -0.01. The estimates are robust to specifications excluding days without service provision, aggregation, and distributional assumptions.

Chapter 3 investigates physicians' labor supply decision to substitute between leisure and work. The chapter estimates the effect of fee increases on the total supply of physician services. Estimates of the direction and responsiveness to fee increases provide insight into its effect on health care expenditures. Further, the estimates determine whether the fee schedule can be used to encourage the supply of physician services, when patient demands are unmet. The empirical framework uses weights to aggregate the panel data to a time series of days. Moreover, the empirical framework uses two stages of regression to account for medical and technological advances affecting each of the service's provision over time.

The estimates show physicians supply 7.9% more services when their fees increase by 10%. Physicians' responsiveness varies significantly with physician specialty, where Nephrology has the largest elasticity at 1.12 and General Surgery has the smallest elasticity at 0.45. The estimates are robust in estimating the model with different weights, fee fixed effects, and number of physicians, placebo number of servics provided, and in first-differences. This chapter also simulates physician revenue with counterfactual fees in the estimated model. The simulated revenues show that physicians are more responsive to increases than decreases in the fee schedule. Thus, policy makers can increase fees to encourage physicians to supply more services, but have to consider the significant increase in health care expenditure from physicians' elastic response.

Chapter 4 investigates the health of immigrants. Immigrants on arrival have a health advantage over native residents, but their health advantage disappears as their duration increases in the host country. The stylized fact is well-documented and has been dubbed the Healthy Immigrant Effect. The implication is significant because it implies that the health care system has been ineffective in providing care for immigrants. It may also imply that barriers in the host country's labor market and education system have been detrimental to immigrants' long-term health.

Chapter 4 uses longitudinal data to compare individual rate changes in health between immigrants and native residents. The estimated model is a first-difference model with fixed effects, which accounts for observable time variant and invariant characteristics. Further, the estimated model corrects for survey attrition. The estimates show immigrants' health do not decline at a steeper rate than that of native residents. Particularly, immigrants' change in chronic conditions and BMI differ insignificantly over time from that of native residents. Immigrants' change in perceived poor health, however, is 0.0387 higher than that of native residents. In contrast to the literature, immigrants maintain their health advantage over native residents over time.

Chapter 2

Physician Fees and Services: Evidence from Comprehensive Physician Claims Data

2.1 Introduction

Determining the fees paid for physician services is difficult in a publicly funded, Fee-For-Service (FFS) system. The fees are unknown to patients, so patients' utilization reflects only their need rather than their response to changes in fees. The unknown fees provide physicians the opportunity to choose what and how many services to bill the government. Physicians may choose services that do not optimize patient care if fees are "wrong" relative to each other. Alternatively, the government can achieve the same level of patient care at a lower cost if it sets the fees appropriately, relative to each other. Policy makers deciding fees must account for the change in patient care and its associated cost after the fee change. If responses to fee changes are known, then policy makers can change fees to contain growing government expenditures while optimizing patient care. This paper uses physician claims data from a large health region to estimate physicians' choice of services when their relative fees change.

There is an abundant empirical literature estimating physicians' response to changes in fees[17;22;23;31;40;42–44;48–50;58;66;67;72]. The literature stems from the Physician Induced Demand (PID) theory - the theory that physicians use their superior information over patients' health to induce demand for their services[64;68]. Many of the studies estimate the supply of services for a type of service or the total number of services. Many of the studies use publicly governed fee schedules to identify their estimates, i.e., the fees paid are exogenous to patient demands and service provision. The studies offer insight to PID theory, but also illustrate the many methodological issues. The main issue faced by the studies is that their estimates are confounded by physicians' ability to substitute to other services when relative fees change. Estimates of a supply function are confounded if there is a correlation between the provision of a service and other services. Consequently, studies like, Escarce (1993) find as many negative as positive fee elasticities when estimating a supply function for each service provided.

To overcome this issue, many studies focus on services that are close substitutes[26;27]. For example, Gruber et al. (1994) evaluate the likelihood of providing a cesarean delivery over natural delivery when relative fees change. The authors estimate that a 1% increase in the fee differential between cesarean and natural delivery increases the provision of cesarean deliveries by 0.84%[40]. One methodological issue faced by the studies, however, is that physicians or patients can substitute to different payers in the health care system when relative fees change between payers. For example, the large and well-known fee reductions under the US Omnibus Budget Reconciliation Act (OBRA) may have resulted in healthier and higher income patients sorting to private insurers while sicker and poorer patients remaining in the public system. The finding that physicians are less likely to perform a service as its Medicare fee is reduced can be attributed to an increased proportion of unhealthy patients in the public system, and not physicians' response to the fee reduction.

The literature has evolved to evaluating single payer, publicly funded health care systems. Fabbri and Monfardini (2001) estimate that a 20% negative shock to physicians' income increases the adoption of cesarean deliveries for low risk women by 65%. Carlsen et al. (2003) evaluate the effect of fee changes on the supply of laboratory tests in Norway. With a panel of 44 physicians over a four-year time period, they find that fee changes have little impact on the number of consultations and laboratory tests (Carlsen, Grytten, and Skau 2003). Nassiri and Rochaix (2006) use a similar approach as Fabbri and Monfardini

(2001) and Carlsen et al. (2003), but account for the bulk of services primary care physicians can substitute across. The authors use quasi-natural experiments in Quebec's publicly funded, FFS system to evaluate the substitution decision of 113 physicians over six years[56]. The authors find that physicians substitute toward the most technical and well-paid procedures when relative fees increase[8]. Nassiri and Rochaix (2006) assume physicians do not consider the provision of other services when deciding the provision of a service.

This paper builds on Nassiri and Rochaix (2006) approach but relaxes the assumption of independent provision of each physician service. Particularly, this paper estimates the effect of relative fees on physicians' choice of services without aggregating similar services together. Aggregating services masks the substitution between services that are close substitutes. This paper uses physician claims data, which represents services provided by all physicians from 11 specialties over a 6-year period. Estimates from a Negative Binomial regression model suggest that physicians substitute to services with increasing relative fees and is robust across most specialties. Physician responsiveness ranges from -2.2% to 29.1% across the 11 specialties for a 10% increase in relative fees. The wide difference in elasticities across specialties suggests that only some specialties substitute across services when relative fees change, i.e., only Dermatology, Nephrology, Orthopedics, Urology, and Internal Medicine are found to have substantial fee elasticities.

2.2 Empirical Framework

The empirical framework considers all the services that physician can substitute across when relative fees change. Capturing substitution between services requires a panel model of physician services by time. If physicians provide only one service, then the model reduces to a supply function for that service. The following denotes the model:

$$Y_{ijt} = \alpha_j P_{ijt} + X_{ijt}\beta_j + u_{ijt} \tag{2.1}$$

Let j denote the physician's specialty, i denote the type of service, and t denote a day in the study period. Y_{ijt} and P_{ijt} denote the number of services provided and fee. The parameter of interest is α_j which is reported as an elasticity. Elasticities are calculated using the fee parameter, and the ratio of the average number of services provided and fees. X_{ijt} represents a vector of dummy variables for services, service amendments, holidays, weekdays, and fixed-fee periods. X_{ijt} also includes a time trend, time trend squared, and number of physicians.

Different regressions are estimated for different physician specialties, which is denoted by the j on the parameters. Estimating separate parameters for each specialty relaxes the assumption that relative fees and other covariates affect service provision identically across the different specialties. Each physician specialty differs in its set of services, e.g., General Practice physicians primarily provide consultations while General Surgery physicians primarily perform surgeries. The difference in the type and frequency of service provision across specialties affects the responsiveness of each specialty to substitute across services when relative fees change.

A key feature of the empirical framework is that service fees are determined by the government[12]. Patients do not pay the fee and are unaware when fees change across time. The unawareness prevents patients from responding to fee changes and adjusting their utilization across time. The result is both observed and unobserved patient characteristics are the same before and after fee changes. Physicians are aware of fee changes but cannot bill fees that differ from the government determined fee schedule. Physicians can, however, respond to relative fee changes by adjusting the amount of each service they provide. Further, patients' and physicians' inability to influence fees allows this paper to use aggregated data rather than patient or physician level data.

Another important feature of the empirical framework is it accounts for medical advances in practice and technology. Many of the services have, for example, introduced new medical equipment, re-defined overtime hours, updated diagnosis criteria, etc. over the study period. Dummy variables for service amendments are introduced into the specification to account for each service's advancement in practice and technology over the study period[2;3]. In addition, fixed effects for services are introduced to control for differences in the frequency of service provision across services. Particularly, consultation services are more frequently provided than surgical services because consultations differ in the degree of complexity, time involved, and the number of support staff needed. Without fixed effects, the changes in consultation services would dominate changes in surgical services.

Five regressors account for changes in time correlated with service provision and fees. First, dummy variables for fixed-fee time periods are equal to one when a fee is constant for a time period and zero otherwise. Introducing the dummy variables holds all unobservable variables constant when relative fees changes, such as changes in physician income, labor, hospital beds availability, and hospital protocol, etc. The inclusion of the dummy variables results in the parameter on fees being interpreted as the effect of relative fees on the substitution across services rather than changes in total number of services provided for all services. Particularly, relative fees is defined as the change in a service's fee from the average fee for all other services. Substitution is defined as change in the number of services provided for a service from the average number of services provided for all other services. Thus, the inclusion of fixed-fee time period dummies demeans all variables the average of the variable for all other services for each fixed fee time period.

Second, the time trend represents days over the six-year time period, which controls for service provision increasing/decreasing across time. Third, time trend squared accounts for non-linear changes in time that is correlated with service provision and fees.

Fourth, weekday and holiday dummy variables control for difference in clinic hours across days. Last, the number of physicians accounts for changes in the number of physicians across fiscal years. Without this covariate, the increase in services with increasing relative fee may be due to new physicians migrating into the health region rather than existing physicians substituting across services. Particularly, new migrating physicians may prefer to provide more of a type of services than existing physicians, which happen to be services with relative fee increases.

The count data model, Negative Binomial regression model is used to estimate the fee elasticity. The Negative Binomial regression model is used since physicians never provide fractional services or fewer than zero services. Following Green 2003, the Negative Binomial regression model is a general version of the Poisson regression model. Particularly, the Negative Binomial regression introduces an individual, unobserved effect u_i (random effect) to the standard Poisson regression model. Moreover, the Negative Binomial regression can be expressed as a mixture of Poisson and Gamma distributions.

$$f(y_{it}|x_{it}) = \int_0^\infty \frac{e^{-\lambda_{it}u_i}(\lambda_{it}u_i)^{y_{it}}}{y_{it}!} \frac{\theta^\theta}{\Gamma(\theta)} e^{-\theta u_i} u_i^{\theta-1} du_i \tag{2.2}$$

Integrating over u_i and simplifying the above obtains:

$$f(y_{it}|x_{it}) = \frac{\Gamma(\theta + y_{it})}{\Gamma(y_{it}+1)\Gamma(\theta)} \left(\frac{\lambda_{it}}{\theta + \lambda_{it}}\right)^{y_{it}} \left(\frac{\theta}{\theta + \lambda_{it}}\right)^{\theta} \tag{2.3}$$

The above distribution has a conditional mean of λ_{it} and conditional variance of $\lambda_{it} + \frac{\lambda_{it}^2}{\theta}$ [60;73]. While accounting for the count nature of the data, the model also accounts for overdispersion, i.e., when the variance is greater than the conditional mean of the dependent variable. When the overdispersion parameter, θ approaches infinity, the Negative Binomial regression reduces to the Poisson regression. The Results and Discussion section reports $\alpha = \frac{1}{\theta}$ and provides evidence whether α is significantly different from zero.

2.3 Data Description

The physician claims data used in this paper provides two main benefits - exogenous service fees and a comprehensive set of physician services. Fees are exogenous because physicians receive fees from a FFS system that follows a publicly governed schedule. Fee changes stem from changes in the Alberta Medical Services Budget (MSB) which occur every three years[4]. Negotiations between the Alberta Medical Association, Alberta Health and Wellness, and regional health authorities determine MSB changes. When the three members negotiate the budget, they consider changes in the population, physician labor supply, number of insured services, medical liability insurance rates, accessibility to health care, unexpected utilization, and cost of living in Alberta[4].

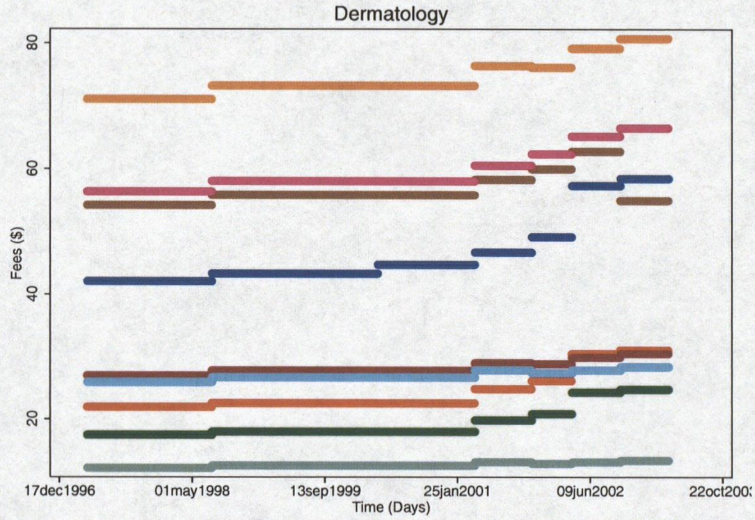
Once the budget negotiations are finished, the Alberta Medical Association coordinates an internal process among physicians to allocate the budget. The first phase of the process divides the budget across the specialties based on specialties' overhead, number of full-time physicians, and health service amendments. The second phase of the process divides each specialty's allocated budget across their services. Physicians within each specialty decide amongst themselves which services to prioritize for fee increases. Fees for the same service may differ across specialties, e.g., 03.03A codes a general clinical visit which ranges in price from \$19.80 to \$29.69 for the 11 specialties. The specialty that provides the service the most decides the amount of the fee. There are 12 dates when the fees exogenously changes from April 1, 1997 to March 31, 2003. The dates are shown in Table 2 of the Appendix.

The second benefit of the data is it consists of almost all services that physicians may substitute across in the Calgary Health Region (CHR). Because the provincial government pays for all insured physician services, the bulk of physicians' income comes from the FFS. Consequently, the data does not exclude any service that physicians can substitute to when relative fees change. Only services that are excluded are services not provided for the entire study period, or services not representing the specialty's most provided services. There are 217 services used in the regressions, representing approximately 90% of the total services provided (the percentage for each specialty is shown in Appendix). For days when there is no provision of a specific service, zeros are imputed to construct a full panel of 2,191 days by services.

The paper includes only 11 of the 62 physician specialties because many of the excluded specialties are small and consequently make very few claims annually. Inclusion of small specialties would result in inconsistent and inefficient estimates. Further, private payers pay for most of the services provided in many of the excluded specialties, such as Chiropractics, Dentistry, and Optometry, so that the model is not identified. Nevertheless, the 11 included specialties capture 63.69% of the total claims, 59,870,408 in the CHR. The data also accounts for the number of physicians in Alberta for each fiscal year and specialty. The number of physicians in Alberta is an adequate proxy for the number of physicians in the CHR, given that all physicians in Alberta face the same fee schedule. Thus, differences in annual change in number of physicians across health regions is uncorrelated with fee schedule changes because Alberta physicians have the same financial incentives across different health regions as the fee schedule changes.

The paper excludes all fees that deviate from the fee schedule. Fees can deviate due to administrative error, or for services involving unusual complications of care[2;5–7;20]. The physician's surgical method, role during a procedure, or patients' age typically cause unusual complications of care, which needs to be documented when claimed. Excluding claims with deviating fees decreases the total number of claims by 1.42% for the 11 specialties (shown in the Appendix). An example of a fee schedule is shown in Figure 2.1 for Dermatology. The nine step-functions represent fee schedules for the nine different services provided by Dermatology. A large increase in a fee schedule relative to other

Figure 2.1: Example of a Fee Schedule



fee schedules represents a change in relative fees, which is used to identify the empirical model.

Table 2.1 describes the total amount claimed and number of services provided for the entire study period by each specialty. General Practice claims the largest average annual amount at \$105.5 million dollars while Nephrology claims the smallest at \$1.5 million dollars. General Practice provides the most total services at an average of 4.325 million claims and claims one of the smallest average fee at \$43.1. In contrast, Gastroenterology provides the fewest services at an average of 24.9 thousand claims and claims one of the highest fee at \$96.9. General Surgery and Obstetrics/Gynaecology physicians provide the most different types of services while Neurology and Nephrology physicians provide the fewest. Table 2.1 also shows the daily average number of service provided and variance. The descriptive statistics show that the variance is significantly larger than the average for all specialties, which suggests that Negative Binomial regression is more appropriate for the count data than Poisson regression.

Table 2.1: Descriptive Statistics

Physician Specialty	Annual Avg. Amount Claimed ($Millions)	Average Fee ($)	Annual Avg. Number of Services (Thousands)	Types of Services*	Daily Avg. Number of Services	Variance of Services Provided	Number of Observations
Neurology	2.3	55.0	26.4	4	19.3	746.4	8764
Internal Medicine	11.8	46.3	243.1	27	26.2	2426.5	59157
Urology	2.3	143.5	33.7	20	4.9	184.9	43820
Dermatology	4.5	38.7	147.5	9	47.8	4212.1	19719
Gastroenterology	2.6	96.9	24.9	8	9.2	187.9	17528
General Surgery	6.4	227.0	84.9	46	5.4	461.6	100786
Orthopedics	4.3	249.6	65.2	28	6.8	754.2	61348
Nephrology	1.5	63.8	28.4	3	28.0	2320.7	6573
Obstetrics/Gynaecology	7.7	104.0	153.4	39	11.5	977.7	85449
Cardiology	7.6	93.5	135.5	12	33.0	3329.2	26292
General Practice	105.5	43.1	4325.0	21	600.9	5662505.0	46011

NOTES:
*Types of Services are the number of different services that each physician specialty provides
Number of services provided and fees are the average for a service per day

2.4 Results and Discussion

Table 2.2 shows that the fee elasticities are large and positive for most of the physician specialties, ranging from -0.22 to 2.91. The t-statistics calculated from robust standard errors suggest that 7 of the 11 specialties are statistically significant at the 95% confidence level. The results suggest that, in general, physicians substitute to services with increasing relative fees. The finding that most specialties respond positively to increasing relative fees is a significant departure from the literature. Past studies evaluating similar number of services or physician specialties find as many positive as negative responses[48;67], resulting in inconclusive interpretations. Further, the result is more intuitively plausible than finding physicians substituting to services with decreasing relative fees. That is, economic theory suggests that the substitution effect should be positive.

