THREE ESSAYS IN HEALTH ECONOMICS

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A Thesis Submitted to the School of Graduate Studies in Partial Fulfilment of the Requirements for the Degree Doctor of Philosophy

McMaster University

DOCTOR OF PHILOSOPHY (2013)
Department of Economics

McMaster University
Hamilton, Ontario

Title: Three Essays in Health Economics

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Number of Pages: xii, 154

Abstract

This thesis comprises three essays that empirically investigate important issues in two areas of health economics: physician labour supply and health insurance policy interventions.

In the first essay, gendered associations between family status and physician labour supply are explored in the Canadian labour market, where physicians are paid according to a common fee schedule and have substantial discretion in setting their hours of work. Data from 1991 to 2006 show no gender difference in physician labour supply after controlling for family status. Male and female physicians have statistically indistinguishable hours of work when never married and without children. Married male physicians, however, have higher market hours than unmarried male physicians and parenthood either increases their hours or leaves them unchanged. In contrast, married female physicians have lower market hours than unmarried physicians and parenthood substantially lowers market hours. Little change over time in these patterns is observed for males, but for females two offsetting trends are observed: the magnitude of the marriage-hours effect declined, whereas that for motherhood increased. Preferences and/or social norms induce substantially different labour market outcome across the sexes. In terms of work at home, the presence of children is associated with higher hours for male physicians, but for females the hours increase is at least twice as large. A male physician's spouse is much less likely to be employed in the presence of children, and if

employed, has lower market hours in the presence of children. In contrast, a female physician's spouse is more likely to be employed in the presence of children, and if employed, has slightly lower market hours in the presence of children. Both male and female physicians have lower hours of work when married to another physician.

This second essay examines the impacts of a mandatory, universal prescription drug insurance program on health care utilization and health outcomes in a public health care system with free physician and hospital services. Beginning in 1997, all residents of the province of Quebec, Canada, were required by law to have drug insurance coverage. Under this program, all persons under age 65 who are eligible for a private plan are required to join that plan, while the public prescription drug insurance plan covers all Quebecers who are not eligible for a private plan. Using the National Population Health Survey from 1994 to 2003, we find that the mandatory program substantially increased drug coverage among the general population. The program also increased medication use and general practitioner visits but had little effect on specialist visits and hospitalization. Findings from quantile regressions suggest that there was a large improvement in the health status of less healthy individuals. Further analysis by pre-policy drug insurance status and the presence of chronic conditions reveals a marked increase in the probability of taking medication and visiting a general practitioner among the previously uninsured and those with a chronic condition. We also find evidence of positive health gains among the chronically ill.

The third essay examines the impact of delisting routine eye exam services on patient eye care utilization and on providers' labour market outcomes in a public health care system. Beginning in the early 1990s, provincial governments in Canada started to

de-insure routine eye examinations from the basket of publicly funded health care services. We exploit delisting policy changes across Canadian provinces to estimate the impact of delisting from the supply- and demand-sides. Demand side analysis using the National Population Health Survey and Canadian Community Health Survey data suggests that the delisting of eye exams for the working age population decreased the probability of using eye care among this population group. However, the number of visits among those who continued to use eye care services was not affected. We also find suggestive evidence that the delisting policies targeted at the working age population were associated with increased eye care utilization among the elderly patients. Using the optometrist sample from the Canadian census data we find that the delisting of eye exams decreased optometrists' weekly work hours while raised their annual work weeks. There was no statistically significant effect on optometrists' income.

Acknowledgements

I would like to thank my supervisor Prof. Jeremiah Hurley for providing me with ongoing guidance and support throughout the past few years. His invaluable assistance and advice has helped me grow as a health economist. He always has a direction to point me in at the right time. I would like to thank my co-supervisor Prof. Arthur Sweetman. I cannot exaggerate how lucky and fortunate I was to end up working with him. His scientific guidance and support in all aspects has been incredibly valuable. I would also like to sincerely thank my thesis committee member Prof. Lonnie Magee, who introduced and taught me about the vast and interesting world of econometrics. The advice and knowledge that he has imparted on me has been invaluable.

I would like to send my full gratitude to Prof. Michael Veall, who introduced me to the field of economics and the Department of Economics at McMaster University. My sincere thanks also go to Prof. Jeffery Racine, Prof. Catherine Cuff and Prof. Philip DeCicca for their advice and discussion of my research topics. I am thankful to Prof. William Scarth, Prof. Marc-Andre Letendre, Prof. Seungjin Han and Prof. A. Abigail Payne who offered their valuable expertise and guidance in so many different ways and helped me a lot when I first entered the department's program several years ago. Many thanks to the department's administrative staff Jan Martens for her encouragement and support.

I would like to thank my friends and classmates: Christopher Gunn, Keqiang Hou, Taha Jamal, Cong Li, Jinhu Li, Qing Li, Evan Meredith, Mustafa Ornek, Sihui Tao, Wei Yang and Junying Zhao. Thank you for discussing and sharing research ideas with me. I have thoroughly enjoyed talking and working with all of you.

I would like to thank my parents-in-law for their understanding and support. I am especially grateful to my parents for their unconditional love, encouragement and support. I wish to thank my daughter Emily Y. Xu, whose happy smiling face inspires me to keep moving forward. Finally, I would like to thank my husband Qingyang Xu. He has been patiently supporting me for the most difficult years of my doctoral studies, and endured a lot of sacrifices. Thank you for always being there and supporting me through the good times and the bad.

Preface

The essays in Chapter 1 and Chapter 3 are co-authored with Professor Arthur Sweetman. I was responsible for the empirical analysis and participated in all stages of the research. The first chapter of this thesis was published in *Social Science & Medicine*, Volume 94, October 2013, Pages 17-25. The second chapter has been submitted for journal publication.

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Introduction

This thesis empirically investigates physician labour supply behavior and two issues of health care financing: the impact on utilization and health of extending drug insurance in the population through a universal insurance program, and the impact on service use and provider labour supply of reducing insurance coverage for optometry services.

Health human resource issues, and adequacy of physician workforce in particular, are an important concern for the health care system. Recent physician shortages or forecasts of shortages in many developed countries pose great challenges for policy-makers and health workforce planners. Understanding the labour supply behaviour of physicians is an important building block for developing effective health human resource policies.

The first essay explores gendered associations between family status and physician labour supply. Female physicians account for an increasing proportion of the physician workforce in the last several decades. On average, the share of female physicians increased from 29% in 1990 to 43% in 2009 across OECD countries (OECD 2012). This trend has crucial implications for physician workforce planning policies since it is well documented that female physicians work fewer hours. Therefore, understanding the sources of differences between male and female physicians' work hours is important.

The first essay differs from many existing studies of gaps in earnings/wages between male and female physicians. Available evidence suggests that female physicians have lower earnings than male physicians but a number of aspects of the wage gender gap remain poorly understood. Some studies suggest that hourly earnings equality has been achieved (e.g. Baker 1996) while other studies report conflicting results (e.g. Esteves-Sorenson and Snyder 2012). Gaps in earnings are driven by differences in work hours and hourly wages which are jointly determined in general. Understanding differences in work hours can provide insights into the earnings gap issue.

Examining gendered differences in labour supply when pay per service has been realized is also of interest from a labour economics perspective. Physicians in Canada have great flexibility in choosing their work hours and are paid by a common fee schedule. The Canadian physician labour market serves as an interesting case to study how males and females allocate their time to the labour market under equalized gross payment. Observed gender differences in labour supply in this context must be driven by factors other than wage-related gender discrimination in the labour market.

Building on research of family economics, this essay also seeks to understand how highly educated males and females respond to family responsibilities in a household context. Economic models of the family recognize the division of labour between spouses as a principal source of the gains to marriage. If women have comparative advantage at household activities while men have a comparative advantage in the labour market, then wives specialize in home production while husbands concentrate on the labour market.

The birth of a child increases the value of time inputs to home production. The time-intensive demands of parenting, which cannot be smoothed over the life-cycle, may change the time allocation across home and market production. Given equal intensive investment in human capital and equal gross payment per service, male physicians have no obvious incentive to provide market hours that differ from those of female physicians. Therefore, it is important to empirically investigate physicians' time allocation to home and market.

Using Canadian census data from 1991 to 2006, findings from the first essay suggest no gender differences in physician labour supply after controlling for family status. Single male and female physicians work similar hours, but marriage and the presence of children opens the gap in work hours by affecting male and female work hours in opposite directions. Female physicians have lower market hours when married and when a parent. Male physicians have higher market hours when married and their hours are unchanged or increased with parenthood. The results are remarkably similar whether labour supply is measured as hours per week, weeks per year, or the probability of working part-time. Given the uniform gross payment schedule, the finding of a substantial difference in work hours of male and female physicians provides a new perspective on explaining the earnings gap.

This study provides direct evidence for the relationship between family status and home hours. Presence of children is associated with higher home hours for both male and female physicians. Combined with the findings on market hours, this implies that male physicians increase both market hours and home hours while female physicians substitute home hours for market hours. Our results also show that, on average, total market and home hours are similar for male and female physicians.

Modelling physician and spousal hours jointly contributes to the literature on physician labour supply. The spouses of both male and female physicians reduce work hours in the presence of children. Having a highly educated spouse has little impact on male physicians' work hours, unless the spouse is a physician. In contrast, having a highly educated spouse reduces female physicians' work hours. Both male and female physicians have lower hours of work when married to another physician.

The first essay models physician labour supply in the family context and provides insight regarding gender differences in physician labour supply. Male and female physicians have similar hours of work when never married and without children. Marriage, motherhood and spouse education are negatively associated with female physician's hours of work. In contrast, marriage and fatherhood are positively associated with male physician's hours of work; spouse education has little impact on his hours of work unless his spouse is a physician.

Health insurance is a key element of the health system. Insurance spreads financial risks associated with illness. But it can also induce individual behavioral responses that increase the expected health care spending. Demand-side cost sharing (user charges) is one way to balance the traditional trade-off between risk protection and moral hazard in insurance design. One focus of recent studies on insurance design is to extend the analysis from a single health care service to multiple services (McGuire 2012), enabling one to study cross-price effects of insurance across services. Such cross-price effects (e.g., the effect of an increase in user charges for a given service on the utilization of other services) are important in the context of optimal insurance design. One most active area for current research is on the cross effects of coverage for prescription drugs.

Public policies related to health insurance influence the functioning of the health care market. Therefore, evidence of how health care utilization responds to insurance coverage is necessary for sound public policy design. The second essay addresses one fundamental question regarding health insurance coverage: how does the expansion of insurance affect health care utilization and health outcomes. Public health insurance expansions are central to numerous health care reforms and public health policy debates. A number of studies have investigated the Medicaid expansions in late 1980s and early 1990s and further expansion of the State Children's Health Insurance Program (SCHIP) in the United States. The evidence suggests that eligibility for these public insurance programs significantly increased health care utilization among children and prenatal care among pregnant women but the effects on health outcomes are relatively small (e.g., Currie and Gruber 1996; Lo Sasso and Buchmueller 2004). A recent study of the Medicaid expansion in Oregon finds that insurance led to increased health care utilization and substantial improvements in mental health, and reduced financial strain (Baicker et al. 2013). At the same time, these public programs result in crowding-out of private insurance, ranging from 4% to 60% (Gruber et al. 2008). However, findings from these studies with a focus on the low income and children may not generalize to the rest of the population and other institutional settings. Moreover, broader institutional context is important since the organization of health insurance varies greatly across countries. Therefore, studies of specific policy interventions can provide valuable insights.

The second essay studies the expansion of one specific insurance type – prescription drug insurance. It examines the impact of a mandatory universal drug program on health care utilization and health outcomes. The rising financial burden associated with prescription drugs has raised public concerns about access to pharmaceutical drugs in Canada and internationally. The recent expansion in the United States of Medicare (Part D) to include prescription drug benefits is one example of a policy intervention to deal with this issue. In Canada, coverage for outpatient prescription drugs is not included in the Canada Health Act. Each province has public drug programs for seniors and social assistance recipients but the working age population in many provinces relies on employer-provided private insurance. This leaves a significant number of Canadians un-insured against the costs of prescription drugs. A number of proposals and recommendations for a national pharmacare program have been put forward and debated in the last decade. Empirical evidence to inform such policy changes in the Canadian context is very limited.

This study makes several contributions. First, it exploits a natural experiment to identify the impacts of a mandatory drug insurance program. Many previous studies suffer from endogeneity in health/drug insurance coverage. The policy change in this analysis is a credible source of exogenous variation in drug coverage. Second, analysis of the mandatory universal drug program in Quebec can provide valuable insights to the policy debate on expanding public drug coverage generally. The mixed public and private financing structure of the Quebec program may also inform policymakers on general health insurance financing. Third, this study employs a systematic approach to examine the cross-effects of drug insurance expansion. Evidence on this is informative for policies related to optimal health insurance design.

Using the National Population Health Survey from 1994 to 2003, we find that the mandatory program substantially increased drug coverage among the general population. The program also increased medication use and general practitioner visits but had no statistically significant effect on specialist visits and hospitalization. There was a large improvement in the health status of less healthy individuals. Further analysis by pre-policy drug insurance status reveals a marked increase in the probability of taking medication and visiting a general practitioner among the previously uninsured. The chronically ill experienced a large increase in medication use and improvement in health status.

The findings have important policy implications. In the economics of health insurance design, demand-side cost sharing is a general mechanism to deal with moral hazard. However, when individuals are poorly informed about the value of a health service, they may cut back on both necessary and unnecessary services. In the case of pharmaceutical drugs, reduction in cost sharing through insurance coverage can generate substantial health gains concentrated among individuals with poor health status. This implies that individuals without insurance did cut back on drugs that were effective treatments for them. The positive cross-effects on physician visits, which are free in Canada, show that drug coverage can improve access to physician services. Finally the mixed public and private design of a universal drug program builds on the prevalent institutional features of the Canadian context.

The third essay turns to a different aspect of health insurance: the scope of insurance coverage. Public health insurers cannot fund all health care services given the resource constraints. On the margin, insurers have to make decisions on a basic benefits package, which involve priority setting or rationing of health services. Therefore, listing or delisting a health service by public insurers always attracts a lot of public attention.

This essay studies the impacts of delisting routine eye exam services from public insurance coverage. We exploit the natural experiment of delisting policy changes across Canadian provinces over time to estimate the impacts of delisting on eye care utilization and provider labour supply. The findings show that the delisting decreased the probability of using eye care. The number of visits among those who continued to use eye care services was not affected. We also find suggestive evidence that the delisting policies targeted at the working age population were associated with increased eye care utilization among the publicly covered elderly patients. For the provider side, optometrists adjusted their working schedule by decreasing weekly hours and increasing work weeks. There was no statistically significant effect on optometrists' income.