The magnitude of the fee elasticities differs significantly across physician specialties. The fee elasticities for Cardiology and Obstetrics/Gynaecology are small but statistically significant. The results suggest the two specialties are slightly responsive to fees. The results for Cardiology are inconsistent with the literature. Yip (1998) finds an opposite relationship, i.e., fee reductions for coronary artery bypass grafting result in an increase in service provision for both Medicare and private markets. The results differ from Yip's study because this paper differs in institutional backgrounds and focuses on physician's substitution across services. In contrast, Yip's study focuses on the total supply of heart revascularization surgical procedure.

The positive elasticity found for Obstetrics/Gynaecology is consistent with Gruber et al. (1998). Those authors, however, estimate a larger elasticity at 0.84 than this paper. The difference is Gruber et al. (1998)'s estimates are based on a relative fee between two procedures, i.e., cesarean and natural deliveries, while this paper's estimates are based on all services provided by Obstetrics/Gynaecology physicians. Thus, Obstetrics/Gynaecology physicians may substitute to services with increasing relative fees, but not to the same extent as previously shown in the literature. In addition, of course, the physicians in this paper may behave differently from those in other situations and countries.

Physicians from four specialties in my dataset (namely General Surgery, Neurology, Gastroenterology, and General Practice) appear not to respond to relative fees. The fee elasticities are close to zero and are statistically insignificant except for Neurology. The small fee elasticity for General Practice is unexpected because the bulk of their services are consultations and preventive care. Substituting across these services may have less effect on patient welfare than procedural services, so General Practice physicians should find it easier to substitute across these services. The literature finds mixed results. Nassiri and Rochaix (2006) found evidence of substitution behavior for General Practice, finding a 0.2141% fee elasticity. Carlsen et al. (2003) and Hughes and Yule (1992), however, find an inelastic fee response.

The fee elasticities for Dermatology, Nephrology, Urology, Orthopedics, and Internal Medicine are large, positive, and statistically significant (except for Urology). The empir-

Table 2.2: The Effect of Relative Fees on the Substitution Across Services Supplied

	Negative Binomial					
Physician Specialty	Coeff.	t-ratio	Elasticity	α	95% Confidence Interval	
Neurology	-0.004	-2.17	-0.22	0.422	0.381	0.466
Internal Medicine	0.011	4.54	0.50	0.580	0.566	0.594
Urology	0.003	1.53	0.41	0.902	0.834	0.975
Dermatology	0.075	14.33	2.91	0.231	0.216	0.247
Gastroenterology	0.001	1.03	0.14	0.134	0.125	0.143
General Surgery	0.000	1.38	0.10	0.543	0.524	0.563
Orthopedics	0.002	5.56	0.47	0.311	0.290	0.332
Nephrology	0.017	2.85	1.09	0.909	0.855	0.966
Obstetrics/Gynaecology	0.001	2.68	0.12	0.597	0.584	0.610
Cardiology	0.001	3.68	0.14	0.441	0.420	0.462
General Practice	0.000	-0.28	-0.01	0.478	0.469	0.487

NOTES:
Elasticities are calculated using the mean of the number of services provided and fees. The regression include covariates for services, service amendments, holidays, weekdays, and fixed-fee periods, time trend, time trend squared, and number of physicians.
T-ratios are calculated with robust standard errors.
α = the overdispersion parameter in the Negative Binomial regression

ical literature on these specialties has not been well researched, so drawing comparisons is difficult. Nevertheless, Hurley et al.'s study supports the large positive fee elasticity for Dermatology found in this paper. Hurley et al.'s study regresses the utilization variable, annual services provided per physician onto a fee index, and finds a large positive coefficient. Mitchell et al. (2000) estimates fee elasticity for the provision of bone joint procedures by Orthopedics. The authors estimate elasticities of 0.58 with OLS and 0.51 with instrumental variables, which match the magnitude and direction of this paper's fee elasticity. However, Tai-Seale et al. (1998)'s estimate of a large negative price elasticity, -1.03 for Urology contrasts with the results of this paper, and is arguably inconsistent with economic theory and common intuition.

2.4.1 Graphing the Substitution Effect

The results in Table 2.2 can be graphed for each specialty. To construct the graph, first, the number of services provided is regressed on all covariates except fees, and then the residuals are saved. Next, the service fees are regressed onto all covariates except number of services provided, and then the residuals are saved. In addition, both of the residuals are divided by the mean of the variable and averaged for each fee constant time period, so the specialties are comparable in these standardized residuals. Last, the first regression's standardized residuals are plotted against the second regression's standardized residuals. The graphs can be interpreted as showing the amount provided of each service as a function of its relative fees, while all else is constant.

The graphs show that most of the scatter plots are clustered along the axes for the specialties with small or insignificant fee elasticities (e.g. Neurology). In contrast, the graph for Dermatology (which has a large fee elasticity) illustrates a clear positive relationship between relative fees and number of services provided. Internal Medicine and Urology have similar upward-sloping scatter plots. Nephrology's large fee elasticity differs from the other four specialties because the elasticity can mainly be attributed to changes from a single service, i.e., hemodialysis. Nephrology's fee elasticity is sensitive to changes in the provision of this service because the specialty only provides three types of services. Further, the positive response to changes in relative fees for this service is large because there is a small percentage of days when the service is not provided relative to the other two services.

2.4.2 Robustness and Model Specification

Table 2.3 shows the estimated model's robustness and specification. First, the model is estimated without days without service provision. This specification shows that imputing days without service provision has little effect on the estimates. The direction and mag-

Figure 2.2: The Substitution Across Services by Physician Specialty

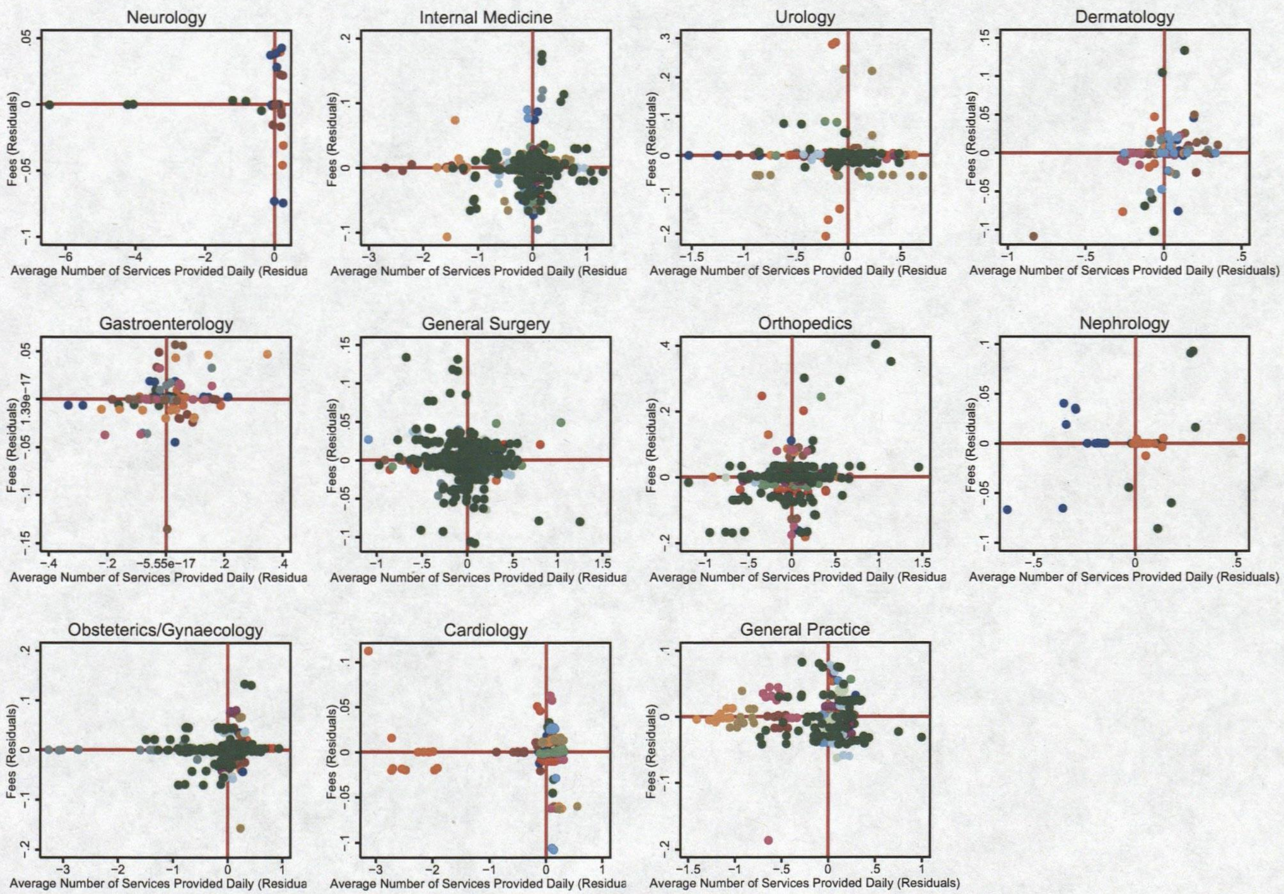


Table 2.3: Robustness and Specification (a)

	Negative Binomial			Negative Binomial No Service Provision			Negative Binomial Aggregated to Weeks			Negative Binomial Services Above 75^{th} Percentile		
Physician Specialty	Coeff.	t-ratio	Elasticity	Coeff.	t-ratio	Elasticity	Coeff.	t-ratio	Elasticity	Coeff.	t-ratio	Elasticity
Neurology	-0.004	-2.17	-0.22	0.001	0.77	0.08	0.002	0.99	0.10	0.001	0.60	0.05
Internal Medicine	0.011	1.54	0.50	0.010	5.03	0.46	0.011	1.14	0.52	0.018	7.39	0.90
Urology	0.003	1.53	0.41	0.007	5.37	1.01	0.008	4.37	1.09	0.001	2.03	0.63
Dermatology	0.075	14.33	2.91	0.064	13.70	2.42	0.056	7.73	2.19	0.078	13.16	2.40
Gastroenterology	0.001	1.03	0.14	0.002	1.29	0.17	0.002	1.23	0.19	0.002	1.03	0.13
General Surgery	0.000	1.38	0.10	0.000	0.08	0.00	-0.001	-1.69	-0.14	0.001	2.27	0.15
Orthopedics	0.002	5.56	0.47	0.002	6.09	0.12	0.002	4.95	0.47	0.005	6.95	1.17
Nephrology	0.017	2.85	1.09	0.013	3.08	0.84	0.011	1.89	0.73	0.017	2.85	1.09
Obstetrics/Gynaecology	0.001	2.68	0.12	0.001	3.13	-0.12	0.002	3.11	0.17	0.000	0.16	0.01
Cardiology	0.001	3.68	0.14	0.002	6.02	0.16	0.002	5.52	0.23	0.004	9.84	0.38
General Practice	0.000	-0.28	-0.01	-0.001	-0.86	-0.03	-0.002	-2.47	-0.10	-0.017	-6.24	-0.69

NOTES:
- Elasticities are calculated using the mean of the number of services provided and fees

All regressions include covariates for services, service amendments, holidays, weekdays, and fixed-fee periods, time trend, time trend squared, and number of physicians.

T-ratios are calculated with robust standard errors.

No Service Provision = the regression excludes all days without service provision

Aggregated to Weeks = the regression aggregates the panel of days to weeks for each service

Services Above 75^{th} Percentile = the regression include services only above the 75^{th} percentile of the most provided services

nitude of all elasticities remain about the same. Only the elasticities for Neurology and Urology are affected by the exclusion. Although the direction of Neurology's elasticities changed from dropping the exclusion, the elasticity is small and imply physicians from Neurology are non-responsive. The increase in Urology's fee elasticity with the exclusion is due to the large percentage of days with no service provision, i.e., 57.32%. Excluding days with no service provision excludes periods of zero non-responsiveness when relative fees change, so the fee elasiticity represents only time periods of service provision.

Second, the model is estimated with the panel data of days aggregated to weeks for each service. The mean is found for number of services provided, while the maximum value for each week is found for all other covariates. The intent is to average out fluctuations in service provision, which may have produced the positive fee elasticities for the panel of days by service. The estimates differ little in magnitude or direction from the aggregation. Only the magnitude and/or sign change for specialties with small elasticities except for Urology. The positive fee elasticities could also be produced from providing services that are infrequently provided. The model is estimated with only services that are above the top 75^{th} percentile of the most frequently provided services. Again, the estimates differ little in magnitude and direction from excluding infrequently provided services except for the large and statistically significant negative elasticity for General Practice. The result for General Practice may imply that physicians from this specialty tend to substitute to services infrequently provided.

Table 2.4 shows when the estimated model excludes fixed effects for services or dummy variables for service amendments. The estimates differ significantly in magnitude and direction by excluding fixed effects either for services or dummy variables for service amendments. The exclusion of service amendment dummy variables results in many of the specialties switching signs or smaller positive fee elasticity than the originally estimated model. For example, Urology switches from 0.41 elasticity to a -1.30 elasticity. Most specialties, however, still have positive fee elasticities. The exclusion of service fixed effects results in switching to negative elasticities for most specialties. That is, estimates without service fixed effects changes the sign for 8 of the 11 specialties, which are all statistically significant at the 95% confidence level. The change in signs can be attributed to most frequently provided services, i.e., consultations tending to have lower fees than infrequently provided services. Without service fixed effects, the response for frequently provided services dominates the response for all other services.

The last robustness check is to estimate the model with OLS rather than Negative Binomial regression. Negative binomial regression is appropriate for the model given the nature of the underlying count data. Further, the overdispersion parameter differing significantly from zero suggests that Negative Binomial regression is more appropriate than the Poisson regression. OLS estimates, nevertheless, differ significantly from Negative Binomial Regression estimates. For example, Nephrology changes from a large positive fee elasticity, 1.09 to a large negative fee elasticity, -7.63. Further, the magnitude of the fee elasticities for OLS differ significantly from Negative Binomial regression.

Table 2.4: Robustness and Specification (b)

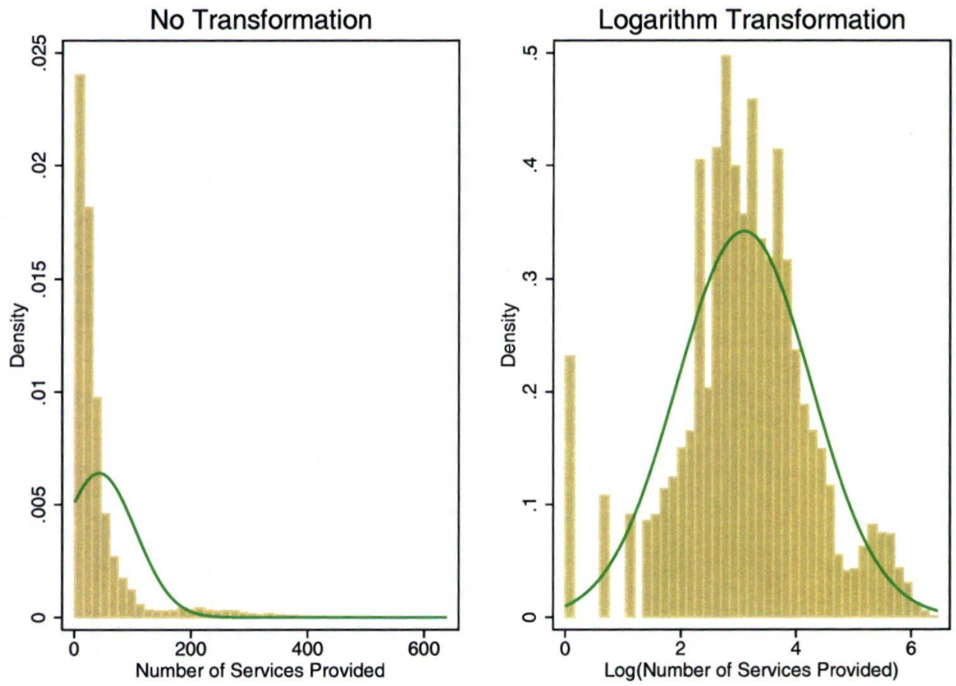
	Negative Binomial			Negative Binomial No Service Amendments			Negative Binomial No Services			OLS		
Physician Specialty	Coeff.	t-ratio	Elasticity	Coeff.	t-ratio	Elasticity	Coeff.	t-ratio	Elasticity	Coeff.	t-ratio	Elasticity
Neurology	-0.004	-2.17	-0.22	-0.036	-14.25	-1.96	0.025	56.94	1.37	0.201	2.29	0.57
Internal Medicine	0.011	4.54	0.50	0.014	9.04	0.64	0.007	20.73	0.32	0.024	0.26	0.04
Urology	0.003	1.53	0.41	-0.009	-4.91	-1.30	-0.004	-63.10	-0.57	0.016	2.47	0.47
Dermatology	0.075	14.33	2.91	0.033	10.87	1.28	-0.030	-61.23	-1.15	1.441	5.32	1.17
Gastroenterology	0.001	1.03	0.14	0.000	-0.45	-0.02	0.001	4.17	0.11	0.117	2.13	1.23
General Surgery	0.000	1.38	0.10	-0.001	-3.53	-0.22	-0.003	-75.36	-0.59	-0.003	-1.60	-0.12
Orthopedics	0.002	5.56	0.47	0.001	4.36	0.28	-0.002	-35.66	-0.38	-0.002	-0.77	-0.08
Nephrology	0.017	2.85	1.09	0.004	1.49	0.24	-0.028	-19.47	-1.79	-3.351	-12.23	-7.63
Obstetrics/Gynaecology	0.001	2.68	0.12	0.000	0.90	0.03	-0.003	-66.50	-0.27	0.006	1.15	0.06
Cardiology	0.001	3.68	0.14	0.001	3.64	0.14	-0.005	-82.31	-0.47	-0.197	-7.28	-0.56
General Practice	0.000	-0.28	-0.01	0.001	2.19	0.05	-0.043	-96.62	-1.86	-0.610	-0.97	-0.04

NOTES:
- Elasticities are calculated using the mean of the number of services provided and fees
All regressions include covariates for services, service amendments, holidays, weekdays, and fixed-fee periods, time trend, time trend squared, and number of physicians if not noted below
No Service Amendments = the regression excludes dummy variables for service amendments
No Services = the regression excludes fixed effects for services
T-ratios are calculated with robust standard errors.

OLS assumes the dependent variable is continuous and can range from negative to positive infinity. Further, the error terms in OLS are often assumed to be independently and identically distributed[61]. The assumptions may be inappropriate for this paper's count data, given the non-negative values and skewed distribution of number of services provided. OLS estimates tend to be larger in magnitude than Negative Binomial estimates because OLS can predict negative number of services provided. The dependent variable, number of services provided can be transformed by taking the logarithm. The transformation reduces the skewness of the distribution.

Figure 2.4.2 shows the change in distribution of the number of services provided from the transformation. Cardiology is used as an example because Cardiology has a negative fee elasticity when their untransformed number of services provided is estimated with OLS. Figure 2.4.2 shows that the untransformed number of services provided are positively skewed and has a mean of 33.0 and variance of 3329.2. The histogram for the log of number of services provided is more symmetrically distributed over the mean of 3.10 than the untransformed number of services provided.

Figure 2.3: Skewness of Number of Services Provided: Example of Cardiology



The transformed number of services provided can be regressed onto the covariates, which is called a loglinear regression. A test to determine whether a linear regression is more appropriate than a loglinear regression for the data can be generated with a Box-Cox regression. The Box-Cox regression, first, transforms the dependent variable by the following:

$$B(Y_{ijt},\lambda) = \begin{cases} \frac{Y_{ijt}^{\lambda}-1}{\lambda} & \text{when } \lambda \neq 0 \\ \log Y_{ijt} & \text{when } \lambda = 0 \end{cases}$$

Second, the transformed dependent variable is regressed onto the covariates, which is shown in Equation 2.4[61]. The model reduces to a loglinear regression when $\lambda = 0$, but reduces to a linear regression when $\lambda = 1^*$. The difference between the log-likelihood functions when λ is restricted to 1, $l(1)$ and when λ is restricted to 0, $l(0)$ tests whether the linear or loglinear model is a better specification. Particularly, the null hypothesis for a linear regression model, or $\lambda = 1$ can be rejected when the test statistic $2(l(0) - l(1)) > \chi^2_{0.95}(1)$. The critical value is $\chi^2_{0.95}(1) = 3.84$ for a 95% confidence level.

$$B(Y_{ijt}, \lambda) = \alpha_j P_{ijt} + X_{ijt}\beta_j + u_{ijt} \tag{2.4}$$

Table 2.5 shows that the test statistic, $2(l(0) - l(1))$ is greater than the critical value for all physician specialties, so the loglinear regression is the preferred specification. The estimates from the loglinear regression have the same direction as the estimates from the Negative Binomial regression. Further, loglinear estimates are similar in magnitude as Negative Binomial estimates. Particularly, Dermatology has a large positive fee elasticity while General Practice and General Surgery have small elasticities. Only Nephrology's elasticity decreases significantly in magnitude from 1.09 to 0.14.

The magnitude of elasticities differ from Negative Binomial regression because the log of number of services provided can not be calculated for days when no services are provided. For example, Urology's fee elasticity increases when estimated with Negative Binomial regression without days of zero service provision or loglinear regression. Thus, estimates from Negative Binomial regression are preferred over loglinear regression because it accounts for both days without service provision and positive skewness. Further, the difference in interpretation is minimal between Negative Binomial and loglinear regression.

2.5 Conclusion

Past empirical studies have provided evidence of physicians responding to relative fees. The lack of robust estimates and many methodological issues, however, have allowed some to question the empirical evidence[71]. Policy makers need to know whether the types of services provided are affected by relative fee changes, and whether the services

*By l'Hospital's Rule, $\log Y_{ijt}$ is the limit of $\frac{Y_{ijt}^{\lambda}-1}{\lambda}$ as $\lambda \to 0$.

Table 2.5: Robustness and Specification - Testing a Loglinear Model

	Negative Binomial				Loglinear		
Physician Specialty	Coeff.	t-ratio	Elasticity	$2(l(0) - l(1))$	Coeff.	t-ratio	Elasticity
Neurology	-0.004	-2.17	-0.22	6748.28	-0.001	-0.36	-0.02
Internal Medicine	0.011	4.54	0.50	88644.84	0.016	6.01	0.29
Urology	0.003	1.53	0.41	46037.27	0.005	4.12	0.58
Dermatology	0.075	14.33	2.91	15552.74	0.112	15.42	1.25
Gastroenterology	0.001	1.03	0.14	19009.76	0.003	1.19	0.13
General Surgery	0.000	1.38	0.10	184354.50	0.000	1.96	0.07
Orthopedics	0.002	5.56	0.47	95168.76	0.001	4.34	0.24
Nephrology	0.017	2.85	1.09	9709.03	0.005	0.86	0.14
Obstetrics/Gynaecology	0.001	2.68	0.12	182914.38	0.001	3.38	0.08
Cardiology	0.001	3.68	0.14	37377.82	0.002	4.46	0.06
General Practice	0.000	-0.28	-0.01	289677.32	-0.001	-1.62	-0.01

NOTES:
- Elasticities are calculated using the mean of the number of services provided and fees
All regressions include covariates for services, service amendments, holidays, weekdays, and fixed-fee periods, time trend, time trend squared, and number of physicians.
$2(l(0) - l(1))$ = Twice the difference between the log-likelihood functions of the Box-Cox regression when λ is restricted to zero and one.
Loglinear = the regression takes the logarithm of number of services provided, which is then estimated with OLS.
T-ratios are calculated with robust standard errors.

chosen by physicians increase government expenditures. This paper takes advantage of exogenous variations in a publicly-determined fee schedule to identify the relationship in a large Canadian health region. The physician claims data used in this paper accounts for the universe of the public health care system, where all services that physicians can substitute across are observed over a 6 year-period.

The results suggest that physicians substitute to services with increasing relative fees given that most specialties are found to have positive fee elasticities. The size of the elasticity varied across specialties. Physicians from specialties like General Practice are not responsive to relative fee changes. The fee elasticities are significant for Dermatology and Nephrology when their relative fees change. Thus, future fee schedules should account for specialties' responsiveness to relative fees, in order to provide a set of services that optimizes patient care.

Chapter 3

Physician Labor Supply: Evidence from Comprehensive Physician Claims Data

3.1 Introduction

Increases in physician wages are used to encourage physicians to supply more services, work longer hours, and attract more physicians to the region[43]. The increase in wages may, however, not encourage physicians to supply more services if physicians are unwilling to trade leisure for work as their wage increases. One hypothesis is that physicians are unresponsive to wage increases because of their relatively high incomes, or could even reduce hours worked as wages per hour increase. This paper explores physician responses to wages by examining the number of services supplied as physicians' fees are increased. Determining physicians' response to fee increases has implications to healthcare expenditures and patient care. The direction and magnitude of physicians' response affects the amount of healthcare expenditure spent on physician incomes, as well as the amount of services provided to patients.

The literature on physicians' labor response to wages is extensive and many approaches have been taken to estimate the relationship[68]. An indirect approach is to determine the effect of physician-population ratio on the number of services supplied. The intuition behind this approach is that as the number of physicians per patient increases, competition for patients increases among physicians. With fewer patients per physician, doctors have an incentive to induce demand for their services, in order to maintain their income. Finding the average number of services per physician increasing as the physician-population ratio increases is indirect evidence of a backward-bending supply function.

The main concern with this approach, however, is that it may be confounded by reverse causation[21]. High demand for physician services from a large proportion of unhealthy patients in an area may cause physicians to migrate into the area, leading to the observed relationship. Another possible confounder is that the rise in physician-population ratio reduces the travel distance between patients and physicians, which improves the accessibility and consequently increases utilization. Both explanations potentially confound the effect of physician-population ratio on the number of services supplied.

The result is a lack of consensus in the papers using the indirect approach. Delattre and Dormont (2002) use a panel of 8000 French self-employed physicians to show that an increase in physician-population ratio results in supply rationing, but increases the intensity of care for each patient. The authors use generalized method of moments estimators with physician fixed effects to estimate elasticities of -0.201 and 0.088 for the number of encounters and intensity of care, respectively[24] . Sorensen and Grytten (1999) find a different result. For Norwegian physicians working in a fixed-fee regime, physicians did not supply more services or increase the intensity of care for each patient when the physician-population ratio increased. The authors find insignificant estimates, which consequently provides no evidence of the inducement hypothesis[63].

Past studies of the indirect approach have also used exogenous variations from government determined fee schedules instead of physician-population ratio to identify physician labor responses. Many US studies focus on physician fees paid by publicly-funded healthcare programs such as Medicare and Medicaid[23;48-50;58;66;69;72]. Feldstein (1970), Hu and Yang (1988), and Escarce (1993) use time series data to estimate the supply of all services or a specific set of services, and find inconclusive and/or contradictory evidence. Tai-Seale et al. (1998), Yip (1998), and Rice et al. (1999) take a different approach and control for physicians' ability to substitute between privately and publicly paid services as their relative fees change. Again, the results are shown to be either inconclusive or not robust across studies. Mitchell et al. (2000) and Mitchell et al. (2002) add to the literature by using a cross-price elasticity to account for physicians' ability to substitute across services. The authors find that Opthamologists and Orthopedic Surgeons provide fewer cataract and joint/hip procedures as their fees are reduced. The direction of the cross-price elasticities are reported to be inconclusive given the invalidity of their instrumental variables.

The difficulty with the above indirect approaches is that finding an increase in the number of services supplied as fees rise does not imply physicians are trading leisure for work hours, in order to supply additional services. The additional services may be provided by the same number of work hours as before the fee increase[17;42;44]. A direct approach determining physicians' supply responses to fee increases is to evaluate the number of hours supplied. Using this approach, Rizzo and Blumenthal (1994) estimate an income effect of -0.26% and substitution effect of 0.49% given a 1% increase in wages. The net effect implies a positive relationship between hourly wages and the number of hours supplied[47].

Thornton and Eakin (1997) and Thorton (1998) use an empirical framework that accounts for physicians' production and labor supply decisions. The authors estimate a production function by regressing the number of patient visits on the number of hours and other input characteristics to estimate a shadow wage, which is then incorporated into the labor supply equation[46]. Thornton and Eakin (1997) find that physicians allocate less time to medical practice activities when hourly earnings and non-practice income increases. Thorton (1998) finds differing evidence that physicians are not responsive to increases in their marginal hourly earnings and non-practice income. The authors conclude that physicians occupy the upward portion of the supply curve, where there's a positive but inelastic relationship between fees and supply of patient visits. The implication is that cost containment policies used to reduce fee would not imply that physicians would increase inducement of services[45].

The main issue dealt in Thorton and Eakin (1997) and Thorton (1998) is using an instrumental variable strategy to account for the endogenous wage. The validity of their instrumental variable, physician work experience, is questionable when the instrumental variable has to be highly correlated with wages, but uncorrelated with the number of hours worked. Baltagi et al. (2005) improves on the two studies by identifying physician labor supply functions with an exogenous wage change. The authors use a panel of Norwegian physicians who follow a fixed fee schedule. The authors estimate short-run wage elasticities that range from 0.303 to 0.342[13]. The authors conclude that physician responses to their wages are nowhere near the backward-bending part of the labor supply curve.

Studies using either indirect or direct approach lack consensus on the direction and magnitude of physicians' response to wage increases. The literature has made significant methodological progress on which this paper builds. Using comprehensive, detailed claims data, the elasticity of physician labor supply is identified using exogenous variations in the fee schedule. The data include all physicians and their services for a large Canadian health region. Further, the empirical framework accounts for changes in services due to medical advances in practice and technology. The estimated model shows that physicians supply significantly more services when their fees increase. All physician specialties are found to have a positive relationship between fees and number of services supplied. However, the magnitude of physicians' response differs across specialty: General Surgery is the least responsive while Nephrology is the most responsive.

3.2 Empirical Framework

A key advantage of using physician claims data from Canada's public health care system is that physician fees are not determined by patient demands. Patients do not pay for physician fees and consequently are unaware of them. Physicians, however, are aware of how much they are paid for their services but cannot charge more or less than the fee determined by the government. This institutional arrangement allows the estimation of the physician supply function independent of the patient demand function[12]. Further, the inability of physicians to charge fees differing from the government-determined fee schedule allows the use of aggregate panel data of services by days rather than physician level data. That is, observable or unobservable differences across physicians do not affect the fees charged, so physicians can only change the supply of services when fee changes.

Using this data is not without complications. The first problem is that physicians provide many different types of services to their patients. If physicians provide only one service then estimating a supply function is straightforward. The supply function is estimated by regressing the number of services provided for that one service onto its fee. The diverse set of services physicians provide, however, requires aggregating the panel data by services, i to generate a time series for the number of services provided, Q_t and fees, P_t. Different services require different levels of patient care, and consequently are provided at different frequency and fees. Particularly, consultations tend to be frequently provided and billed at low fees, while surgical services tend to exhibit the opposite characteristic.

The simplest solution to this aggregation problem is to weight the number of services provided with fees, and vice versa with fees. The weight for each variable is composed of two parts. The first part is an average of the weighting variable by each service's first fixed fee period, i.e., Q_{ji1} and P_{ji1}. The second part sums for all services, the average of the weighting variable for all fixed fee periods. The ratio of the two parts part generates the following: $\frac{Q_{ji1}}{\sum_{i=1}^{N} \bar{Q}_{ji}}$ and $\frac{P_{ji1}}{\sum_{i=1}^{N} \bar{P}_{ji}}$. The weight is chosen because, first, the weighted variables reflect both the frequency of provision and fee charged for each service. Second, the weighted variables also reflect the amount of physicians' revenue that each service represents.

The fees and number of services provided are weighted with each other's value from the first fee period. However, the weighted number of services provided varies by only the number of services provided, while weighted fees vary only by fees, i.e., the weight portion of the variables is held constant, using first period values. Thus, physicians' supply responses to fee changes are maintained, and both weighted variables are interpreted as standardized number of services provided and fees. The weight, however, introduces correlation between the number of services provided and fees, so all claims from the first fixed fee period are dropped from the analysis.

Another issue with using physician claims data is that physician services may have changed across time. Medical and technological advances and/or procedural changes occurring across time likely affect the number of services provided and its corresponding fee[3]. If this factor is not accounted for in the empirical framework, the relationship between service fees and number of services supplied would be confounded. To remove its effect on the relationship, the weighted fees and number of services provided are each regressed onto health service amendment dummies, A_{it}. In addition, the model includes a time trend, time trend-squared, statutory holiday dummies, and weekday dummies, which are denoted by X_{it}. Specifically, the following equations are estimated for each physician specialty:

$$P_{jit} \frac{Q_{ji1}}{\sum_{i=1}^{N} \bar{Q}_{ji}} = A_{it}\beta_{j1} + X_{it}\beta_{j2} + u_{jit} \tag{3.1}$$

$$Q_{jit}\frac{P_{ji1}}{\sum_{i=1}^{N}\bar{P}_{ji}} = A_{it}\delta_{j1} + X_{it}\delta_{j2} + z_{jit} \tag{3.2}$$

The above equations are estimated as a first stage in a two-stage empirical framework. The first stage regression is necessary because a panel of services by days is needed to control specific services amended on a specific date. If the panel dataset is collapsed to a time series of days, then the effect of the amendment for a specific service is not accounted for in the estimated model. The inclusion of the service amendments in the specification is a key advantage over past studies because of the many changes that occur in the provision of health services over time. Second, X_{it} is included in the first stage regression to account for changes in the provision of services that is correlated with time but unique for each service. Thus, the residuals from the above equations removes the effect of X_{it} and A_{it}.

Once Equations 3.1 and 3.2 are estimated, the second stage is estimated by collapsing the panel data to a time series of days by averaging the residuals across all services for each specialty. The average of the residuals across services for Equations 3.1 and 3.2 are denoted by:

$$\tilde{P}_{jt} = \frac{1}{N}\sum_{i=1}^{N}\hat{u}_{ijt} \tag{3.3}$$

$$\tilde{Q}_{jt} = \frac{1}{N}\sum_{i=1}^{N}\hat{z}_{ijt} \tag{3.4}$$

The relationship between service fees and number of services supplied is identified by regressing $\tilde{Q}_{it}$ onto $\tilde{P}_{it}$ for each specialty, which is shown in Equation 3.5. The parameter for $\tilde{P}_{jt}$ indicates the relationship between physicians' supply of standardized services and standardized fees, holding all else constant. Both regression stages are estimated with Ordinary Least Squares (OLS).

$$\tilde{Q}_{jt} = \delta_{j0} + \delta_{j1}\tilde{P}_{jt} + v_{jt} \tag{3.5}$$

3.3 Data Description

A benefit of using the physician claims data is the exogenous fee schedule. Physicians are paid by a publicly funded, Fee-For-Service (FFS) system that follows a publicly governed schedule. The budget for physician labor is negotiated by three government organizations, namely Alberta Medical Association, Alberta Health Region, and regional health authorities. Once the budget is determined, the Alberta Medical Association divides the budget across physician specialties based on the number of full time physicians, and capital and operating costs[2;4–7;20]. Once each specialty knows its budget, the specialty decides the fee for each service, subject to the overall cap.

The amount of the fee may vary across specialties. For example, a general clinical visit ranges in price from \$19.80 to \$29.69 for the 11 specialties, as described in Chapter 2. All service fees change on the same schedule which is determined by the provincial government. From April 1, 1997 to March 31, 2003, the fee exogenously changes on 12 dates. Each specialty's final dataset has zeros imputed for days when no services are provided to construct a full panel of 2,191 days by the number of services.

A significant benefit of this dataset is that it encompasses all services provided by all physicians in the Calgary Health Region. The data records all claims made by physicians for services they provided to their patients. The paper examines the 11 largest publicly-funded physician specialties, which captures 63.69% of the total claims, 59,870,408. The paper excludes services that are not provided for the entire study period. Further, services that are infrequently provided are excluded, i.e., services making-up the bottom 10% of service provision (please refer to Chapter 2 for further details). The result is 217 services are kept for the 11 specialties.

Table 3.1 shows descriptive statistics for physicians. The number of physicians and average income is shown for all physicians in Alberta because the information was unavailable for the Calgary Health Region[1]. However, all physicians in Alberta follow the same fee schedule, so the average incomes and number of physicians in Alberta are likely representative of physicians in the Calgary Health Region. Table 3.1 shows the wide differences across physician specialties, which suggest the importance of estimating the model for each specialty. First, most physicians in the Calgary Health region belong to General Practice while the fewest physicians belong to Dermatology and Urology. The lowest average income is earned by physicians from Neurology and General Practice, while Dermatology and Cardiology earn the highest average income. Second, the average number of services provided and fee in Table 3.1 is calculated for a service provided per day. Table 3.1 shows that physicians from General Practice tend to earn their income by providing services frequently while charging one of the lowest average fees at $43.12. Physicians from Orthopedics show the opposite relationship, i.e., physicians provide one of the fewest average number of services per service but charge the highest average fee of $249.57.