The third essay develops an economic framework to evaluate public health insurer's delisting policies. One major contribution of this study is that it examines the delisting policy effects from both demand- and supply-sides. Findings from this chapter suggest that a system-wide approach of evaluating delisting policies is important and necessary from a policy perspective.

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Chapter 1

Gender, Family Status and Physician Labour Supply

1.1 Introduction

Over the last several decades one dramatic change across OECD countries in the structure of the physician workforce is the increasing participation of women physicians. In the United States, the proportion of female physicians increased from 20% to 30% between 1990 and 2007 (NCHS, 2009). In Canada the share increased from 12% in 1980 to 36% in 2010 (CIHI, 2011). Implications of this trend are of interest to health human resource planners as well as to researchers. A key issue is that, on average, female physicians practice fewer hours than their male counterparts. Understanding the sources of differences between male and female physicians' work hours is essential for developing effective human resource policies in the health care sector.

Most existing economic studies of labour market differences between female and male physicians have focused on understanding gaps in earnings and/or wages.

Contributions include Ohsfeldt and Culler (1986), Rizzo and Zeckhauser (2007), and Theurl and Winner (2011). Across developed countries a common finding is that females have lower annual earnings, although there is more debate – see, for example, Baker (1996), and Bashaw and Heywood (2001) – about gaps in hourly wages. Gaps in earnings stem from differences in work hours and/or hourly wages; therefore work hours are central to understanding the mechanics of this issue.

Physician labour supply is, additionally, an important question independent of earnings since physician time is key to service provision. Research looking at trends in hours of work includes Crossley et al. (2009) and Sarma et al. (2011), with the latter noting that the presence of children influences female physicians' hours; service provision is explored, for example, by Constant and Léger (2008). These studies observe females providing fewer hours or services per year. Similarly, Watson et al. (2006) find that in 2001 average female general practitioners in Canada had paid workloads equivalent to 68% of their male counterparts. Understanding physician labour supply and gender issues is useful for human resource planning purposes in the health care sector given that medical fields are highly regulated and entry is limited. Although not an issue restricted to physicians, the public return also increases with greater physician work time (within safe limits) since the cost of training is taxpayer subsidized.

Canada's institutional context is particularly amenable to this study and permits a contribution to the large research literatures regarding gendered labour market differentials surveyed by Bertrand (2011), and the economics of the family discussed by Browning et al. (forthcoming). It has a publicly financed single-payer system (there is virtually no private sector for medically necessary physician services) where physicians have flexibility in choosing their hours of work. Most physicians are not employees but self-employed professionals. Some physicians, such as surgeons, may have aspects of their practice restricted because of limited access to facilities such as operating theaters, but there are not normally limitations on office services. A small number of physicians have salaried positions, but even here those wishing to work extra clinic or office hours may normally do so. Furthermore, for most of our data period there was a perceived physician shortage (Postl, 2006). Thus physicians not only have enormous flexibility in setting their hours, but there has been social and government pressure to increase them and offer after-hour services.

Beyond flexible hours, gender gaps in market wages have been proposed as one reason that females allocate less time to the labour market. Pre-labour market gender discrimination may exist regarding the allocation of, and/or self-selection into, medical specialties (Gjerberg, 2002), but in Canada, conditional on specialty, the fixed and universal fee schedule for all medically necessary services that is negotiated between each provincial government and its medical association largely eliminates fees as a source of gender discrimination given the single-payer system. Whenever they deliver a service in either the outpatient or inpatient sector, physicians are reimbursed by the government based on the fee schedule. These common fees imply equal gross payments per service regardless of gender – i.e., equal earnings potential. Net payments may, however, vary if physicians have different overhead costs. In particular, physicians who mostly work in hospitals or certain clinics have lower overhead costs, but the fee schedule reflects this to some extent. Payment per hour may also vary with treatment style and/or productivity. Importantly, these production side decisions are endogenous and potential income – payment per service – is gender blind. Beyond medically necessary services, physicians may also receive payments for publically uninsured services such as completing insurance application forms or cosmetic procedures. On average, this source of income is modest and provincial medical associations commonly provide guidance (including a recommended common fee schedule) regarding billing for uninsured services. Overall, while pre-labour market gender based barriers may exist in some contexts (e.g., Nomura & Gohchi, 2012), for practicing physicians, the Canadian labour market serves as a laboratory allowing us to study how, given current social norms and individual preferences, highly educated females and males respond when equal gross pay per service (reflecting potential earnings) has been realized and workers have substantial discretion in choosing their market hours.

In this paper, we use Canadian census masterfiles to characterize gender differences in the relationship between family structure and labour supply broadly defined; we focus on weekly hours of market work, although we also examine other elements of labour supply including weeks of paid work per year, part-time employment, and hours of non-market work. Family responsibilities, especially child care, have been cited as reasons for female physicians' lower hours of paid work. Few studies have, however, formally examined the relationship between family structure and physician labour supply from a household perspective. Furthermore, there is little evidence on how spousal characteristics are related to it. Lee and Mroz (1991) explore limited aspects of family context, but find that non-practice income is the key correlate explaining the male-female gap. On the other hand, Jacobson et al. (2004) show that female physicians who are parents have significantly reduced hours of market work. Gjerberg (2003), using

Norwegian data, looks at how family obligations are combined with market work finding that specialty choice and the probability of working part-time are affected by the presence of children, and that having a spouse who is a physician improves career outcomes. Although her focus is on earnings, Sasser (2005) is particularly relevant since she examines how much of the gender gap in annual earnings among physicians is due to women's greater family responsibilities using panel data for young US physicians. Comparing before and after family status changes, she finds that female physicians earn 11 percent less once married, plus an additional 14 percent less after having had one child or 22 percent less after having had two or more children.

The remainder of the paper is organized as follows. Section 2 presents a short review of the family status-labour supply research literature outside of health economics. The data and econometric methods used are briefly described in the third section; section 4 reports descriptive statistics; and regression based empirical results are presented in section 5. The last section discusses the findings and concludes.

1.2 Family Status and Labour Supply Patterns

Research on the economics of the family regards specialization in production and economies of scale as principal sources of economic gains to partners co-habiting. Traditionally, if women have a comparative advantage in home production, while men have a comparative advantage in the labour market, then wives specialize more in home production, while husbands concentrate on the labour market (e.g., Becker, 1991; Lundberg & Rose, 2000, 2002). Empirical work in developed countries observes that married men work longer hours of paid employment and have higher wages than unmarried men, while the case is reversed for women (e.g., Antonovics & Town, 2004).

After the birth of a child the value of home production increases, as do household costs; however, unlike costs which, according to the life-cycle hypothesis, may be smoothed across time by forward-looking agents via saving and borrowing in financial markets, the time-intensive demands of children cannot be shifted inter-temporally (although some services may be purchased in the market, and the associated costs can be smoothed). This inability to inter-temporally substitute parenting time/effort has implications for the distribution of labour supply across home and market production. Total family home and market hours, the sum of hours of work at home and in the labour market by both partners, will vary in the presence of children to compensate for the demands of child rearing whose costs cannot be smoothed. Additionally, the value of specialization in the household, the difference across partners in each of home and market hours of work, may increase depending upon the technology of child rearing employed (Lundberg & Rose, 2000, 2002). Beyond pure economic rationales deriving from market factors, the degree of specialization between home and market labour supply may vary systematically across the sexes as a direct effect of underlying preferences and social norms. Of course, tastes also generate family matches, and assortative mating may play a role in household labour supply. In a broader theoretical framework, household labour supply and consumption decisions are modeled using different household utility functions: unitary, collective or non-cooperative models.