3.4 Results and Discussion

Table 3.2 shows the parameter for $\tilde{P}$ in Equation 3.5 for the 11 physician specialties. The results for Model 1 show clearly that all physician specialties supply more services as their fees increase. The t-statistics calculated with robust standard errors show that all parameters are statistically significant at the 95% confidence level. Both Cardiology's and General Practice's elasticity of 0.52 and 0.64 do not differ significantly from the literature. Tai-Seale et al. (1998) estimate a 0.32 elasticity for Cardiology, which is

Table 3.1: Descriptive Statistics

Physician Specialty	# of Physicians*	Avg. Income*	Avg. # of Services	Avg. Fees
Neurology	58	$169,147	19.29	$54.98
Internal Medicine	483	$246,196	26.19	$46.33
Urology	35	$376,983	4.92	$143.47
Dermatology	34	$480,242	47.80	$38.71
Gastroenterology	40	$288,611	9.17	$96.93
General Surgery	144	$317,109	5.39	$227.03
Orthopedics	113	$270,387	6.81	$249.57
Nephrology	58		28.01	$63.77
Obstetrics/Gynaecology	130	$349,249	11.49	$104.00
Cardiology	63	$465,895	32.99	$93.55
General Practice	2841	$191,354	600.91	$43.12

NOTES:
*Source: Alberta Health Care Insurance Plan Statistical Supplement for all physicians in Alberta in the 2002/03 fiscal year
**Average income for Nephrology was not available in Alberta Health Care Insurance Plan Statistical Supplement
Average number of services provided and average fees are the average for a service per day

for procedures deemed overvalued*under Omnibus Budget Reconciliation Act of 1989 (OBRA89) in the US Medicare program. Baltagi et al. (2005) estimate a 0.3 short-run elasticity for General Practice. It is not surprising that Baltagi et al. (2005)'s estimated elasticity is similar to the result presented in this paper, given the similar institutional background, i.e., single payer, public health care system with a fixed fee schedule. All other specialties, however, are found to be more responsive, i.e., from 0.45 - 1.12 than the evidence shown in the literature. Orthopedic's elasticity of 0.94, for example, is large relative to Mitchell et al. (2000)'s elasticity of 0.58 for joint surgical services, and an opposite relationship relative to the negative elasticities Rice et al. (1999) estimate for carpal tunnel release, knee arthroscopy, and total hip replacement services.

The positive relationship is apparent in Figure 3.1, which plots $\tilde{Q}_{jt}$ against $\tilde{P}_{jt}$ in Equation 3.5 for all physician specialties. Both the variables are standardized to have a mean of zero and standard deviation of one, in order for the plots to be comparable across specialties. Further, because these figures plot the variables in Equation 3.5, the

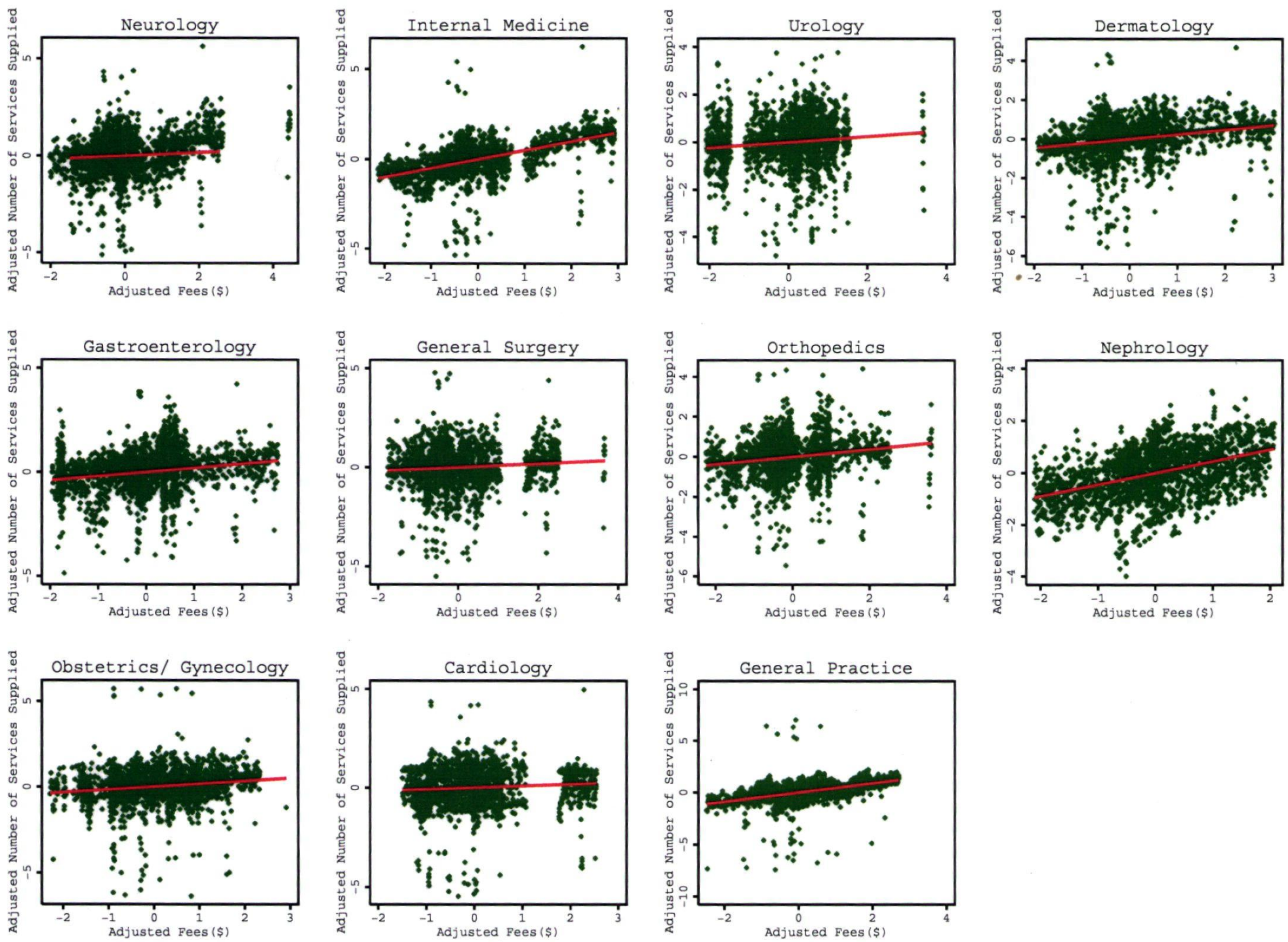
*Overvalued procedures are procedures with prevailing charges that are 85% or above the national average figures already account for the correlations between fees, number of services provided, time, service amendments, and clinic hours of operation.

Both the scatter plots and trend line show an upward slope for most physician specialties. The plots for General Surgery and Cardiology show that physicians from these specialties are not very responsive to fee increases. In contrast, the plot for Nephrology shows significant responsiveness to fee increases. Further, Nephrology's plot shows scatter plots clustering closely together, which represent the lack of days when no services are provided. The lack of days with no services provided allows Nephrologists to always respond to fee increases. Other specialties do not provide services on some days even though fees have increased, which generates the greater variability in the number of services provided for specialties other than Nephrology. The plots also show for specialties like Urology and Cardiology a block of scatter plots representing both an increase and decrease in services supplied for a 3 standard deviation increase in fees. The result is due to services that are usually provided on the weekends but not supplied for a two-week period exactly when the fee schedule increases. Services like comprehensive consultations are not provided for a couple of weekends because physicians were away at conferences and vacations.

3.4.1 Robustness and Model Specification

The sensitivity of the weight is checked by estimating Model 1 with another weight. The new weight uses each service's average of all fixed fee time periods in the ratio rather than the first fixed fee time period, i.e., $\frac{\bar{Q}_{ji}}{\sum_{i=1}^{N}\bar{Q}_{ji}}$ and $\frac{\bar{P}_{ji}}{\sum_{i=1}^{N}\bar{P}_{ji}}$. The difference in parameters between Model 1 and Model 2 is small. General Surgery's supply of services is still the most fee inelastic and Nephrology's supply of services is still the most fee elastic. All parameters are again statistically significant at the 95% confidence level. Elasticities for Gastroenterology, Orthopedics, and Cardiology are, however, shown to increase slightly in

Figure 3.1: Supply Functions by Physician Specialty



The weighted fees and number of services provided are adjusted for health service amendments, time-trend, time-trend squared, statutory holidays, and weekdays. In addition, both variables are standardized to have a mean of zero and standard deviation of one. There are 1977 observations for each graph

Model 2. In any case, the underlying results are not sensitive to these different weighting schemes.

Model 1's sensitivity to the inclusion of fixed-fee time period dummy variables in the second stage is checked in Model 3. Fixed-fee time period dummy variables are variables indicating time periods when the fee is constant. The inclusion of fixed-fee time period dummy variables in the second stage controls for all characteristics correlated with total service provision and occur when the fee schedule changes, e.g., an income effect. The income effect can be depicted in a labor supply model[34]. The physician maximizes utility in the model by choosing consumption and labor, $v(c, l)$. Alternatively, the physician can choose consumption and leisure, $v(c, L)$, where $L = \bar{L} - l$ and $\bar{L}$ = maximum number of work hours. The physician is constrained by $pc + fL = f\bar{L} + m$, where p = price of consumption goods, f = fees, and m = nonlabour income. The Slutsky equation shows the demand for leisure as fees change:

$$\frac{dL(p,f,m)}{df} = \frac{\partial L(p,f,u)}{\partial f} + \frac{\partial L(p,f,m)}{\partial m}(\bar{L} - L) \tag{3.6}$$

Equation 3.6 shows that demand for leisure is the sum of a nonpositive term and nonnegative term, which makes the demand for leisure ambiguous as fees changes[†]. The nonpositive term, $\frac{\partial L(p,f,u)}{\partial f}$ shows that as fees increase, the consumption of leisure is more expensive than before, so the physician substitutes from leisure to supply more services. This term can be called the substitution effect. The nonnegative term, $\frac{\partial L(p,f,m)}{\partial m}$ shows that increasing fees increase physician's income, so the physician demands more leisure time and supply fewer services than before. This term can be called the income effect. Estimates from Model 1 represent the net effect of the two effects, i.e., $\frac{dL(p,f,m)}{df}$, while estimates from Model 3 represent only the substitution effect, $\frac{\partial L(p,f,u)}{\partial f}$ with fixed-fee time period dummy variables in the specification.

[†]The first term is nonpositive because the utility function is assumed to be concave, while the second term is nonnegative because leisure is assumed to be a normal good.

Elasticities for Model 3 are positive for all specialties, which is consistent with the negative sign for the substitution effect, i.e., more labor is supplied and less leisure is demanded when the wage rate increases. The magnitude of the elasticities in Model 3 are similar to Model 1, implying the income effect has a small effect on physician labor supply decision. Only General Surgery and Cardiology are shown to differ significantly from Model 1. For example, Cardiology is found to have the largest elasticity of 1.39 in Model 3 while the smallest elasticity in Model 1. This result implies that physicians from Cardiology and General Surgery demand significant leisure time when their income increases with increasing fees, but still supply more services in total. This result is consistent with Rizzo and Blumenthal (1994), who found a negative income effect but positive net effect on labor supply[‡].

Model 1's sensitivity to the inclusion of number of physicians in the second stage is checked in Model 4. The intuition behind this inclusion is that the positive relationship can be explained by physicians migrating into the Calgary Health Region due to increasing fees, and consequently, supplying more services than previous periods. The number of physicians used in Model 4 represents only physicians in Alberta, so the variable is only a proxy for the number of physicians in the Calgary Health region. (However, since fees are the same across Alberta, there is no reason to expect that changes in fees would influence migration within the province.) Further, the number of physicians varies only by the fiscal year and physician specialty. Results for Model 4 show that the number of physicians has little influence on the magnitude of the parameters and no impact on the parameter signs. Only the magnitude of Dermatology's and Obstetrics/Gynaecology's parameters is shown to decrease slightly. Thus, the results suggest that the increase in number of services is due to physicians working more intensively rather than more

[‡]Rizzo and Blumenthal (1994) pools physicians from different specialties into a single regression model, which represents physicians from Pediatrics, General Internal, Internal Medicine, General Surgery, surgeons with subspecialties, Obstetric/Gynaecology, Psychiatry, and General/Family Practice

Table 3.2: Effect of Fees on the Number of Services Supplied

	Model(1)			Model(2)			Model(3)			Model(4)		
Physician Specialty	Coeff.	t-ratio	Elasticity	Coeff.	t-ratio	Elasticity	Coeff.	t-ratio	Elasticity	Coeff.	t-ratio	Elasticity
Neurology	0.33	15.07	0.96	0.33	15.08	0.95	0.38	9.70	1.10	0.33	15.09	0.97
Internal Medicine	0.56	26.41	1.00	0.57	26.69	1.01	0.62	14.43	1.10	0.56	26.42	1.00
Urology	0.03	5.03	0.84	0.03	5.27	0.89	0.03	1.96	0.87	0.03	5.02	0.84
Dermatology	1.21	10.79	0.95	1.10	11.00	0.89	1.09	4.52	0.85	0.90	6.56	0.71
Gastroenterology	0.08	9.45	0.79	0.09	10.21	0.92	0.08	5.08	0.82	0.08	9.42	0.79
General Surgery	0.01	4.08	0.45	0.01	4.56	0.50	0.03	4.21	1.21	0.01	4.08	0.45
Orthopedics	0.03	8.45	0.94	0.03	8.43	1.00	0.02	3.13	0.79	0.03	8.42	0.94
Nephrology	0.54	25.36	1.12	0.51	25.28	1.11	0.61	10.50	1.28	0.54	24.16	1.13
Obstetrics/Gynaecology	0.11	8.09	0.98	0.11	8.28	1.03	0.07	3.18	0.68	0.09	6.91	0.85
Cardiology	0.20	4.01	0.52	0.23	4.20	0.65	0.53	4.23	1.39	0.20	4.01	0.52
General Practice	9.45	23.69	0.64	8.98	23.65	0.65	10.72	13.85	0.73	9.50	23.24	0.65

NOTES:
Model (1) = OLS is used in both regression stages and Fees and Number of Services are weighted with a ratio of the weighting variable first fixed fee period for each service and the sum of all services for the average weighting variable of all fixed fee periods
Model (2) = Model (1) but the weight is the ratio of the average of each weighting variable for each service and the sum of all services for the average weighting variable of all fixed fee periods
Model (3) = Model(1) but the second stage includes fixed-fee time period dummy variables as a covariate
Model (4) = Model(1) but the second stage includes Number of Physicians as a covariate
- All weighted Service Fees and Number of Services are adjusted for health service amendments, time-trend, time-trend squared, statutory holidays, and weekdays
- Elasticities are calculated using the mean of the average weighted number of services provided and fees
- T-ratios are calculated with robust standard errors

physicians migrating into the region.

3.4.2 Placebo-Number of Services Supplied

The empirical framework is checked in Model 5. The empirical framework may be generating the strong positive relationship between the number of services provided and fees because the weight for the two variables are each other in the first stage regression. To check the empirical framework, the number of services supplied is randomly generated from a normal distribution, i.e., a placebo number of service supplied. All other variables and methods remain the same as Model 1.

First, the weight is a ratio of the weighting variable's first fixed fee period for each service and sum of all services for the average weighting variable of all fixed fee periods. Second, the weighted placebo-number of services supplied and weighted fees are regressed onto a time trend, time-trend squared, health service amendments, and statutory holidays. Third, the residuals from both regressions are averaged across services into

Table 3.3: Effect of Fees on Placebo-Number of Services Supplied

	Model(1)		Placebo	
Physician Specialty	Coeff.	t-ratio	Coeff.	t-ratio
Neurology	0.33	15.07	-0.0013	-0.81
Internal Medicine	0.56	26.41	-0.0006	-0.65
Urology	0.03	5.03	0.0032	1.40
Dermatology	1.21	10.79	-0.0020	-0.47
Gastroenterology	0.08	9.45	0.0003	0.21
General Surgery	0.01	4.08	-0.0004	-0.35
Orthopedics	0.03	8.45	0.0008	0.56
Nephrology	0.54	25.36	0.0004	0.33
Obstetrics/Gynaecology	0.11	8.09	-0.0006	-0.20
Cardiology	0.20	4.01	-0.0024	-0.62
General Practice	9.45	23.69	0.0005	0.22

NOTES:
Model (1) = OLS is used in both regression stages and Fees and Number of Services are weighted with a ratio of weighting variable's first fixed fee period for each service and the sum of all services for the average weighting variable of all fixed fee periods
Placebo = Model(1) but is estimated with a randomly generated Number of Services
- The weighted fees and number of services provided are adjusted for health service amendments, time-trend, time-trend squared, statutory holidays, and weekdays.
T-ratios are calculated with robust standard errors.

a time-series of days. Last, the averaged residuals from the placebo-number of services supplied regression is regressed onto averaged residuals from the fees regression. Table 3.3 shows that no relationship exist between the placebo-number of services supplied and fees. The parameters for all physician specialties are close to zero and not statistically significant at the 95% confidence level. Thus, the empirical framework for Model 1's specification is unlikely generating the upward sloping supply curve for all physician specialties.

3.4.3 First-Differences

The first difference of Equation 3.5 is calculated by, first, estimating the first stage regressions shown for Equations 3.1 and 3.2. However, rather than averaging the residuals to a time series of days for the second stage regression, the residuals are averaged to three different time series, i.e., months, quarters, and semi-annual periods. Second, a first difference is taken for the new time periods by subtracting the lag of the variable. Equation 3.7 shows the first difference model estimated for the three time series, t and eleven physician specialties, j:

$$\tilde{Q}_{jt} - \tilde{Q}_{jt-1} = \delta_{j0} + \delta_{j1}\left(\tilde{P}_{jt} - \tilde{P}_{jt-1}\right) + z_{jt} \tag{3.7}$$

The intuition behind estimating first-differences is that δ_{j1} measures physicians' responsiveness after a change in time. For example, the first difference between days determines physicians' average response the day after a fee change. Physicians are, however, unlikely to respond the day after a fee change because physicians might take time to notice a change in their income, or find it difficult to re-allocate resources, e.g., availability of hospital beds, ordering supplies, the number of support staff. As more time passes, physicians are likely to notice that the cost of spending leisure time has increased with increasing fees. If physicians' elasticity increases with changes in longer time periods, then physicians have lagged supply response to fee increases. Besides measuring the timing of physicians' responsiveness, first-differences also difference out the effect of time invariant unobservables on the mean level of fees and number of services supplied daily.