An extensive literature, surveyed by Browning et al. (forthcoming), also examines how spousal characteristics affect joint labour supply and/or time allocation. In the general population, the presence of children is found to have positive or insignificant effects on earnings and paid work hours of men and negative effects on those of women (Choi et al., 2008; Angrist & Evans, 1998). Some empirical evidence using reduced form regressions suggests that female labour supply is responsive to changes in spouses' wages while male labour supply is not (e.g., Bloemen & Stancanelli, 2008). Husbands' hours are also not related to either their own education, or their spouses' hours.

How relevant these findings from the general population are to physicians is an empirical question, but there are reasons to expect some differences. Female physicians invest as much in human capital as male physicians, and both are relatively more productive in the paid labour market than the general population, so the substitution effect with respect to labour supply is larger than the average in society. Of course, the income effect is also larger. Also, as discussed above, in the Canadian context female physicians are paid by the same fee schedule as male physicians, so it's not clear that, conditional on field of specialization, male (female) physicians have any incentive originating in the labour market to provide market hours that differ from those of females (males). This suggests the existence of non-market motivations for any observed differences in labour supply patterns.

1.3 Data and Methods

Our study uses pooled Canadian Census masterfiles from 1991, 1996, 2001 and 2006, which are 20% random samples of the population. For the topic addressed in this paper, the census is the only available data source. In addition to being able to credibly identify a large and representative sample of physicians, it has information on marital status and children in the household. Furthermore, for each physician and his/her spouse (if one is present), it has information on demographics and market labour supply (hours of work in the census week, weeks in the previous calendar year, and whether most weeks in that year were part/full time). Also, since 1996 it has had three questions with categorical responses about weekly time use for child care, housework, and caring for seniors.

We identify as physicians those who both report their occupation as such and also indicate that they have a "degree in medicine, dentistry, veterinary medicine or optometry" (this census question does not distinguish among these categories, but does indicate all relevant degrees, not only the highest degree). Physician specialties, although not perfectly observed, are obtained from questions about the occupation and major field of study of the highest degree (usually post-MD residency training unless a PhD or other advanced degree is also held). The sample for analysis is restricted to permanent residents of Canada (citizens and non-citizens), and since the hours of work decision differs for resident trainee physicians, we remove those who report attending school either part-time or full-time. Since we focus on practicing physicians, those who are out of the labour force or unemployed are omitted. As shown in Table 1.1, only a very small proportion of physicians are unemployed or out of the labour force. (The number of unemployed is so small that we have to merge it with the out of labour force group to satisfy Statistics Canada's minimum cell size requirements.) For simplicity, those reporting non-positive income are also omitted, although this last restriction makes little difference to the labour supply results.

The variable of "work at home" is calculated as the sum of the midpoints of the three categorical variables representing weekly time use for child care, housework, and caring for seniors. (Sensitivity tests using interval regression for the minimums and maximums of each range are presented in Appendix Table 1.9 and the results are quite similar.) Given the topic at hand, we restrict our sample to physicians aged 28 to 50 in each census year (about 63% of all practicing physicians) since it is more likely that children would still be resident in the household and we cannot observe children ever born in most years. However, in the 1991 census there is a measure of children ever born for females and it suggests that the disagreement between children ever born and children resident in the household is less than 4% for those aged 28 to 50. (Section 5.1 discusses physicians older than 50.) Our dependent variables, denoted as Y_i in equation (1) for physician *i*, are measures of labour supply including self-reported market and home hours in the census week, and part-time status and weeks of market work in the previous year. We estimate the following reduced form models:

$$Yi = b_0 + b_1 Female_i + b_2 MaritalStatus_i + b_3 Female_i * MaritalStatus_i + b_4 Children_i + b_5 Female_i * Children_i + b_6 X_i + \varepsilon_i \quad (1)$$

where *Female* is an indicator variable set equal to 1 for a female physician, 0 otherwise; *MaritalStatus* is a vector of indicator variables for married, common-law, and divorced/widowed/separated, with never married being the omitted group. Similarly, *Children* is a vector of indicators for one child, two children, and three or more children, with no children being the omitted group (alternative specifications are discussed below). Interactions between *Female* and all the family status variables (marital status and children) are also included. Control variables also included in the regression are vectors of variables for age, physician specialty, work settings and locations, province of residence and census year. One advantage of this approach, compared to separate estimations for male and female physicians, is that we can perform tests on these differences between the sexes.

A probit model is used for the part-time status equation. Ordinary least squares (OLS) regressions are employed for the hours and weeks equations rather than Tobit models since, as discussed by Angrist and Pischke (2009), Tobit is well suited to situations with censored dependent variables, which is not the case here. Since weeks of work above 52 per year or hours of work per week below zero are not feasible, Tobit coefficients are difficult to interpret and represent tastes for nonexistent values of the dependent variable. (However, Tobit and negative binomial regressions are presented as sensitivity tests in Appendix Table 1.9, showing that the findings are robust.) Instrumental variables estimation is also not employed to attempt to establish causal impacts of marital status or the presence of children on work time since there are no obvious variables in our dataset to serve as instruments. Our findings are, therefore, best interpreted as descriptive.

Beyond the analysis in equation 1, the family context is explored in more detail in a series of regressions that focus on hours of work as the dependent variable. (In additional work the estimated coefficient patterns were quite similar when weeks of work and part-time status were employed, but only the results for hours are presented to save space.) In addition to depending upon tastes, the degree of specialization in the household may depend on each partner's wage rate. However, since the partner's wage is endogenous, we use the education level of the spouse (who may or may not be a physician), which is pre-determined and an important predictor of earnings potential, as an independent variable to proxy the trade-off between market and home time.

A series of regressions modelling spouses' joint market labour supply decision are presented in Table 1.5. First, a probit equation models the probability of the spouse not working (all physicians are working by construction). Second, as specified in equation 2, a seemingly unrelated regression (SUR) model is estimated for the sample where both spouses participate in the paid labour market. Including both spouses regardless of their labour market status changes the coefficient estimates very little.

$$Phys.Hours_i = b_{10} + b_{11}CommonLaw_i + b_{12}Children_i + b_{13}SpouseEducation_i + b_{14}X_i + u_i$$
$$SpouseHours_i = b_{20} + b_{21}CommonLaw_i + b_{22}Children_i + b_{23}SpouseEducation_i + b_{24}X_i + v_i \quad (2)$$

The SUR framework jointly models the two equations and provides an estimate of the covariance between the two error terms – cov(u_i, v_i). This provides insight into one aspect of assortative mating: do individuals who have positive (negative) unobservables/residuals in the hours equation tend, on average, to partner with similar spouses? In the two branches of the SUR model, the *CommonLaw*, *Children* and *SpouseEducation* (which refer to the physician's spouse's education) variables take on exactly the same values for each couple. However, the values of some control variables take on different values in the two branches.