Table 3.4 shows that all physician specialties supply more services when their fees are increased regardless of the amount of time passed in the three aggregated time periods. Second, the positive response is large for all first-difference time periods. Third, the results show that physician specialties differ in the immediacy of physicians' response. Nephrology, Cardiology, General Practice, and General Surgery are the most responsive a month after the fee change. In contrast, Urology, Neurology, Dermatology, Gastroenterology, Orthopedics, and Obstetrics are most responsive 6 months after the fee change. Internal Medicine's responsiveness do not vary with first differences of different time periods. Thus, all physicians are still found to supply more services when fees increase regardless of the time change. However, physicians' specialty influences the immediacy

Table 3.4: Effect of Fees on the Number of Services Supplied - First Differences

	Monthly Differences			Quarterly Differences			Semi-annual Differences		
Physician Specialty	Coeff.	t-ratio	Elasticity	Coeff.	t-ratio	Elasticity	Coeff.	t-ratio	Elasticity
Neurology	0.15	1.53	0.44	0.36	4.44	1.07	0.31	3.45	0.91
Internal Medicine	0.57	4.70	1.02	0.65	7.06	1.16	0.56	4.76	1.00
Urology	0.02	1.17	0.67	0.03	1.95	0.95	0.05	1.92	1.38
Dermatology	0.83	1.41	0.65	1.23	4.03	0.97	1.23	2.94	0.98
Gastroenterology	0.07	1.86	0.74	0.08	2.37	0.80	0.09	2.10	0.95
General Surgery	0.02	1.35	0.89	0.02	1.24	0.70	0.01	0.63	0.36
Orthopedics	0.01	0.60	0.33	0.03	2.86	1.04	0.03	2.03	1.07
Nephrology	0.73	5.48	1.53	0.53	6.23	1.12	0.53	3.51	1.11
Obstetrics/Gynaecology	0.05	1.05	0.49	0.04	0.97	0.40	0.12	0.89	1.10
Cardiology	0.36	1.26	0.94	0.30	1.58	0.79	0.16	0.58	0.43
General Practice	10.46	7.22	0.71	8.72	6.52	0.59	7.05	1.87	0.48

NOTES:
The panel of services by days is averaged across services to a time series of months, quarters, and semi-annual periods. Next, first-difference is taken for each time series, which generates 64, 21, and 12 observations for Month, Quarter, and Semi-annual first-differences. Elasticities are calculated using the mean of the average weighted number of services provided and fees. T-ratios are calculated with robust standard errors.

of physicians' responsiveness.

3.4.4 Pooled Regression

The labor supply function for all physicians is estimated by pooling the time series data of days for all physicians specialties. The new dataset is a panel of physician specialties (denoted by j) by days (denoted by t). The following model is estimated with OLS:

$$\tilde{Q}_{jt} = \gamma_0 + \gamma_1 \tilde{P}_{jt} + \gamma_2 Physician\ Specialty + z_{jt} \tag{3.8}$$

The model includes dummy variables for each physician specialty to account for differences across specialties' provision of services. Table 3.5 shows that the fee parameter is positive and large in magnitude, i.e., physicians supply 0.79% more services as their fees increase by 1% percent. The parameter is statistically significant with a test statistic of 56.84, which indicates with its large magnitude that the parameter is estimated with precision. The parameters for the physician specialty dummy variables are large and statistically significant at the 95 percent confidence level. The large and statistically sig-

Table 3.5: Effect of Fees on the Number of Services Supplied: Specialties Pooled Together

Physician Specialty	Coeff.	t-ratio	Elasticity
Adjusted Price	0.47	56.84	0.79
Cardiology	.	.	.
Dermatology	40.86	88.61	0.95
Gastroenterology	-25.42	-426.59	-0.59
General Surgery	-90.97	-81.50	-2.11
General Practice	591.87	1394.34	13.72
Internal Medicine	15.63	39.27	0.36
Nephrology	9.16	36.04	0.21
Neurology	4.61	14.13	0.11
Obstetrics	-26.46	-259.73	-0.61
Orthopedics	-100.26	-76.87	-2.32
Urology	-51.77	-123.17	-1.20

NOTES:
A panel of physician specialty by days in the study period. The effect of fees on the number of services supplied is estimated with OLS. Elasticities are calculated using the mean of the average weighted number of services provided and fees. The results control for differences in the provision of services across specialties. T-ratios are calculated with robust standard errors.

nificant parameters for physician specialty dummy variables suggest that the weighting of fees and number of services provided does not account for differences across specialties.

This paper's results differ significantly from Thornton and Eakin (1997)'s estimate of a backward bending supply function, or Thornton (1998)'s finding of fee inelastic physicians. Both the studies' estimates are generated from data based on different institutional settings, such as an endogenous wage in Thornton and Eakin (1997) and Thornton (1998). The large elasticity estimated in this paper shows that physicians are more responsive to fee increases than the evidence suggested in some of the past literature. For example, the 0.79 elasticity is larger than Rizzo and Blumenthal (1994)'s elasticity of 0.49.

3.4.5 Simulating Physician Revenues

Physicians' response to changes in fees determines their revenue, and consequently, government expenditure on healthcare. The change in physician revenue can be simulated to determine physicians' response to three counterfactual fees. The three counterfactual fees are increasing and decreasing observed fees by 20%, and holding observed fees constant to fees in the first fixed fee period in 1997/98. The revenues are simulated by first, calculating the predicted values with Equation 3.9 using observed fees, $\tilde{P}^o_{jt}$. The predicted values represent historical average number of services supplied daily per type of service. Second, three different predicted values are calculated using the three different counterfactual fees, $\tilde{P}^c_{jt}$ instead of observed fees, $\tilde{P}^o_{jt}$ (as shown in Equation 3.10). The three different predicted values represent counterfactual average number of services supplied daily per type of service for the three different counterfactual fees. Last, all four predicted values are multiplied with its corresponding fee to calculate simulated physician revenues (as shown in Equation 3.11):

$$Historical\ Number\ of\ Services\ Supplied = \hat{\tilde{Q}}^o_{jt} = \hat{\delta}_{j0} + \hat{\delta}_{j1}\tilde{P}^o_{jt} \tag{3.9}$$

$$Counterfactual\ Number\ of\ Services\ Supplied = \hat{\tilde{Q}}^c_{jt} = \hat{\delta}_{j0} + \hat{\delta}_{j1}\tilde{P}^c_{jt} \tag{3.10}$$

$$Physician\ Revenue = \hat{\tilde{Q}}_{jt} \cdot \tilde{P}_{jt} = \left(\hat{\delta}_{j0} + \hat{\delta}_{j1}\tilde{P}_{jt}\right) \cdot \tilde{P}_{jt} \tag{3.11}$$

$\tilde{P}^o_{jt}$, $\tilde{P}^c_{jt}$, $\tilde{Q}^o_{jt}$, and $\tilde{Q}^c_{jt}$ are residuals from first stage regressions, so the variables are centered on zero. In order to calculate predicted values comparable to observed number of services supplied and observed fees, the mean of observed number of services supplied is added to $\tilde{Q}^o_{jt}$ and $\tilde{Q}^c_{jt}$, and the mean of observed fees is added to $\tilde{P}^o_{jt}$ and $\tilde{P}^c_{jt}$. Adding a mean to each variable does not affect the coefficients, elasticities, or statistical significance because the mean is a constant.

Table 3.6 compares physician specialties' observed revenue to historical revenue. Observed revenue is observed fees multiplied by observed number of services supplied daily for a service. The average historical revenue is identical to average observed revenue, where the average percentage difference is less than 1% for all specialties except for Neurology and Nephrology. Table 3.6 shows that the difference between the two revenues is that historical revenue varies over a smaller range than observed revenue. Nevertheless, the small difference between the two revenues suggest that simulated revenues are credible.

Table 3.7 shows physician specialties' daily average revenue for a service, given the three counterfactual fees. The 20% decrease in observed fees results in a decrease in revenue for all physician specialties, but the decrease in revenue ranges from -39.52% to -26.98% across the specialties. Nephrology's revenue decreases the most at -39.52% while General Surgery's revenue decreases the least at -26.98%. The results correspond closely to the specialties' elasticites, i.e., Nephrology has the largest while General Surgery has the smallest positive fee elasticity. The same correspondence between the decrease in revenues and fee elasticity is found for all other physician specialties. Further, the decrease in revenue is similar across specialties, where most specialties' revenue decreases approximately 34%. The similarity is due to most specialties' elasticities being approximately 0.9.

The same correspondence between elasticity and change in revenue is found when observed fees are increased 20%. Nephrology's revenue increases the most at 49.28% while General Surgey's revenue increases the least at 30.47%. Most physician specialties' revenue increases by approximately 41%, which is almost 10% larger response than a decrease in the fee schedule. Physicians are more responsive to increasing than decreasing the fee schedule, so increasing fees increases total government expenditure substantially. Although the largest proportion of total government expenditure is spent on General Practice, which has one of the smallest elasticities at 0.64, the elasticity still produces

Table 3.6: Physician Specialties' Daily Average Observed Revenue versus Historical Revenue for a Service

Physician Specialty	Observed ($)	Range ($)	Historical ($)	Range ($)	Diff. (%)
Neurology	1061.94	(418.14 - 1902.30)	1061.94	(902.94 - 1452.07)	1.46
Internal Medicine	1213.42	(1151.83 - 1295.41)	1213.42	(1189.90 - 1245.87)	0.01
Urology	705.56	(679.83 - 726.66)	705.56	(702.58 - 710.47)	0.01
Dermatology	1850.41	(1595.93 - 2087.92)	1850.41	(1809.96 - 1914.88)	0.06
Gastroenterology	889.36	(710.29 - 1053.70)	889.36	(859.89 - 931.35)	0.15
General Surgery	1224.34	(1212.73 - 1234.39)	1224.34	(1223.28 - 1226.53)	0.00
Orthopedics	1699.20	(1674.39 - 1720.82)	1699.20	(1695.25 - 1705.58)	0.00
Nephrology	1793.71	(632.90 - 2835.04)	1793.71	(1329.15 - 2301.08)	2.37
Obstetrics/Gynaecology	1194.99	(1176.12 - 1212.10)	1194.99	(1192.73 - 1197.87)	0.00
Cardiology	3085.73	(2845.86 - 3318.01)	3085.73	(3070.37 - 3111.92)	0.02
General Practice	25912.45	(24849.86 - 26747.09)	25912.45	(25586.47 - 26269.66)	0.00

NOTES:
Observed = Observed revenue from observed fees
Historical = Simulated revenue from observed fee
Diff. = The average percent difference in historical revenue from observed revenue.

36.27% increase in their revenue. The result is a significant increase in total government expenditure. Further, the elasticity is large relative to the elasticities found in past studies, e.g., Baltagi et al. (2005) estimate a 0.30 elasticity for General Practice physicians. Thus, policy makers can use the fee schedule to encourage physicians to meet increasing patient demands, but should expect a significant increase in total government expenditure.

The revenue when the observed fee schedule is held constant decreases for all physician specialties. General Practice's revenue declines the most at -22.81% while Urology's revenue declines the least at -7.63%. General Practice's revenue declines the most from historical revenue because historical revenue reflects large increases in observed fees over the study period, while the opposite occurred for Urology. The results, nevertheless, imply that if policy makers had never increased the fee schedule, most physicians would have supplied approximately 10% fewer services. The decrease in revenue from policy makers' decreasing or fixing the fee schedule also implies that physicians won't offset the

Table 3.7: Physician Specialties' Daily Average Revenue for a Service with Three Different Fee Schedules

Physician Specialty	Fees simulated at:						
	Historical ($)	Decrease ($)	Diff. (%)	Increase ($)	Diff. (%)	Constant ($)	Diff. (%)
Neurology	1061.94	689.82	-35.04	1513.91	42.56	888.67	-16.26
Internal Medicine	1213.42	778.41	-35.85	1744.59	43.77	1082.22	-10.81
Urology	705.56	472.39	-33.05	984.77	39.57	651.76	-7.63
Dermatology	1850.41	1191.05	-35.63	2654.40	43.45	1644.48	-11.13
Gastroenterology	889.36	594.04	-33.20	1243.39	39.81	798.44	-10.22
General Surgery	1224.34	894.00	-26.98	1597.41	30.47	1124.09	-8.19
Orthopedics	1699.20	1105.96	-34.91	2419.14	42.37	1475.31	-13.18
Nephrology	1793.71	1085.40	-39.52	2676.79	49.28	1540.35	-14.19
Obstetrics/Gynaecology	1194.99	770.56	-35.52	1712.13	43.28	1052.08	-11.96
Cardiology	3085.73	2189.44	-29.05	4121.61	33.57	2782.28	-9.83
General Practice	25912.45	17919.54	-30.85	35310.56	36.27	20002.78	-22.81

NOTES:
The above shows each specialties' simulated average daily physician revenue from a service, given three counterfactual fee schedules. The simulated revenue is equal to predicted average number of services supplied daily for a service multiplied by the fee schedule. For average number of services provided daily and all counterfactual fee schedules, the mean of observed fees and number of services provided daily is added $\tilde{P}^o_{jt}$ and $\tilde{P}^c_{jt}$, and $\tilde{Q}^o_{jt}$ and $\tilde{Q}^c_{jt}$, respectively
Historical = Simulated revenue from observed fee
Decrease = Simulated revenue from a 20% decrease in observed fees
Increase = Simulated revenue from a 20% increase in observed fees
Constant = Simulated revenue from observed fees held constant to fees in the first fee constant period in 1997/98
Diff. = The average percent difference in revenue from historical revenue.

decrease in revenues by supplying more services.

3.5 Conclusion

This paper takes advantage of exogenous variation in the fee schedule to identify physician supply responses. This paper also benefits from the comprehensiveness of the data, which represent all physicians and their services for 11 large physician specialties. Further, the empirical framework uses two stages of regression to account for medical and technological advances unique to each service. The estimated model shows that physicians supply significantly more services as their fees increase. This positive relationship is consistently shown across different physician specialties, but varies in the magnitude of response, i.e., General Surgery is shown to be the least responsive while Nephrology is the most responsive. The fee elasticity is positive and large in magnitude when physician specialties are pooled together.

This finding of a strong positive response to fees differs from some of the recent literature, which had showed physicians as unwilling to trade leisure for work, or unresponsive given their high incomes. This paper contributes to the literature on physician labor supply by showing that physicians' large positive response to fee changes is robust across many different specialties, services, and physicians in a large health region in Canada. Further, physicians for all specialties are found to supply more services by increasing their work intensity, but the immediacy of their response differs across specialties.

The policy implications are that policy makers can use the fee schedule to encourage physicians to supply more services when patient demand outpaces the supply of physician services. However, this is an awkward policy implication, since increases in fees result in a significant increase in total government expenditure. The government could perhaps address this by establishing a lower fee schedule after the first \$100,000 of billing.

Chapter 4

Revisiting the Healthy Immigrant Effect

4.1 Introduction

Many studies have observed that immigrants are initially healthier than native residents on arrival, but find that their health declines faster than native residents over time[11;19;29;35;37;52;54;55;59;65;75]. The stylized fact is known as the Healthy Immigrant Effect (HIE). The studies have documented the HIE in different countries and sub-populations using a wide range of health outcomes (as shown in Table 4.1). The decline of immigrants' health advantage is a key policy concern because it implies additional costs in healthcare and workforce productivity. The cost is significant when immigrants represent a large proportion of the population, e.g., immigrants represent 18.4% of Canada's population[25]. Further, the decline of immigrants' health advantage may be due to barriers in the health care system, labor market and education system[29]. This paper takes advantage of longitudinal data to estimate the association between immigrant status and individual rate changes in health. A first-differenced model with fixed effects estimates the individual rate changes in health, which controls for the effect of time-variant and time-invariant observable characteristics.

Past studies using cross-sectional data control for time-invariant observable differences between immigrants and native residents[11;19;29;35;37;52;55;59;65;75]. The studies compare long-term and recent immigrants' health to show the disappearance of immigrants' health advantage. The evidence, however, may be confounded by a cohort effect. Past immigrants may have faced less stringent immigration policies regarding their health than recently entering immigrants. For example, Canada's immigration policy was liberalized in 1962, which encouraged immigration from non-European countries. Deri (2004) attempts to control for cohort effects by pooling several cross-sectional datasets from the National Population Health Survey (NPHS) together. Cohort effects are captured because recent immigrants from past cross-sectional datasets may have a common time of entry with long-term immigrants from current cross-sectional datasets. Nevertheless, Deri (2004) still finds immigrants to have a 9.2% greater likelihood of reporting an activity limitation after living in Canada for ten years than native-born Canadians[15].

Many American, German, Australian, and Canadian studies using pooled cross-sectional data support Deri's (2004) finding[9;28;33;36;39;53;54;70]. Antecol and Bedard (2006) finds that while immigrant women are 10% less likely to be overweight than natives at entry, 90% of this health advantage disappears within 10 years of living in the United States (US)[33]. Frisbie et al. (2001) finds Asian and Pacific Islander immigrants whose duration of residence was less than 5 years, 5-9 years, and 10 years or more have 0.45, 0.65, and 0.73 greater odds of activity limitation than native US residents[74]. The authors explain the decline can be attributed to immigrants' inferior access to medical care relative to native residents.

All the studies using pooled cross-sectional data still assume that the main difference between recent and long-term immigrants is the time of entry. First, immigrants differ in many observable characteristics such as their diet, ethnicity, and religion. This paper finds, for example, that recent immigrants to Canada have more education, and earn less income than long-term immigrants (as shown in Table 4.2). Second, immigrants differ in their unobservable characteristics, such as their motivation, ability, ambition, and discount rate. Differences in observable and unobservable characteristics may explain the disappearance of immigrants' initial health advantage over time. Thus, comparisons between recent and long-term immigrants in pooled-cross sectional studies may still be confounded by differences in characteristics.

Table 4.1: Selected Studies on Healthy Immigrant Effect

Selected Studies on HIE	Dataset	Sub-Population	Outcomes
Gee, Kobayashi, and Prus (2003)	CCHS, 2000-01, Cross Section	Canada, Middle-age and Elderly	Self-rated Health
Deri (2004)	NPHS, 1994-98, Pooled Cross sectional	Canada	Self-rated Health, Activity Limitation BMI, Presence of Chronic Condition
McDonald and Kennedy (2004)	CCHS and NPHS, Pooled Cross sectional	Canada	Presence of Chronic Conditions, Self Assessed Health
Antecol and Bedard (2006)	US National Health Interview Surveys, 1989-1996 Pooled Cross sectional	United States	BMI, Health Status
Lechner and Mielck (1998)	German Socio-Economic Panel, 1984, 1988, 1992	Germany	Physical inactivity due to illness, chronic illness, and disablement
Palloni and Arias (2004)	National Health Interview Survey 1985-1994	United States, Hispanics	Mortality
Frisbie, Choy, and Hummer (2001)	National Health Interview Survey 1992-95	United States, Asian and Pacific Island Adults	Self-reported health, Activity limitation Status, Number of days in bed due to illness
Chiswick (2006)	Longitudinal Survey of Immigrants to Australia, 1993-95	Australia	Health Status
Ng (2004)	NPHS, 1994-02 Longitudinal	Canada	Self-perceived health, health care use and health related behaviors
Lou and Beaujot (2006)	CCHS 2002, 1.2	Canada	Mental Health
Kennedy, McDonald, and Biddle (2006)	National Health Interview Survey: 2000-05, National Population Health Survey: 1996-97, Canadian Community Health Survey, 2002-03, National Health Survey: 1995 and 2001	US Canada, Canada, Australia	Health status, and Health Service Use
Newbold (2005)	NPHS, 1994-2000, Longitudinal	Canada	Self-perceived health and Health Care Utilization

Longitudinal data follows the same immigrant over time and consequently measures the change in health for the same person. Current studies using longitudinal data, however, still find evidence of immigrants' health declining significantly[10;14;25]. Ng et al. use the longitudinal National Population Health Survey (NPHS) and find recent non-European immigrants twice as likely as Canadian-born residents to subsequently report fair/poor-health[25]. Ng et al. uses survival analysis to estimate the risk from transitioning from good to poor health status. Chiswick (2006) uses both fixed and random effects and still finds a decline in immigrants' likelihood of reporting good health. The results are, however, limited because the dataset consists of only 2 survey waves and includes responses from only immigrants, preventing any comparison to native residents.

One difficulty with using longitudinal survey data is missing responses in subsequent surveys. If immigrants' health differs from native residents' health among those who dropped out of the survey, survey attrition may confound the results. Immigrants tend to drop out more often than native residents because immigrants move more often within Canada typically because they tend to have fewer attachments to family and friends, and tend to be more willing to improve their education and occupational opportunities[57]. Moreover, immigrants have a higher attrition rate than native residents because they tend to return to their home country[9;14;57]. For example, Sanders (2007) finds that unhealthy male immigrants are less likely to return to their home country because of the host country's health care system. The attrition rate in this paper is consistent with the literature. Table 4.2 shows that 59.60% of recent immigrants and 40.18% of long term immigrants leave the survey, while only 32.63% of native residents leave the survey.

This paper contributes three advances to the literature. First, the paper differences respondents' health to estimate a change in health for each respondent. Differencing respondents' health departs from past studies using longitudinal data[10;25]. The studies tend to show a change in health only if respondents transition from good to poor health

Table 4.2: Descriptive Statistics based on Responses in 1994/95

Variables		Recent n	Recent %	Long-Term n	Long-Term %	Native Residents n	Native Residents %
Overall		251	2.50	1120	11.15	8678	86.36
Sex	Male	122	48.61	541	48.30	4138	47.68
	Female	129	51.39	579	51.70	4540	52.32
Age	Less than 39	174	69.32	253	22.59	3746	43.17
	40 - 59	57	22.71	459	40.98	2908	33.51
	Greater than 60	20	7.97	408	36.43	2024	23.32
Household Size	1	38	15.14	284	25.36	1896	21.85
	2	49	19.52	381	34.02	2905	33.48
	3	57	22.71	189	16.88	1632	18.81
	4	60	23.90	179	15.98	1524	17.56
	5+	47	18.73	87	7.77	721	8.31
Marital Status	Married/ Common Law/ Partner	162	64.54	697	62.23	5162	59.48
	Widowed/Separated/Divorced	25	9.96	283	25.27	1666	19.20
	Single/ Never Married	64	25.50	140	12.50	1850	21.32
Education	Less than Secondary	17	6.77	124	11.07	612	7.05
	Secondary	69	27.49	314	28.04	3284	37.84
	Trade School	36	14.34	244	21.79	1817	20.94
	University	129	51.39	438	39.11	2965	34.17
Income	Missing	11	4.38	55	4.91	321	3.70
	Less than $30,000	125	49.80	452	40.36	3532	40.70
	$30,000-$60,000	88	35.06	353	31.52	3201	36.89
	Greater than $60,000	27	10.76	260	23.21	1624	18.71

Attrition Rate	1994 n	1996 n	1996 %	1998 n	1998 %	2000 n	2000 %	2002 n	2002 %	2004 n	2004 %
Native Resident	8678	8028	-7.49	7525	-13.29	6964	-19.75	6432	-25.88	5849	-32.60
Long-term Immigrant	1120	994	-11.25	902	-19.46	813	-27.41	749	-33.13	670	-40.18
Recent Immigrant	251	209	-16.73	184	-26.69	165	-34.26	149	-40.64	124	-50.60

NOTES:
Provincial residence is not included in the table because cell sizes are less than 5
Recent immigrants represent respondents living in Canada fewer than 10 years
Long-term immigrants represent respondents living in Canada 10 years or greater

status over the study period. Second, the empirical framework controls for the effect of time-variant observable characteristics, which is a significant advantage over past cross-sectional studies. Third, this paper corrects for survey attrition bias using Olsen's (1980) sample selection model.

The estimates show that recent immigrants' increase in chronic conditions and BMI is insignificantly different from native residents, but their increase in perceived poor health is higher than native residents. The relatively steep increase in immigrants' perceived poor health may have resulted in a relatively steep increase in the number of visits to family physicians. Controlling for survey attrition has little or no effect on the estimates. The effect of using longitudinal data and controlling for observable characteristics is a reversal of previous findings: in contrast to the literature, this paper does not find a deterioration of immigrants' health advantage over time.

4.2 Empirical Framework

Cross-sectional studies compare the average level of health between recent immigrants to observationally identical long-term immigrants, in order to determine the deterioration of immigrants' health advantage[9;28;33;36;39;53;54;59;70]. These studies typically estimate the relationship by regressing a health outcome onto immigrant characteristics, such as immigrant status, years since immigration, and years since immigration-squared. Observable characteristics are held constant in the regression, such as age, age-squared, sex, provincial residence, income, education, marital status, household size, and time-trend are held constant. The following equation is estimated as the baseline model, which is used to illustrate the difference between cross-sectional and longitudinal model estimates.

$$H_i = \gamma_1 YSM_i + \gamma_2 YSM_i^2 + \gamma_3 Immigrant_i + X_i\beta + u_i \tag{4.1}$$

An immigrant dummy controls for the difference in average level of health between immigrants and native residents. The parameter, γ_3 shows the effect of being an immigrant on the average level of health. The variables years since immigration, YSM_i and years since immigration-squared, YSM_i^2 control for the mean level and non-linear difference in health across the immigrant population. The sign and magnitude of γ_1 and γ_2 indicate whether duration in the host-country is correlated with a deterioration in immigrants' health. The parameter γ_1 represents the difference in health between long-term immigrants and observationally identical recent immigrants.

Equation 4.1 does not, however, address the non-random assignment of immigrant status. Immigrant status is non-randomly assigned both because respondents choose to become an immigrant and because the host country chooses immigrants based on their observable characteristics. The non-random assignment results in differences in health between immigrants and native residents on arrival. The causal effect of immigrant status on changes in health cannot be identified if differences in characteristics between native residents and immigrants determine changes in health. For example, if immigrants discount their health at a higher rate than native residents, they will be more willing to work at physically demanding occupations in the present time and to risk their future health than native residents. The discount rate confounds the effect of immigrant status on changes in health because immigrants would have had the same decline in their health if they remained in their home country and had the same work preferences.

Given the lack of policy experiments randomly assigning immigrant status, this paper takes advantage of longitudinal data to estimate the relationship between immigrant status and change in health over time. A fixed effect model can be estimated for the longitudinal data, as shown in Equation 4.2. The i denotes each respondent and t denotes each survey year. The difference between Equation 4.1 and Equation 4.2 is each respondent is observed repeatedly over several survey years, which tracks each respondents' health over time. On the right hand side, a fixed effect, c_i is estimated for each respondent, which represents all time invariants variables affecting each respondent's level of health. The time invariant variables represent all unobservable characteristics, such as work preferences and observable characteristics, such as age and sex. The fixed effect also accounts for the effect of covariates of interest, immigrants status and years since immigration on the level of health. Time-varying observable characteristics are accounted with X_{it}

$$H_{it} = c_i + X_{it}\delta + v_{it} \tag{4.2}$$

Equation 4.2, however, does not account for time varying unobservables affecting the average level of health over time. One approach to controlling for time-varying unobservables is taking the first difference, which is shown in Equation 4.3. The first difference calculates each respondent's change in health from past to current time period, as well as each respondent's change in the observable time-varying characteristics, $(X_{it} - X_{it-1})$.

$$H_{it} - H_{it-1} = \beta i + (X_{it} - X_{it-1})\phi + u_{it} - u_{it-1} \tag{4.3}$$

Since the dependent variables represents each respondent's change in health, the introduction of a fixed effect, β_i in Equation 4.3 controls for all time-varying unobservable covariates affecting each respondent's average change in health, such as changes in respondent's diet, physical activity, and/or smoking status[65]. Further, the fixed effect controls for all time-invariant covariates affecting each respondent's average change in health, e.g., sex, ethnicity, religion, culture, discount rate, and genetics. The change in time-varying observables affecting changes in health, such as income, household size, and provincial residence are controlled with $X_{it} - X_{it-1}$. Second, the advantage of taking first differences is that the change in health is calculated for the same respondent. In contrast, cross-sectional studies determine immigrants' change in health by calculating the difference in mean level of health between recent immigrants and observationally identical long-term immigrants.

The disadvantage of equation 4.3 is it cannot include the covariate of interest, immigrant status and all other time invariant covariates because the covariates are perfectly collinear with respondents' fixed effects. To recover the association of immigrant status and change in health, Equation 4.3 is estimated as a first stage regression. Next, the $\hat{\beta}_i$ estimated in the first stage regression is regressed onto immigrant status and all other time-invariant covariates, as shown in Equation 4.4. Time-invariant covariates, X_i consists of age, sex, and the 1994/95 survey year levels of provincial residence, income, education, marital status, and household size. The $\hat{\beta}_i$ represents each respondent's average rate of change in health relative to all other respondents' average rate of change in health. The change in time-varying observable characteristics, $(X_{it}-X_{it-1})$ affecting each respondent's average rate of change in health is accounted for in $\hat{\beta}_i$ because it is purged in the first stage regression. The parameter for immigrant status, $\hat{\alpha_3}$ is interpreted as the association of being an immigrant on average rate of change in health relative to being a native resident, while time-variant and time-invariant observable characteristics are held constant.

$$\hat{\beta}_i = \alpha_1 YSM_i + \alpha_2 YSM_i^2 + \alpha_3 Immigrant_i + X_i\theta + z_i \tag{4.4}$$

The benefit of estimating α_3 in two stages is it holds the effect of time-varying observable characteristics constant. For example, an alternative approach to estimating α_3 in two stages is to estimate Equation 4.3 without fixed effects but with immigrant status and all other time-invariant covariates. That is, the specification includes time invariant covariates that are not first-differenced. The parameter for immigrant status in this alternative approach can be compared to α_3 in Equation 4.4, which is shown in the Results and Discussion section below. This alternative approach, however, does not control for time invariant unobserved characteristics, and consequently, the parameters for time-varying observables can be confounded. If the parameters for the time-varying observable are confounded, then the parameter for immigrants status is also confounded. In contrast, estimates for time-varying observables in Equation 4.3 holds unobserved characteristics constant with β_i. Thus, the estimate for immigrant status in Equation 4.4 controls for the effect of time-varying observable characteristics.

The use of longitudinal data in this approach introduces the problem of survey attrition. The health of those dropping out in subsequent survey waves may differ from those continuing to respond. Particularly if the average health of immigrants who dropped out in subsequent survey waves differs from that of native residents who dropped out, then estimates of the effect of immigrant status are likely biased. An approach to correct for attrition bias is Heckman's sample selection model[41]. First, a separate selection equation is estimated for the likelihood of dropping out with the Probit model (as shown in Equation 4.5)[30;62]. Second, the outcome equation is estimated with the Inverse Mills ratio. The Inverse Mills ratio is a ratio between the density and cumulative distribution function of the selection equation's predicted values, $\frac{f(Z\hat{\delta})}{F(-Z\hat{\delta})}$[51]. A t-test for λ, i.e., $H : \lambda = 0$ tests for attrition bias.

However, Heckman's correction cannot incorporate fixed effects in the selection equation because fixed effects cannot be conditioned out of the Probit's likelihood function. Olsen's correction provides an alternative approach, which solves the difficulty with the fixed effects by estimating the selection equation with the Linear Probability Model. Olsen's correction also replaces the Inverse Mills ratio in the outcome equation with the selection equation's predicted values less one, i.e, $z\hat{\delta}$-1 (as shown in Equation 4.6). Olsen (1980) shows that the difference in estimates between the two corrections is small, so Olsen's correction is followed in this paper.

$$s_{it} - s_{it-1} = 1[\beta_i + (Z_{it} - Z_{it-1})\delta + v_{it} - v_{it-1} \geq 0] \tag{4.5}$$

$$H_{it} - H_{it-1} = \beta_i + (X_{it} - X_{it-1})\phi + \lambda(s_{it} \hat{-} s_{it-1}) - 1 + u_{it} - u_{it-1} \tag{4.6}$$

Olsen's (1980) correction requires an exclusion restriction to obtain convincing estimates. Estimating without an exclusion restriction makes it difficult to distinguish sample selection from misspecified functional form. A valid exclusion restriction must be significantly correlated with dropping out in subsequent survey waves but not correlated with the health outcome. This paper's exclusion restrictions are dummy variables indicating the survey quarter in which the respondent was interviewed in the survey year. The exclusion restrictions are valid because survey quarters in 1994/95 are chosen by survey administrators rather than by respondents, so are uncorrelated with respondents' health[16].

Table 4.3 shows that respondents initially interviewed in the last three quarters of the 1994/95 survey year are more likely to drop out than respondents initially interviewed in the first two quarters. For example, 32.86% of respondents interviewed in June 1994 quarter dropped out, while 47.52% of respondents interviewed in June 1995 quarter dropped out. Most respondents are interviewed in the same survey quarter in all survey waves, so the same amount of time occurs between survey waves. However, some respondents initially interviewed in the first two quarters in 1994/95 are interviewed in last three quarters of subsequent survey waves. The bottom of Table 4.3 shows that a small percentage of respondents initially interviewed in the first 2 quarters are interviewed in the last 3 quarters for subsequent survey waves, while no respondents initially interviewed in last 3 quarters are interviewed in the first 2 quarters of subsequent survey waves. Respondents initially interviewed in the first 2 survey quarters have more time in the survey year to be interviewed by interviewers than respondents initially interviewed in the last three quarters, so are less likely to drop out of the survey[18]. The exclusion

Table 4.3: Exclusion Restriction

Quarter	Remained	Dropped Out	%
June 1994	1763	864	32.89
August 1994	1908	879	31.54
November 1994	1530	831	35.20
March 1995	1318	717	35.23
June 1995	124	115	18.12

Quarter	1996			1998			2000			2002			2004		
	Not	1st 2 QTRS	%	Not	1st 2 QTRS	%	Not	1st 2 QTRS	%	Not	1st 2 QTRS	%	Not	1st 2 QTRS	%
June 1994	0	2506	100.00	0	2352	100.00	0	1998	100.00	0	1849	100.00	0	1648	100.00
August 1994	0	2658	100.00	0	2479	100.00	0	2135	100.00	0	2015	100.00	0	1825	100.00
November 1994	2266	109	4.59	2107	87	3.97	1770	102	5.41	1711	117	6.40	1563	244	13.50
March 1995	2164	31	1.40	2939	113	5.27	1654	114	6.45	1549	112	6.74	1473	224	13.20
June 1995	205	110	34.92	198	383	43.47	1211	1065	16.79	1375	1321	49.00	1599	1473	47.95

NOTES:
1st 2 QTRS = the respondents interviewed in the first 2 quarters of the 1994/95 survey year

restrictions are considered strong if the f-test on the exclusion restrictions is greater than 10[38].

4.3 Data Description

This paper uses Statistics Canada's National Population Health Survey (NPHS) from 1994/95 to 2004/05. The NPHS biennially collects socio-demographic and health information from the same respondents for ten years. A member of each selected household is randomly chosen. All households surveyed are composed of private households and institutional residents in all provinces except residents of Indian reserves, Canadian Forces bases, and some remote area. In 1994/95, the initial data consists of 17,276 respondents.

The main advantage of the survey data is the rich set of characteristics tracked over time, such as education, income, marital status, household size, age, sex, provincial residence, and years since immigration. Immigrant status is segmented into three categories. Using 1994 as this paper's reference year, recent immigrants are all immigrants entering Canada between 1985-1994, while long-term immigrants are all immigrants entering Canada prior to 1985. All other respondents are defined as native residents. There are 8,678 native residents, 1,120 long-term immigrants, and 251 recent immigrants. Respondents with missing responses for the covariates defined in Equation 4.1 for any of the survey waves are dropped from the dataset, which reduces the dataset to 10,049 respondents. The covariates for respondents who dropped out of the survey are also missing responses. In order to estimate the probability of dropping out in Olsen's sample selection model, the covariates' missing values are imputed values from responses in their last survey wave[18].

This paper evaluates four health outcomes, body mass index, perceived poor health, number of visits to a family physician, and number of chronic conditions. Body mass index is a ratio between respondents' self-reported height and weight. Perceived poor health is self-reported health status with ratings ranging from excellent to poor health status. The number of chronic conditions represents the following conditions: heart disease, diabetes, kidney disease, HIV, high blood pressure, cancer, intestinal and stomach ulcers, and dementia, and zero otherwise.

Respondents dropping out of subsequent survey waves are respondents missing responses sequentially over time. All respondents missing responses for an outcome in a survey wave year who eventually drop out in future survey waves are excluded from the dataset. These exclusion criteria drop 582 respondents from the dataset. The exclusion criteria also excludes all respondents who reported being Canadian-born but originating from a different country, i.e., 45 respondents are excluded. Further, the first difference model drops all respondents who dropped out after the first survey wave because a first-difference cannot be calculated for these respondents. The first difference model excludes 823 respondents.