As a third approach, expanding upon Lundberg and Rose (1999), we define measures of family (physician and spouse), as opposed to individual, labour supply. These dependent variables are employed in columns 4 through 7 of Table 1.5 and, using Lundberg and Rose's terminology, are: (i) "intensity", the sum of the two spouses' hours that allows the variation in total family effort to be observed; (ii) "specialization", which is the mathematical difference between the two partner's hours and reflects the divergence in husband's and wife's hours within the family. Each has two versions: one measuring market hours, and the other combining market and home time.

1.4 Descriptive Analysis

Table 1.1 shows sample means (and standard deviations for the dependent variables) for outcome, demographic and professional variables from the 1991 and 2006 Censuses. Though the average market weeks and hours are both high compared to the population, they are lower for females than males. Females' hours of work at home, however, are substantially greater than those for males. Part-time status is much more common for females. For both males and females, average market weeks and hours declined slightly while part-time status increased between 1991 and 2006; average weekly home hours increased during this period. Interestingly, the sum of market and home hours is almost the same for male and female physicians in 2006.

Focusing on family status, although the difference between males and females declines over time, male physicians are older and substantially more likely to be married than females. About a quarter of the males have no children, as do one-third of females. Among those with children, males are more likely to have larger families.

The bottom panel of Table 1.1 presents descriptive statistics for physicians' spouses. On average, male physicians' spouses are much more likely to be unemployed or out of labour force than female physicians' spouses. At the intensive margin, male physicians' spouses tend to have much lower hours of work than female physicians' spouses. Male physicians' spouses have lower educational attainment compared to female physicians' spouses. Interestingly, about one third of female physicians are married to other physicians.

Labour market hours in the census week by gender, marital status and the presence of children are presented in Table 1.2. Never married male and female physicians without children work hours that are statistically indistinguishable, and virtually no "never married" physicians have children. However a gender gap opens with marriage, which on average is associated with higher labour market hours for males but lower ones for females. Similarly, the presence of children, particularly those less than age 5, is associated with stable or increased hours for men but decreased hours for women.

1.5 Estimation Results

1.5.1 Family Status and Physician Labour Supply

OLS regression results with labour market hours as the dependent variable are shown in the first two columns of Table 1.3 with different sets of control variables (specialties and main work setting); hours of work at home are displayed in column 3, annual weeks of market work in column 4, and marginal effects from a probit model of part-time status in column 5. As reflected in the *Female* variable, even in our large sample with commensurate standard errors, never-married male and female physicians with no children have market labour supply outcomes that are statistically indistinguishable, although never married female physicians work more at home. Overall, when neither married nor caring for children, male and female physicians make very similar allocations of time to market production in this occupation characterized by substantial autonomy in determining labour force participation and equal earnings potential.

Marriage is, however, associated with substantial and statistically significant differences. Married male physicians work longer hours per week both at work and home, more weeks per year, and have a decreased probability of working part-time compared to those not married. For females, the coefficient on the interaction term tells us they work approximately six fewer hours per week than their married male counterparts in the labour market, but the coefficient for work at home is effectively zero. The sum of the coefficients on married and female-married in column 2 indicates that married females work in the market is on average over three and a half hours less than their unmarried female counterparts.

The effect on market work for females in common law relationships goes in the same direction as that for married females, but the point estimates are marginally significant. In contrast, females who are widowed, separated or divorced, resemble the never married for hours, although they work slightly more weeks and are less likely to work part-time.

Focusing next on the presence of children, conditional on marital status, fatherhood has little effect on male market labour supply until men have three or more children, when it increases. In stark contrast, motherhood reduces market hours substantially. Having one child subtracts six hours per week, while two children reduce hours by seven, and three by eight. Motherhood similarly reduces weeks of work and increases part-time status. Alternative specifications of the presence of children in Appendix Table 1.7 show that the magnitude of the negative motherhood effect is largest when a pre-school child is in the household.

In terms of home production, the presence of children is associated with higher home hours for men, but the increase in home hours for women is twice as large. Clearly, there is an asymmetry in the division of labour in the market and in the home. Supplementary analysis in Appendix Table 1.7 shows that children's age is an important correlate of home hours.

Looking at the lower half of Table 1.3, the specialty and work setting variables have the expected signs. Regressions with and without the full set of controls are presented only for hours of market work to conserve space, since all changes in the coefficient estimates are small. Small changes in the family status coefficients with the introduction of the additional variables imply that there is not much association between family status and areas of specialization.

Figure 1.1 depicts age-hours profiles for market and home hours by sex from regressions like those in columns 2 and 3 of Table 1.3, but allowing for interactions of the *Female* variable with the cubic in age. (As seen in Appendix Table 1.8, adding these interactions has little impact on the coefficients of interest.) Holding regressors other than *Female* and *Age* constant at their means, interesting patterns in the age-hours profiles are visible. Female physicians' market hours increase from their mid-30s to mid-40s, while their home hours are an inverted-U that peaks in the mid-30s. Male physicians' market hours decrease and their home hours have a similar inverted-U pattern. Male physicians always work more hours in the market and less hours at home than female physicians.

Beyond our sample, Appendix Table 1.6 presents regression results for physicians aged 51 to 74. Remarkable conformity with the younger group is evident. The relationship between family structure and labour supply appears over the life cycle. In the appendix, we also explore the relationship between gaps in spousal ages and labour supply; it finds modest relationships, but no real effect on the marriage and children coefficients (Results are shown in Appendix Table 1.10 and Appendix Table 1.11).

Table 1.4 disaggregates the regression results by census year to determine if the associations between family status and work hours have changed over time. (The results are similar when, respectively, common law, and widowed, separated or divorced, are separated from married and never married. However, the standard errors are slightly larger.) For male physicians, the effects of marriage and fatherhood are fairly stable. However, for females, two offsetting and remarkable trends are observed. The marriage effect has decreased appreciably relative to 1991, whereas the gap for motherhood has grown, with most of the change occurring by 1996. This suggests females are growing less affected (becoming more like males) in terms of the relationship between marital status and working time, but moving in the opposite direction with respect to motherhood.

1.5.2 Family Status and Household Working Time

Table 1.5 focuses on the joint labour supply outcomes of common law and married families. Column 1 reports marginal effects from probit models with the indicator for being unemployed or out of labour force as the dependent variable, and columns 2 and 3 report seemingly unrelated regression (SUR) results, with male physician families in the upper panel and females below. Individuals in physician-physician families are included twice: once as a physician, and a second time as a spouse. The regressors presented are identical; hence, for example, the "spouse with Master or PhD" coefficient should be interpreted in the physician column as the relationship between a physician’s hours and his or her spouse’s education, and in the spouse column as that between the spouse's hours and own education.

At the extensive margin (column 1), a male physician’s spouse is much more likely to be unemployed or out of labour force in the presence of children. In contrast, a female physician's spouse shows no, or the opposite, relationship on average.

Interpreting the SUR models in columns 2 and 3, it is interesting to first note that there is a positive and strongly statistically significant correlation between the unobserved components of the hours supplied by each couple. This implies that, conditional on the observed characteristics, if one partner works unexpectedly high (low) hours, the other is also likely to do so, which is consistent with assortative mating. Looking next at marital status, both male and female physicians' hours are unaffected by whether they are living married or common law. However, the spouses of male physicians appear to work slightly longer hours when living common-law, whereas the reverse is true for the spouses of female physicians.