4.4 Results and Discussion

Table 4.4 shows the average of the four health outcomes by survey year and immigrant status. The comparison between native residents and recent immigrants in the first survey year provides evidence of immigrants' initial health advantage. In the 1994/95 survey year, recent immigrants have a lower average number of chronic conditions at 0.58 (compared to 1.19 for native residents), lower perceived-poor health at 2.17 (compared to 2.29 for native residents), and lower body mass index at 23.82 (compared to 25.53 for native residents). Further, recent immigrants' initial health advantage is correlated with a lower average number of visits to their family physician at 3.92 (compared to 4.67 for native residents).

Table 4.4 also shows that all groups of respondents have an increase in the number of chronic conditions, perceived poor health, and BMI over the ten-year study period. The opposite relationship is found for the change in number of visits to family physician for all respondents. The difference in health outcomes between immigrants and native residents varies over the study period. First, recent immigrants' initial 0.75 fewer visits to a family physician increases to 1.02 fewer visits by 2004/05. Second, recent immigrants' initial advantage of having a 0.12 lower perceived poor health becomes a disadvantage by 2004/05, increasing to 0.02 above native residents' perceived poor health. Further, recent immigrants' initial advantage of having a BMI 1.71 lower than native residents decreases to 1.28 by 2004/05. Figure 4.1 shows the average change in health outcomes by survey year and immigrant status. The figure shows that immigrants' perceived poor health and BMI increases significantly relative to native residents. The steeper deterioration in immigrant health relative to native residents is not clearly shown for the number of family physician visits and number of chronic conditions.

One concern is that the deterioration of immigrants' health advantage may be ex-

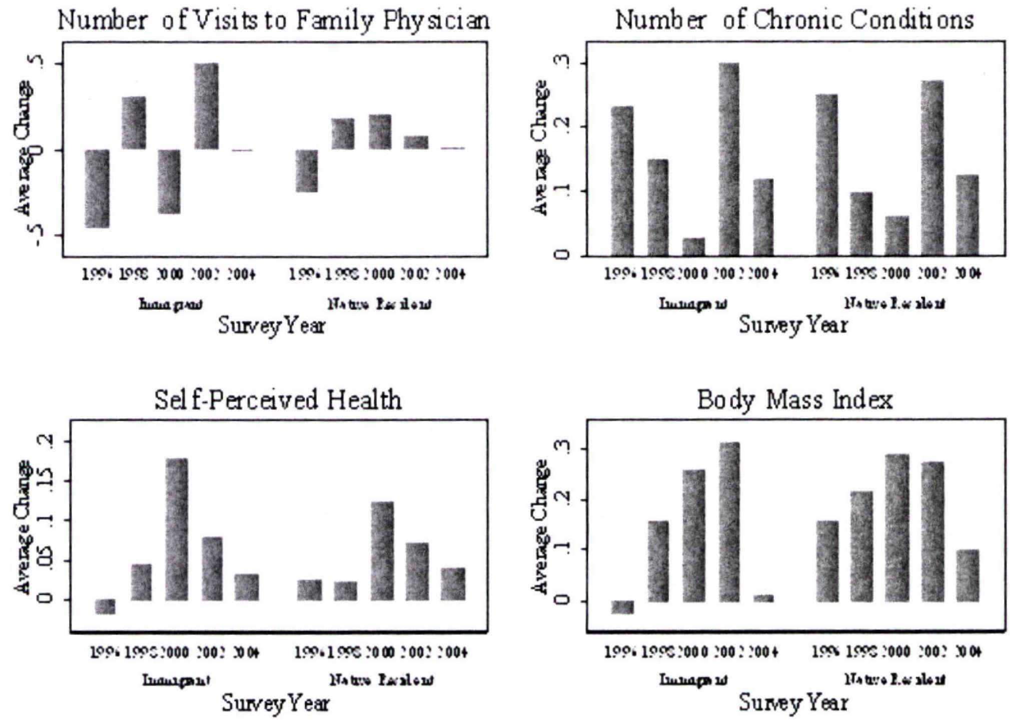
Table 4.4: Average Health and Health Service Outcome by Survey Year

Outcome	Immigrant Status	1994 Mean	1996 Mean	1998 Mean	2000 Mean	2002 Mean	2004 Mean
Number of Visits to Family Doctor	Native Resident	4.67	4.34	4.42	4.45	4.47	4.41
	Long-term Immigrant	5.63	4.68	4.92	4.31	4.56	4.47
	Recent Immigrant	3.92	4.55	3.74	3.79	4.21	3.39
Number of Chronic Conditions	Native Resident	1.19	1.42	1.49	1.51	1.77	1.89
	Long-term Immigrant	1.34	1.54	1.64	1.59	1.85	1.93
	Recent Immigrant	0.58	0.82	0.94	0.98	1.15	1.25
Self-Perceived Health	Native Resident	2.29	2.28	2.27	2.36	2.41	2.42
	Long-term Immigrant	2.44	2.36	2.36	2.49	2.54	2.53
	Recent Immigrant	2.17	2.20	2.22	2.41	2.47	2.44
Body Mass Index*	Native Resident	25.53	25.76	26.03	26.37	26.69	26.86
	Long-term Immigrant	25.28	25.26	25.51	25.75	26.11	26.27
	Recent Immigrant	23.82	24.24	24.53	24.92	25.47	25.84

NOTES:
Perceived Poor Health is measured with a scale from 1-5, which represents excellent to poor health
Recent immigrants represent respondents living in Canada fewer than 10 years
Long-term immigrants represent respondents living in Canada 10 years or greater

plained simply by the significant proportion of respondents dropping out, i.e, 33.89% of all respondents. Table 4.5 compares health outcomes in 1994/95 survey between those who dropped out and remained in the survey. For all outcomes except for BMI, the respondents who dropped out have significantly worse average health outcomes than those who remained. The percent difference in average health outcomes is smaller for recent immigrants' number of chronic conditions and perceived poor health than the same outcomes for native residents or long-term immigrants. Particularly, recent immigrants who dropped out have 1.76% and 4.69% more chronic conditions and perceived poor health than immigrants who remained, while the percent difference for native residents for the same outcomes are 28.66% and 17.02%. The percent difference is the opposite for BMI. The percent difference in BMI is larger for recent immigrants than either native residents and long-term immigrants. The difference in average health outcomes by immigrant status and drop-out status suggests survey attrition biases estimates. The following cor-

Figure 4.1: Average Change in Health Outcome by Survey Year and Immigrant Status



Note: Self-perceived Health is measured on a scale from 1-5, representing excellent to poor health

rects the first difference model with fixed effects for survey attrition using Olsen's (1980) sample selection model.

4.4.1 Estimates from the Literature

Equation 4.1 is estimated to provide a comparison to selected cross-sectional studies, i.e., Macdonald and Kennedy (2004), Antecol and Bedard (2006), and Deri (2004). Equation 4.1 uses only a cross-section of this study's longitudinal data to estimate a baseline model, which is then used to compare to the selected studies and Equations 4.4 and 4.6. A cross-section of the longitudinal data is constructed by using responses from only the 1994/95 survey year. All health outcomes are dichotomized into binary variables,

Table 4.5: Average Health Outcomes by Dropout Status

Outcome	Immigrant Status	Dropped Out n	Dropped Out mean	Remained n	Remained mean	Percent Difference
Number of Visits to Family Doctor	Native Resident	2829	5.58	5849	4.23	31.95
	Long-term Immigrant	450	6.85	670	4.81	42.51
	Recent Immigrant	127	4.46	124	3.35	33.08
Number of Chronic Conditions	Native Resident	2829	1.40	5849	1.09	28.66
	Long-term Immigrant	450	1.65	670	1.13	45.17
	Recent Immigrant	127	0.58	124	0.57	1.76
Self-Perceived Health	Native Resident	2829	2.54	5849	2.17	17.02
	Long-term Immigrant	450	2.66	670	2.30	15.88
	Recent Immigrant	127	2.22	124	2.12	4.69
Body Mass Index	Native Resident	2829	25.28	5849	25.66	-1.50
	Long-term Immigrant	450	25.03	670	25.44	-1.61
	Recent Immigrant	127	23.44	124	24.22	-3.23

NOTES:
Perceived Poor Health is measured with a scale from 1-5 representing excellent to poor health
Recent immigrants represent respondents living in Canada fewer than 10 years
Long-term immigrants represent respondents living in Canada 10 years or greater

so they can be compared to the outcomes in the selected studies. The binary variables are self-explanatory: *have a chronic condition* equals one if respondents have at least one chronic condition, *poor health* equals one if respondents report fair or poor health status, and *overweight* equals one if respondents have a BMI greater or equal to 25. All parameters are estimated with the Probit model and reported in marginal effects, which are the same estimation methods used by MacDonald (2004) and Deri (2004).

The baseline estimates shown in Table 4.6 shows that the probability of having a chronic condition increases by 0.006 as years since immigration increases by a year. The probability of reporting poor health and being overweight also increases by 0.002 and 0.009 as years since immigration increases by a year. The direction of the baseline estimates match the selected studies. All estimates from MacDonald and Kennedy (2004), Antecol (2006), and Deri (2004) show increasing likelihood of the three health outcomes being worse as years since immigration increases. Figure 4.2 also shows a deterioration in these outcomes, and self-perceived health, as years since immigration increases. Only immigrants who have been in Canada for more than 60 years do not show an increasing BMI as years since immigration increases.

The magnitude of the baseline estimates are smaller than the estimates from the selected studies. For example, as years since immigration increases a year, Macdonald and Kennedy (2004) estimates a 0.006% increase in the likelihood of reporting poor health, while the baseline model shows a 0.003% increase. The difference is likely due to MacDonald and Kennedy's inclusion of observations with missing responses for covariates in subsequent survey waves. The difference between the baseline estimates and Deri (2004)'s estimates is the inclusion of cohort and year effect in Deri (2004)'s estimates*. Nevertheless, when using the cross-sectional approach employed in the literature, this paper replicates the finding that immigrants' health advantage deteriorates with duration in the host country.

4.4.2 Estimating Individual Rate Changes in Health

Table 4.7 compares estimates from a first-difference model without fixed effects to Equation 4.4. The first-difference model without fixed effects includes time-invariant characteristics, i.e., age, age-squared, sex, immigrant dummy, time trend, years since immigration, and years since immigration squared (which are not first-differenced). The time invariant covariates can be included in this model because there are no fixed effects perfectly collinear with these covariates. Model 1 in Table 4.7 shows the results of the

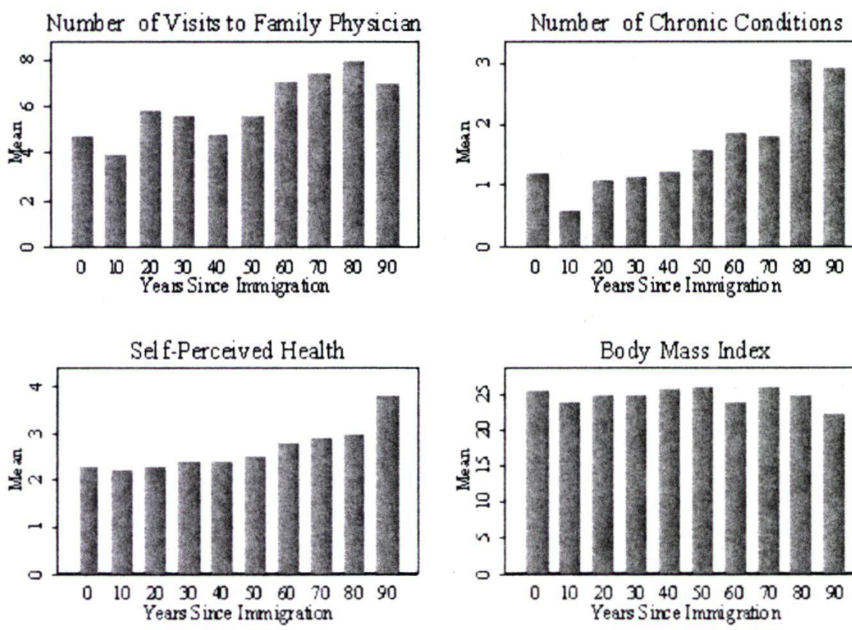
*Cohort effects could not be included in the baseline estimates because there is only one reference year, 1994/94 in the longitudinal data to define the number of years since immigration. Pooled cross-sectional studies have several reference years because there are several cross sectional datasets pooled together. Cohort effects represent immigrants with the same time of arrival but participated in different cross sectional datasets. For example, immigrants entering Canada in 1989 is defined to have lived in Canada for 5 years if they participated in the 1994/95 survey wave, but defined to have lived in Canada for 9 years if participated in the the 1998/99 survey wave. Without immigrants with the same time arrival but different reference years for years since immigration, the longitudinal data cannot account for cohort effects.

Table 4.6: The Association Between Years Since Immigration and the Likelihood of a Poor Health Outcome

Dependent Variables	Baseline		Macdonald et al. (2004)		Antecol and Bedard* (2006)		Deri (2004)	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Having a Chronic Condition	0.006	0.002	0.012	0.009	.	.	0.010	0.005
Poor Health	0.002	0.001	0.006	0.009	.	.	0.011	0.003
Overweight (BMI¿25)	0.009	0.002	.	.	0.038	0.022	0.100	0.054

NOTES:
All estimates are reported in marginal effects and estimated with the Probit Model
The cross-sectional data is constructed from the longitudinal data by excluding responses beyond the 1994/95 survey year
Having a chronic condition equals one for respondents with at least one chronic condition
Poor Health equals one for respondents reporting fair or poor health status
*Antecol and Bedard (2006) report the likelihood of being overweight for immigrants living in the host country for 10-14 years for all men

Figure 4.2: Average Health Outcome by Years Since Immigration



first-difference model without fixed effects. The change for the two objective measures of health, number of chronic conditions and BMI are 0.0106 and 0.0001 lower than native residents over time. Both the estimates are small in magnitude and statistically insignificantly at the 95% confidence level. The change in perceived poor health and number of visits to family physician is 0.0461 and 0.1065 higher than native residents over the same time period. Both the estimates are large relative to the other two outcomes, and is statistically significant for only the perceived poor health outcome.

The estimates for other immigrant characteristics, i.e., years since immigration, and years since immigration-squared purges the effect of immigrant status on changes in the health outcomes. Consequently, the estimates represent the effect of the covariates for immigrants only. Model 1 shows that as years since immigration increases a year, the change in perceived poor health, BMI, and number of visits decreases by 0.0035, 0.0026, and 0.0258, but the change in number of chronic conditions increases by 0.0013. All estimates are not statistically significant at the 95% confidence level except for the immigrant dummy for the perceived poor health outcome†. The insignificant estimates are likely due to the number of observations excluded for immigrants when the first-difference is taken. The estimates for years since immigration-squared are positive for all outcomes, so the change in all outcomes is occuring at an increasing rate. The increasing rate is, however, small in magnitude.

Model 2 shows the second stage estimates for Equation 4.4 after the first-difference model with fixed effects (first stage regression estimates are shown in the Appendix). Model 2 shows that Model 1 underestimates the immigrant status parameter for number of visits to family physician and number of chronic conditions. For example, Model 2 shows that immigrants' change in the number of visits to a family physician is 0.5919 higher than that of native residents, while Model 1 shows that immigrants' change in number of visits to a family physician is 0.1065 higher. Further, Model 1 overestimates

Table 4.7: The Association Between Immigrant Characteristics and Changes in Health Outcomes and Health Service Use Over Time

Dependent Variables	Covariates of Interest	Model(1)		Model(2)		Model(3)	
		Coefficient	t-test	Coefficient	t-test	Coefficient	t-test
Number of Visits to Family Doctor	Years Since Immigration	-0.0258	-0.77	-0.0868	-0.86	-0.0870	-0.86
	Years Since Immigration-Squared	0.0005	0.75	0.0017	0.88	0.0017	0.88
	Immigrant Dummy	0.1065	0.34	0.5919	0.67	0.5944	0.67
	Lambda	.	.	.	.	5.5095	1.33
Number of Chronic Conditions	Years Since Immigration	0.0013	0.63	0.0033	0.96	0.0033	0.96
	Years Since Immigration-Squared	0.0000	-1.10	-0.0001	-1.25	-0.0001	-1.25
	Immigrant Dummy	-0.0106	-0.40	-0.0261	-0.60	-0.0261	-0.60
	Lambda	.	.	.	.	-0.0159	-0.04
Perceived-Poor Health	Years Since Immigration	-0.0035	-2.27	-0.0035	-1.27	-0.0035	-1.27
	Years Since Immigration-Squared	0.0001	2.03	0.0000	1.05	0.0000	1.05
	Immigrant Dummy	0.0461	2.05	0.0387	0.98	0.0388	0.98
	Lambda	.	.	.	.	0.0915	0.31
Body Mass Index	Years Since Immigration	-0.0026	-0.57	-0.0019	-0.28	-0.0019	-0.28
	Years Since Immigration-Squared	0.0001	0.80	0.0000	0.19	0.0000	0.19
	Immigrant Dummy	-0.0001	0.00	0.0175	0.17	0.0178	0.17
	Lambda	.	.	.	.	0.5006	0.58

NOTES:
Model 1 represents the first differenced model without fixed effects. The covariates include age, sex, province, marital status, education, household size, income, time trend, years since immigration, years since immigration squared, and immigrant status.
Model 2 represents second stage estimates after the first stage regression. In the first stage, each respondents' health outcome is first-differenced and regressed onto fixed effects for each respondent and time-varying covariates in first differences, which are provincial residence, marital status, education, household size, and income. Second, the parameters for respondents' fixed effects are regressed onto age, age-squared sex, years since immigration, years since immigration squared, immigrant status, and 1994 levels of provincial residence, marital status, education, household size, and income. The number of observations in Model 1 and 2 are n=39,757 and n=9,231. Model 3 is the same as Model 2 except that it includes Lambda. Lambda represents the selection equation's predicted value less one. The selection equation estimates the probability of dropping out with covariates equal to all covariates in Model 2 plus survey quarters as exclusion restriction. The selection equation is estimated in first differences with OLS. The f-statistic on the exclusion restrictions is 143.70.

immigrants' increase in perceived poor health over time relative to native residents, and shows the opposite direction for immigrants' change in BMI relative to native residents. The parameter for years since immigration and years since immigration-squared for Model 2 differs slightly from Model 1. Only for the number of chronic conditions outcome, the parameter for years since immigration squared differ in direction between the two models.

The results show that the deterioration of immigrants' health advantage does not occur for the two objective measures of health. Immigrants' increase in number of chronic conditions and BMI over time differs insignificantly from native residents. Estimates from Model 2 are inconsistent with the baseline estimates and cross-sectional studies, which

†The robust standard errors are adjusted for clustering across respondents

show increasing likelihood of having a chronic condition, poor perceived health, and being overweight as immigrants live longer in Canada. Further, estimates from Model 2 are also inconsistent with other longitudinal studies. For example, Ng et al. (2005) use survival analysis to find that immigrants have a greater risk of transitioning to poor health, becoming inactive, and a 10% increase in BMI. The difference in results is likely related to the fact that Ng et al. (2005) do not control for time-varying observable differences across respondents. Thus, in contrast to the literature, immigrants' health advantage over native residents remains over time.

However, the finding that immigrants perceived a relatively rapid decline in their health over time, relative to native residents, is consistent with the literature. Newbold (2005) suggests that immigrants tend to re-evaluate their health each survey wave, and report lower levels of health in future surveys than native residents. Immigrants' poor perception of their health may have resulted in more visits to their primary care physician over time than native residents. Immigrants' increase in visits to their family physician over time may have helped to prevent chronic conditions and lowered their BMI relative to native residents, because of more rapid diagnosis and treatment. It is possible that policies promoting and maintaining immigrant health have helped immigrants keep their health advantage over native residents over time.

Equation 4.4 holds the effect of time variant observables constant by accounting for the effect of unobservables on time variant observables with fixed effects in Equation 4.3. Equation 4.4 does not, however, account for the effect of unobservables on immigrant status and all other covariates in Equation 4.4. Consequently, the interpretation from Equation 4.4's estimates are limited because it can be confounded by unobservables. For example, immigrants' modifications to their diets may prevent fewer chronic conditions and lower BMI than native residents. If immigrants remained in their home country, the same change in chronic conditions and BMI might have occurred given the same modifications in diet. Nevertheless, this paper contributes to the literature because past studies do not estimate individual rate changes in health, or control for the effect of time-variant observables. This paper's approach reduces the number of confounders that potentially explains the relationship between immigrant status and change in health over time.

4.4.3 Controlling for Survey Attrition Bias

Model 3 corrects Model 2 for survey attrition bias. The results show little or no difference in the magnitude of the parameters or statistical significance from correcting for attrition bias, as shown in Table 4.7. (First stage regression estimates for Model 2 and 3 are shown in the Appendix). Only the parameters for the outcome, number of visits to family physician are slightly larger in Model 3 than in Model 2. Model 2 underestimates immigrants' increase in visits to family physicians relative to native residents. The parameters for the likelihood of remaining in the survey, λ are small, and not statistically significant for all outcomes. The f-statistic on the exclusion restrictions satisfies the rule of thumb of being greater than 10 at 143.70. Thus, the results show that survey attrition has little effect on the relationship between immigrant status and changes in health outcomes.

The results are consistent with past studies considering survey attrition in this context[14;15;32]. Chiswick (2006) suggests that the significant portion missing by the third survey wave, i.e., 31.1% does not explain the decline in immigrants' health status because the health status of those respondents who drop out differs insignificantly from those who remain in the survey. Chiswick suggests that immigrants' deterioration in health is not due to outmigration of healthier immigrants. Deri (2004) also suggests that outmigration does not affect her results because she finds evidence of increasing predicted health from the earliest immigrant cohort to the most recent immigrant cohort.

4.5 Conclusion

This paper contributes three advances to the literature comparing immigrants' change in health over time to that of native residents. First, this paper estimates individual rate changes in health for the same respondent over a ten-year study period. Most past studies use cross-sectional data, and so comparisons are made between recent immigrants and observationally identical, but different, long-term immigrants. Second, the individual rate changes in health are estimated with a first-difference model with fixed effects, which controls for both time-variant and time-invariant observable differences across respondents. Past longitudinal studies do not account for changes in income, education, or other time variant observable differences in respondents. Consequently, different rate changes in health between immigrants and native residents could be confounded by time-variant observable differences. Third, this paper corrects for different rate changes in health between immigrants and native residents, who have dropped out of the longitudinal survey, with Olsen (1980)'s sample selection model.

The results show that immigrants' increase in number of chronic conditions and BMI differ insignificantly from native residents over time. The result is inconsistent with most cross-sectional and longitudinal studies, which suggest immigrants' health deteriorates at a steeper rate than native residents' health. The result is also inconsistent with this paper's estimates based on a cross-section of the longitudinal data. The result implies that cross-sectional data showing long-term immigrants having worse health than recently entering immigrants does *not* imply immigrants' health deteriorate at a steeper rate than native residents. These results are not affected by survey attrition. Although the unadjusted average health outcomes differs by immigrant and dropout status, the estimates differ minimally from correcting for survey attrition with Olsen (1980)'s sample selection model.

However, immigrants' perceptions of their own health are found to worsen at a steeper rate than native residents. The steeper decline in immigrants' perceived health may have also resulted in a steeper rise in number of visits to family physicians than native residents. That is, immigrants' increasing negative perceptions of their health over time may have motivated them to visit their family physician more often than native residents over time. Thus, the results are consistent with the hypothesis that current policies promoting and maintaining immigrant health have helped immigrants keep their health advantage over native residents.

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Appendix A

Physician Fees and Services: Evidence from Comprehensive Physician Claims Data

Table A.1: The Percent Change in Number of Observations from the Exclusion Criteria

Physician Specialty	Total	(i) n	%	(ii) n	%
Neurology	207082	183605	88.66	169019	92.06
Internal Medicine	1915100	1705459	89.05	1549444	90.85
Urology	256485	229287	89.40	215495	93.98
Dermatology	1142350	990622	86.72	942607	95.15
Gastroenterology	197177	176168	89.35	160810	91.28
General Surgery	683006	610342	89.36	543529	89.05
Orthopedics	501641	442301	88.17	417690	94.44
Nephrology	229907	185318	80.61	184104	99.34
Obstetcrics/Gynaecology	1217992	1095438	89.94	981812	89.63
Cardiology	1032338	883848	85.62	867261	98.12
General Practice	30746698	27663932	89.97	27648392	99.94
Total	38129776	34166320	89.61	33680163	98.58

(i) Denotes the inclusion of services above the 90^{th} percentile of the most frequently provided services in the specialty
(ii) Denotes the inclusion of keeping fees that do not deviate from the fee schedule

Table A.2: Fixed Fee and Service Amendment Time Periods

Fixed-fee	Number of Days	Services Amendment	Number of Days
April 1, 1997	214	April 1, 1997	274
November 1, 1997	151	January 1, 1998	89
April 1, 1998	105	March 1, 1998	31
July 15, 1998	260	April 2, 1998	135
April 1, 1999	214	August 16, 1998	228
November 1, 1999	152	April 2, 1999	13
April 1, 2000	183	April 16, 1999	15
October 1, 2000	182	May 2, 1999	122
April 1, 2001	214	September 2, 1999	74
November 1, 2001	151	November 16, 1999	46
April 1, 2002	183	January 2, 2000	90
October 1, 2002	182	April 2, 2000	60
		June 2, 2000	121
		October 2, 2000	395
		November 2, 2001	150
		April 2, 2002	182
		October 2, 2002	60
		December 1, 2002	120

Appendix B

Revisiting the Healthy Immigrant Effect

Table B.1: First Stage Estimates of the Association between Covariates and Changes in Health Outcomes

Covariates	Categories	Number of Visits to Family Doctor				Number of Chronic Conditions				Perceived Poor Health				Body Mass Index			
		Model(1)		Model(2)		Model(1)		Model(2)		Model(1)		Model(2)		Model(1)		Model(2)	
		Coefficient	t-test	Coefficient	t-test	Coefficient	t-test	Coefficient	t-test	Coefficient	t-test	Coefficient	t-test	Coefficient	t-test	Coefficient	t-test
Education	Less than Secondary	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Secondary	2.593	0.84	2.053	0.45	0.461	1.31	0.644	1.33	-0.010	-0.05	0.195	0.74	0.436	0.58	1.044	1.13
	Trade School	2.196	0.71	1.436	0.31	0.631	1.78	0.800	1.64	-0.027	-0.12	0.166	0.62	0.695	0.93	1.277	1.38
	University	2.204	0.71	1.425	0.31	0.630	1.79	0.809	1.67	-0.014	-0.06	0.180	0.67	0.621	0.83	1.235	1.34
Marital Status	Married/ Common Law/ Partner	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Widowed/Separated/Divorced	0.202	1.02	0.282	1.19	0.088	3.33	0.101	3.37	-0.044	-2.11	-0.056	-2.38	-0.199	-3.97	-0.162	-2.89
	Single/ Never Married	-0.135	-0.35	-0.125	-0.27	0.046	1.62	0.061	1.90	-0.058	-2.39	-0.068	-2.36	-0.292	-5.12	-0.245	-3.80
Household Size	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	2	0.466	1.72	0.388	1.26	0.025	0.98	0.028	0.98	-0.007	-0.35	-0.016	-0.68	0.090	1.80	0.094	1.74
	3	0.488	1.92	0.489	1.73	0.013	0.44	0.013	0.41	-0.014	-0.58	-0.023	-0.85	0.124	2.13	0.157	2.47
	4	0.693	2.56	0.705	2.31	0.032	0.99	0.040	1.08	-0.006	-0.21	-0.019	-0.62	0.212	3.15	0.287	3.83
	5+	0.799	2.10	0.943	2.15	0.035	0.83	0.057	1.21	-0.033	-0.95	-0.042	-1.07	0.085	0.90	0.156	1.46
Income	Missing	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Less than $30,000	0.371	2.26	0.433	2.47	0.088	3.22	0.088	3.04	-0.006	-0.26	-0.011	-0.48	0.013	0.75	0.056	0.91
	$30,000-$60,000	0.115	0.85	0.201	1.39	0.066	2.66	0.064	2.45	-0.026	-1.31	-0.027	-1.27	0.067	1.24	0.068	1.19
	Greater than $60,000	0.181	1.22	0.261	1.64	0.084	3.17	0.090	3.20	-0.037	-1.69	-0.033	-1.45	0.103	1.86	0.103	1.75
Sex	Male	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Female	-0.133	-2.56	.	.	0.052	7.24	.	.	-0.018	-3.55	.	.	0.037	2.42	.	.
Age		0.009	0.72	.	.	0.009	5.71	.	.	0.001	1.15	.	.	-0.001	-0.41	.	.
Age-Squared		0.000	0.07	.	.	0.000	-2.80	.	.	0.000	-0.13	.	.	0.000	-3.21	.	.
Time Trend		0.033	2.65	.	.	-0.004	-2.21	.	.	0.006	4.40	.	.	-0.001	-0.32	.	.
Years Since Immigration		-0.026	-0.77	.	.	0.001	0.63	.	.	-0.004	-2.27	.	.	-0.003	-0.57	.	.
Years Since Immigration Squared		0.000	0.75	.	.	0.000	-1.10	.	.	0.000	2.03	.	.	0.000	0.80	.	.
Immigrant		0.106	0.34	.	.	-0.011	-0.40	.	.	0.046	2.05	.	.	0.000	0.00	.	.
Constant		-65.568	-2.66	0.046	2.94	7.635	2.16	0.163	107.39	-11.514	-4.40	0.053	41.42	2.715	0.39	0.194	60.48

NOTES:
Model 1 represents first differenced model without fixed effect. The covariates include age, age-squared, sex, province, marital status, education, household size, income, time trend, country of birth, years since immigration. years since immigration squared, and immigrant status
Model 2 represents second stage estimates after the first stage regression. In the first stage, each respondents' health outcome is first-differenced and regressed onto fixed effects for each respondent and time-varying covariates in first differences, which are provincial residence, marital status. education, household size, and income. Second, the parameters for respondents' fixed effects are regressed onto age, age-squared, sex, years since immigration, years since immigration squared, immigrant status. country of origin, and 1994 levels of provincial residence, marital status, education, household size, and income. The number of observations in Model 1 is n=39,757 and Model 2 is n=9,231

Table B.2: Second Stage Estimates of the Association between Covariates and Changes in Health Outcomes

Covariates	Categories	Number of Visits to Family Doctor				Number of Chronic Conditions				Perceived Poor Health				Body Mass Index			
		Model(2)		Model(3)		Model(2)		Model(3)		Model(2)		Model(3)		Model(2)		Model(3)	
		Coefficient	t-test	Coefficient	t-test	Coefficient	t-test	Coefficient	t-test	Coefficient	t-test	Coefficient	t-test	Coefficient	t-test	Coefficient	t-test
Education	Less than Secondary	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Secondary	-0.309	-1.09	-0.309	-1.09	0.017	0.53	0.017	0.53	0.041	1.81	0.041	1.81	-0.008	-0.14	-0.008	-0.14
	Trade School	-0.385	-1.33	-0.383	-1.32	0.028	0.86	0.028	0.86	0.046	1.98	0.046	1.98	-0.008	-0.13	-0.008	-0.12
	University	-0.210	-0.75	-0.208	-0.74	0.008	0.25	0.008	0.25	0.039	1.73	0.039	1.74	0.032	0.57	0.032	0.57
Marital Status	Married/ Common Law/ Partner	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Widowed/Separated/Divorced	0.062	0.24	0.060	0.23	0.009	0.49	0.009	0.49	-0.023	-1.44	-0.023	-1.44	-0.013	-0.33	-0.013	-0.33
	Single/ Never Married	-0.035	-0.23	-0.033	-0.21	0.029	1.65	0.029	1.65	-0.009	-0.62	-0.009	-0.62	0.067	1.87	0.067	1.88
Household Size	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	2	-0.084	-0.33	-0.087	-0.34	0.035	1.77	0.035	1.77	-0.016	-1.04	-0.016	-1.04	0.011	0.29	0.011	0.28
	3	0.110	0.33	0.106	0.32	0.014	0.69	0.015	0.69	-0.012	-0.68	-0.012	-0.68	0.029	0.66	0.029	0.65
	4	-0.093	-0.33	-0.097	-0.34	0.020	0.87	0.020	0.87	-0.026	-1.49	-0.026	-1.49	0.085	1.93	0.084	1.92
	5+	0.131	0.45	0.126	0.44	0.025	1.02	0.025	1.02	-0.016	-0.83	-0.016	-0.83	0.012	0.26	0.012	0.25
Income	Missing	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Less than $30,000	0.276	1.78	0.267	1.72	0.061	1.99	0.061	1.99	0.018	0.78	0.018	0.78	0.056	0.94	0.056	0.93
	$30,000-$60,000	0.438	2.56	0.432	2.52	0.041	1.40	0.041	1.41	0.013	0.56	0.012	0.55	0.089	1.55	0.088	1.54
	Greater than $60,000	0.447	2.90	0.443	2.87	0.030	1.01	0.030	1.01	0.024	1.03	0.024	1.03	0.065	1.12	0.065	1.12
Sex	Male	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Female	-0.236	-2.13	-0.234	-2.11	0.055	5.13	0.055	5.13	-0.016	-1.96	-0.016	-1.95	0.046	2.10	0.047	2.11
Age		-0.008	-0.28	-0.008	-0.27	0.010	3.45	0.010	3.45	0.002	0.79	0.002	0.79	0.002	0.43	0.002	0.43
Age-Squared		0.000	0.66	0.000	0.66	0.000	-1.77	0.000	-1.77	0.000	-0.04	0.000	-0.04	0.000	-2.47	0.000	-2.47
Years Since Immigration		-0.087	-0.86	-0.087	-0.86	0.003	0.96	0.003	0.96	-0.004	-1.27	-0.004	-1.27	-0.002	-0.28	-0.002	-0.28
Years Since Immigration Squared		0.002	0.88	0.002	0.88	0.000	-1.25	0.000	-1.25	0.000	1.05	0.000	1.05	0.000	0.19	0.000	0.19
Immigrant		0.592	0.67	0.594	0.67	-0.026	-0.60	-0.026	-0.60	0.039	0.98	0.039	0.98	0.018	0.17	0.018	0.17
Constant		0.054	0.07	0.052	0.07	-0.450	-5.45	-0.450	-5.45	-0.108	-1.84	-0.108	-1.84	0.049	0.34	0.049	0.34

NOTES:
Model 2 represents second stage estimates after the first stage regression. In the first stage, each respondents' health outcome is first-differenced and regressed onto fixed effects for each respondent and time-varying covariates in first differences, which are provincial residence, marital status, education, household size, and income. Second, the parameters for respondents' fixed effects are regressed onto age, age-squared, sex, years since immigration, years since immigration squared, immigrant status, country of origin, and 1994 levels of provincial residence, marital status, education, household size, and income. Model 3 is equal to Model 3 but includes Lambda. Lambda represents the selection equation's predicted value less one
The selection equation estimates the probability of dropping out with covariates equal to all covariates in Model 2 plus survey quarters as exclusion restriction. The selection equation is estimated in first differences with OLS. The f-statistic on the exclusion restrictions is 140.73 The number of observations in Model 2 and 3 is n = 9,231

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2009-04-27

Dr. Chris Auld
Department of Economics
University of Calgary
2500 University Drive. N.W.
Calgary, AB

Dear Dr. Auld:

RE: Physician Fees and Services: Evidence from Comprehensive Physician Claims Data

Ethics ID: E-22358

Student: Lawrence So

The above-noted proposal including the Letters (Funding Confirmation Letter, April 13, 2009), Protocol has been submitted for Board review and found to be ethically acceptable.

Please note that this approval is subject to the following conditions:
(1) access to personal identifiable health information was not requested in this submission;
(2) a copy of the informed consent form must have been given to each research subject, if required for this study;
(3) a Progress Report must be submitted by **April 27, 2010**, containing the following information:
- i) the number of subjects recruited;
- ii) a description of any protocol modification;
- iii) any unusual and/or severe complications, adverse events or unanticipated problems involving risks to subjects or others, withdrawal of subjects from the research, or complaints about the research;
- iv) a summary of any recent literature, finding, or other relevant information, especially information about risks associated with the research;
- v) a copy of the current informed consent form;
- vi) the expected date of termination of this project.

4) a Final Report must be submitted at the termination of the project.

Please note that you have been named as the principal collaborator on this study because students are not permitted to serve as principal investigators. Please accept the Board's best wishes for success in your research.
Yours sincerely,

Glenys Godlovitch, BA(Hons), LLB, PhD
Chair, Conjoint Health Research Ethics Board

GG/emcg
c.c. Ms. Gladys Glowacki (Health Records) Ms. Donna McDonald (RTA) Dr. Ken McKenzie
(information) Research Services Lawrence So (Student) Dr. A. Hollis (Co-Investigator)
Office of Information & Privacy Commissioner