Regarding the presence of children, consistent with the results seen in Table 1.3, male physicians' hours of work are unaffected by the presence of a small number of children, but increase as the number of children grows. Male physicians' spouses, in contrast, reduce their hours of work substantially with the presence of the first child, and the point estimates suggest slightly larger hours reductions as the number of children increases. Interestingly, married female physicians' hours reductions associated with children are remarkably similar to those of male physicians' spouses. However, female physicians' spouses also reduce their hours for small numbers of children, but not for larger ones. Overall, in households with male and/or female physicians, the reduction in market hours associated with motherhood is much greater than that for fatherhood. In Appendix Table 1.12, we explore this phenomenon separately for physician-nonphysician and physician-physician households, and the pattern holds for each.

Addressing the impact of spouses' education (and by proxy earnings potential) on physicians' hours of market work, spouses' education is observed to have no effect for male physicians unless the spouse is also an M.D., however it does have some effect on the spouses' own hours. In contrast, female physicians' hours of market work are somewhat influenced by the education level of their husbands, especially if the husband is a physician.

In the remainder of Table 1.5, the results for household work intensity (the sum of both spouses' hours) are presented for market hours in column 4, and the sum of market and home hours in column 5. The degree of specialization (differences between the spouses' hours) is presented in column 6 for market hours, and in column 7 for combined home and market hours. To facilitate interpretation, the average value of each dependent variable is presented in the relevant column.

First, we focus on the results for market hours (columns 4 and 6). For both male and female physician households, the sum of market hours (intensity) decreases, and the difference between the market hours of the two spouses (specialization) increases, in the presence of children. Interestingly, for males in the upper panel, the absolute value of the change in specialization exceeds that for intensity, whereas for female physicians the reverse is true. This is consistent with mothers reducing their hours more regardless of occupation. For male physicians with a highly educated spouse, and particularly a physician for a spouse, these results imply greater household total hours (intensity), and a more even allocation of hours across the sexes (less specialization). In contrast, the intensity of labour supply for female physician households, as seen in the lower panel, is not particularly affected by the husband's education, unless he is a physician, and the difference in market hours (specialization) between spouses increases slightly with the spouse’s education.

Second, we look at the results for total home and market hours (columns 5 and 7). The presence of children is associated with substantial increases in intensity (total home and market hours) for both male and female physician households. However, the degree of specialization (gap between the male and female hours) massively increases in magnitude for male physician households (becomes more negative, since mean specialization is negative). In contrast, there is a much smaller effect in female physician households although the direction is similar (the mean specialization is again negative, indicating that total hours of market and home production are greater for females).

1.6 Conclusion and Discussion

Building on previous studies that find a relationship between the presence of young children and physician market labour supply, such as Watson et al. (2006) and Sarma et al. (2011), this analysis explores relationships between aspects of family structure and various dimensions of labour supply. Moreover, it extends the literature by providing evidence on work at home.

Family status has a quantitatively important relationship with labour supply for both male and female physicians. On average, there is little or no difference in labour supply for market or home production among physicians who are never married and do not have children. However, both marriage and the presence of children are associated with differences in the amount of professional labour provided that are of opposite signs for males and females: increases for males (not when there are a small number of children), and decreases for females. The results are remarkably similar whether labour supply is measured as hours per week, weeks per year, or the probability of working part-time. For this occupation with a relatively large degree of autonomy in selecting market hours, and uniform rates of pay conditional on specialty, we observe gendered differences in professional hours that are substantial. Moreover, while trends over time appear to be stable for males, they have shifted somewhat for females. Females are reducing their hours of market work less when married, but increasingly cutting back in the presence of children.

Home production is, similar to the well-known pattern in the general population, disproportionately undertaken by females even within this highly market-oriented population. Interestingly, while the total home and market work hours associated with children is comparable in male and female physician households, specialization associated with the presence of children is substantial in male physician households but modest in female physician ones.

Modelling physician and spousal hours jointly also differs from most existing studies. Both male and female physicians' spouses reduce work hours in the presence of children, as do female physicians. In contrast, male physicians not married to other physicians do not reduce, and even increase, their market work. More generally, there is a positive and strongly significant relationship between the unobserved components of the hours supplied by the two members of a couple. For both male and female physician households, market intensity decreases and specialization increases in the presence of children. For male physicians, having a more educated spouse implies greater household market intensity and less specialization. In contrast, the intensity of labour supply for female physician households is not particularly affected by the spouse's education, unless the spouse is a physician.

Our study is not without its limitations. The measure of physician specialty is not ideal. This makes it difficult to do further analysis on specialty choice, which would be a topic for future research given the findings of Gjerberg (2002, 2003). Mapping differences in physician time inputs into service provision, including quality issues, is an aspect of physician labour supply that would link our work to that of Constant and Léger's (2008). Also, if a credible source of exogenous variation could be identified, a future study could go beyond reduced form estimates such as those presented here and explore causal relationships. But, no such source is evident in our data, nor in the relevant institutional/policy framework.

Given the increasing proportion of practicing female physicians, going beyond simple differences between male and female work hours to understanding their origins is important for the design of human resource policies in the health sector. Our empirical findings imply that female physicians bear most of the time cost associated with children in spite of equal earnings potential in the labour market. Gender differences in labour supply in this context appear to be matters of choice regarding leisure and home production, and/or the influence of social norms, rather than gender discrimination in hours or gross pay -- although institutional barriers may also play a role.

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Table 1.1: Sample means by gender and census year

	1991		2006	
	Male	Female	Male	Female
Dependent Variables for Physicians				
Market hours in reference week	54.218	42.787	53.666	42.210
	(17.632)	(19.343)	(17.548)	(18.679)
Market weeks in reference year	49.273	47.458	48.655	46.552
	(4.560)	(7.190)	(4.954)	(8.278)
Part-time in reference year	0.009	0.137	0.013	0.141
	(0.094)	(0.345)	(0.114)	(0.348)
Weekly home hours in reference week (1996 and 2006)	19.857	34.853	23.654	36.481
	(19.172)	(28.242)	(21.317)	(29.289)
Unemployed or out of labour force in reference week	0.006	0.025	0.011	0.026
	(0.081)	(0.157)	(0.107)	(0.160)
Independent Variables for Physicians				
Never married (single)	0.092	0.182	0.096	0.156
Married	0.810	0.662	0.771	0.638
Common-law	0.055	0.075	0.096	0.150
Divorced/widowed/separated	0.042	0.081	0.037	0.056
No child	0.250	0.373	0.247	0.316
One child	0.129	0.181	0.133	0.164
Two children	0.318	0.275	0.338	0.317
Three or more children	0.304	0.172	0.282	0.203
General practitioner	0.632	0.685	0.486	0.561
Surgical specialist	0.075	0.034	0.080	0.048
Medical specialist	0.082	0.100	0.111	0.115
Other specialist	0.050	0.039	0.057	0.048
Specialist with specialty undeclared	0.161	0.141	0.266	0.228
Hospital is primary work setting	0.226	0.224	0.353	0.381
Office is primary work setting	0.675	0.640	0.610	0.570
Other primary work settings	0.100	0.136	0.037	0.049
Urban	0.880	0.916	0.905	0.907
Age	39.607	36.840	41.730	39.902
Activity limitation status	0.011	0.013	0.042	0.054
Minority status	0.151	0.140	0.226	0.180
Immigrant status	0.320	0.282	0.298	0.251
N	4015	1545	2910	2225

For Spouses				
Unemployed or out of labour force in reference week	0.193	0.051	0.152	0.068
Work hours if employed	26.821	45.443	27.385	44.086
Degree below bachelor among employed	0.400	0.136	0.220	0.156
Bachelor degree among employed	0.276	0.218	0.320	0.259
M.D. among employed	0.138	0.362	0.209	0.293
MA or PhD among employed	0.186	0.284	0.252	0.292
N	2800	1085	2115	1635

Notes: “Weekly home hours” is the sum of child care, housework and elder care time; home hours are not available in 1991 and are for 1996. Standard deviations for the dependent variables are presented in parentheses for physicians. The reported number of observations is rounded to 5 or 10 for data confidentiality.

Table 1.2: Hours of market work per week by family status

		1991			2006		
		Male	Female		Male	Female	
Never Married	No child	51.2	50.8		51.4	49.5	
	Presence of child<=5	N.A.	N.A.		N.A.	N.A.	
	Presence of child 6 to 24	N.A.	N.A.		N.A.	N.A.	
Married	No child	53.5	44.4	***	54.3	47.5	***
	Presence of child<=5	53.6	35.7	***	53.5	32.8	***
	Presence of child 6 to 24	55.5	41.4	***	54.9	41.0	***
Common-law	No child	50.7	41.5	***	49.3	47.8	
	Presence of child<=5	45.4	41.4		47.5	34.4	***
	Presence of child 6 to 24	55.5	39.8	**	50.1	43.5	***
Div./wid./sep.	No child	52.1	49.9		52.3	45.8	
	Presence of child<=5	N.A.	N.A.		N.A.	N.A.	
	Presence of child 6 to 24	50.8	49.7		52.5	43.4	***

Notes: N.A. indicates that the sample size in the cell is too small to generate reliable estimates (<=20).
*** indicates the sample means of males and females are significantly different at 1% and ** at 5%.

Table 1.3: Family status and labour supply

	Market hours		Home hours	Weeks	Part-time
	1	2	3	4	5
Female	-0.485	-0.658	2.369***	0.051	0.000
	(0.852)	(0.842)	(0.579)	(0.299)	(0.006)
Married	2.422***	2.130***	0.925*	1.247***	-0.027**
	(0.774)	(0.772)	(0.553)	(0.262)	(0.012)
Common-law	1.016	0.637	4.794***	0.817***	-0.011*
	(0.848)	(0.846)	(0.759)	(0.292)	(0.006)
Div./wid./sep.	0.216	0.142	12.419***	-0.270	0.013
	(1.062)	(1.054)	(1.114)	(0.360)	(0.011)
Female*married	-6.315***	-5.823***	-0.222	-0.973**	0.041**
	(1.103)	(1.091)	(0.899)	(0.397)	(0.018)
Female*common-law	-2.220*	-1.699	-5.713***	-0.808*	0.010
	(1.216)	(1.202)	(1.159)	(0.460)	(0.014)
Female*div./wid./sep.	0.073	0.426	-9.628***	0.953*	-0.014**
	(1.483)	(1.474)	(1.764)	(0.539)	(0.006)
One child	-0.169	-0.206	15.368***	0.131	-0.002
	(0.621)	(0.622)	(0.672)	(0.174)	(0.007)
Two children	0.343	0.325	17.170***	-0.141	0.000
	(0.545)	(0.545)	(0.548)	(0.167)	(0.007)
Three or more children	1.955***	1.871***	19.598***	-0.043	-0.010
	(0.561)	(0.561)	(0.604)	(0.157)	(0.006)
Female*one child	-6.636***	-6.425***	20.116***	-2.596***	0.062**
	(0.980)	(0.969)	(1.167)	(0.390)	(0.025)
Female*two children	-7.258***	-6.855***	18.713***	-0.641**	0.079***
	(0.818)	(0.809)	(0.924)	(0.307)	(0.025)
Female*three or more children	-9.901***	-9.455***	19.646***	-0.872***	0.185***
	(0.902)	(0.895)	(1.131)	(0.327)	(0.043)
Surgical specialties		8.027***	-2.098***	-0.203	-0.019***
		(0.600)	(0.741)	(0.174)	(0.003)
Medical specialties		0.410	-0.220	-0.218	-0.005*
		(0.464)	(0.590)	(0.148)	(0.003)
Other specialties		0.210	0.413	0.225	-0.009**
		(0.650)	(0.853)	(0.212)	(0.004)
Specialists with undeclared specialties		1.720***	-1.429***	0.140	-0.007***
		(0.357)	(0.441)	(0.116)	(0.002)
Office		-2.238***	0.380	0.142	0.011***
		(0.322)	(0.408)	(0.117)	(0.002)
Other work setting		-4.447***	0.777	0.688***	0.029***
		(0.582)	(0.785)	(0.192)	(0.007)
Urban		-3.064***	-0.485	0.416***	0.005
		(0.435)	(0.608)	(0.140)	(0.003)
Minority status	-1.215***	-0.874**	-0.609	-0.097	-0.004
	(0.431)	(0.428)	(0.552)	(0.146)	(0.003)
Activity limitation status	-9.172***	-8.977***	0.849	-1.763***	0.079***
	(0.934)	(0.954)	(1.125)	(0.459)	(0.016)
Immigrant status	1.181***	0.985***	-2.538***	-0.208*	-0.001
	(0.352)	(0.350)	(0.464)	(0.114)	(0.003)
N	22407	22407	16845	22407	22407
Pseudo/R^2	0.131	0.147	0.342	0.060	0.236

Notes: The reference group for specialties is general practitioners, and for work settings is hospital. Columns 1-4 are OLS and column 5 presents probit marginal effects. Other regressors include a cubic polynomial in age, and year and province effects. * indicates significance at 10% level, ** at 5%, *** at 1%. Heteroscedasticity consistent standard errors are in parenthesis.

Table 1.4: Family status and market work hours per week over time

	Male	Female
	1	2
Married	1.530	-6.852***
	(1.180)	(1.437)
Married*year96	-0.529	6.684***
	(1.780)	(1.880)
Married*year01	1.511	5.254***
	(1.666)	(1.780)
Married*year06	-0.596	4.473**
	(1.706)	(1.823)
Presence of children	1.925**	-4.767***
	(0.923)	(1.288)
Presence of children*year96	0.478	-5.445***
	(1.332)	(1.627)
Presence of children*year01	-1.039	-3.665**
	(1.272)	(1.563)
Presence of children*year06	-0.663	-3.518**
	(1.355)	(1.574)
N	14518	7889
R^2	0.062	0.122

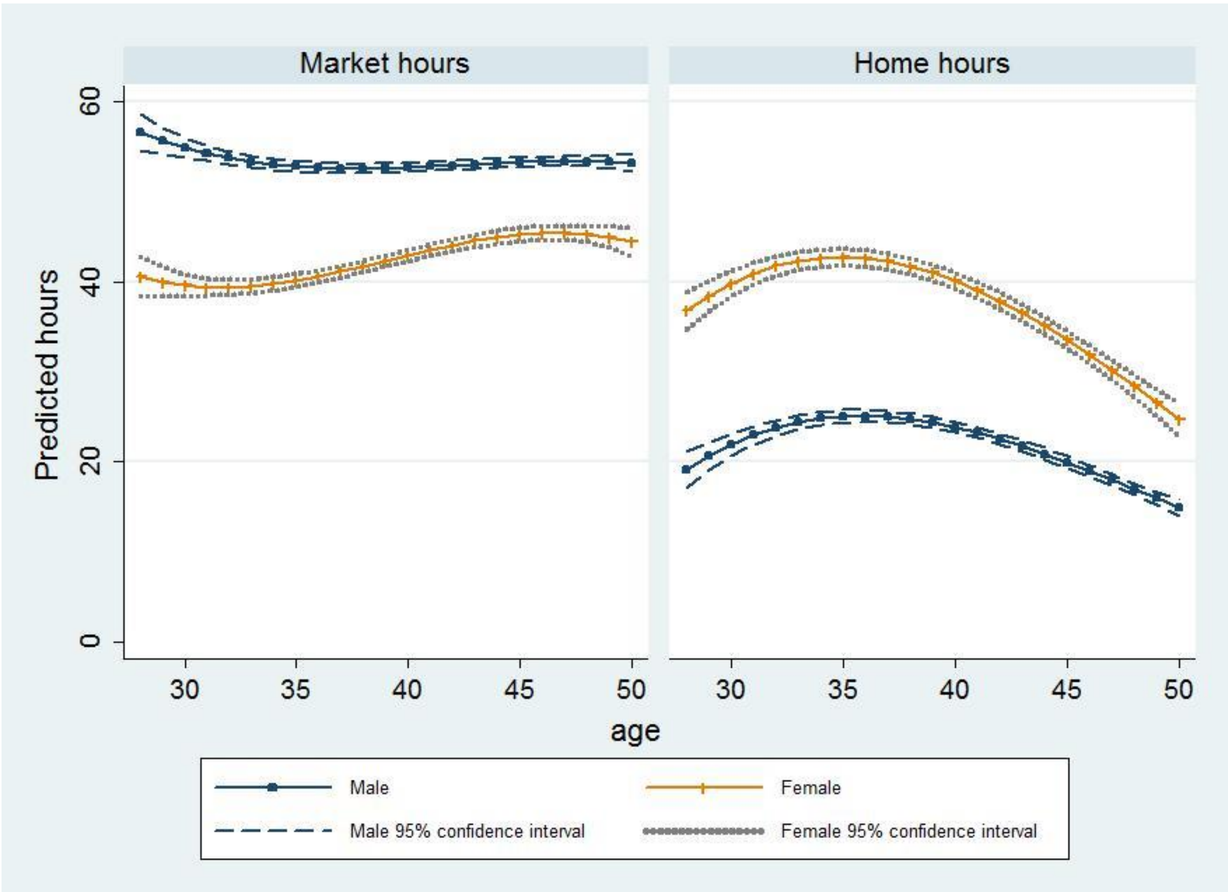
Notes: Married here includes married and common-law couples. Physician specialties and work settings and their interactions with year are also included. The interaction terms are rarely significant and are not presented to conserve space. Other independent variables include age, age^2, age^3, activity limitation, immigrant and visible minority status, year effects and province effects. * indicates significance at 10% level, ** at 5% level, *** at 1% level. Heteroscedasticity consistent standard errors are in parenthesis.

Table 1.5: Family market and home hours of work regressions

	Extensive margin	SUR		Intensity		Specialization	
	1	2	3	4	5	6	7
	Spouse unemployed or out of labour force	Male market hours	Female market hours	Market hours sum (Hus.+wife)	Home & market hours sum (Hus.+wife)	Market hours difference (Hus.-wife)	Home & market hours difference (Hus.-wife)
Male Physicians							
Common-law	-0.030**	-0.861	3.482***	2.199**	4.288**	-4.822***	-1.000
	(0.012)	(0.620)	(0.571)	(1.097)	(2.046)	(0.906)	(1.118)
One child	0.128***	0.334	-9.117***	-8.204***	50.450***	9.518***	-16.600***
	(0.018)	(0.630)	(0.579)	(1.223)	(1.980)	(0.997)	(1.173)
Two children	0.108***	1.574***	-10.302***	-8.574***	54.900***	11.370***	-17.070***
	(0.014)	(0.548)	(0.508)	(1.075)	(1.655)	(0.862)	(1.021)
Three or more children	0.118***	3.017***	-11.801***	-8.562***	65.630***	14.130***	-20.910***
	(0.015)	(0.567)	(0.530)	(1.107)	(1.782)	(0.899)	(1.054)
Spouse with bachelor degree	-0.021***	-0.517	0.476	0.485	0.088	-0.608	1.293
	(0.008)	(0.428)	(0.391)	(0.770)	(1.543)	(0.674)	(0.979)
Spouse with M.D.	-0.164***	-2.605***	14.292***	12.490***	4.843***	-17.410***	4.921***
	(0.005)	(0.505)	(0.460)	(1.019)	(1.825)	(0.755)	(0.987)
Spouse with Master or PhD	-0.035***	-0.153	3.884***	4.052***	0.668	-4.791***	2.241**
	(0.008)	(0.464)	(0.425)	(0.838)	(1.650)	(0.739)	(1.016)
N	12577	10522		7724	7724	7724	7724
R^2/Pseudo-R^2	0.074	Corr.of residuals=0.179***		0.076	0.232	0.180	0.079
Mean of dep. var.	0.163			81.304	159.538	27.116	-4.958
Female Physicians							
Common-law	0.013	-1.810***	0.376	-1.886	0.118	-2.719***	-0.908
	(0.008)	(0.666)	(0.717)	(1.222)	(2.111)	(0.988)	(1.127)
One child	-0.001	-3.577***	-9.320***	-14.270***	49.900***	6.038***	-2.258*
	(0.006)	(0.705)	(0.761)	(1.480)	(2.273)	(1.203)	(1.164)
Two children	-0.002	-1.304**	-9.565***	-12.540***	52.050***	8.301***	-2.153**
	(0.006)	(0.640)	(0.697)	(1.226)	(1.877)	(1.021)	(1.047)
Three or more children	-0.013**	-0.672	-11.180***	-13.600***	58.950***	10.330***	-1.441
	(0.006)	(0.703)	(0.770)	(1.389)	(2.354)	(1.122)	(1.174)
Spouse with bachelor degree	-0.007	0.720	-1.319*	-0.015	0.087	2.823**	0.458
	(0.005)	(0.700)	(0.751)	(1.405)	(2.436)	(1.302)	(1.313)
Spouse with M.D.	-0.074***	11.339***	-4.174***	8.025***	3.564	16.020***	3.584***
	(0.005)	(0.671)	(0.719)	(1.462)	(2.369)	(1.227)	(1.294)
Spouse with Master or PhD	-0.020***	1.916***	-1.873**	0.572	-4.645**	4.368***	-1.649
	(0.005)	(0.687)	(0.738)	(1.375)	(2.342)	(1.269)	(1.305)
N	6111	5772		4688	4688	4688	4688
R^2/Pseudo-R^2	0.120	Corr.of residuals=0.190***		0.077	0.224	0.135	0.023
Mean of dep. var.	0.055			85.377	157.995	3.949	-5.375

Notes: Variables as Table 1.3. * indicates 10%, ** 5%, *** 1%. Heteroscedasticity consistent standard errors are in parenthesis.

Figure 1.1: Predicted market and home hours for male and female physicians



Notes: Predicted market and home hours from age 28 to 50 using the models in columns 1and 2 of Appendix Table 1.8. These are the same as columns 2 and 3 of Table 1.3, but allow for interactions of the cubic age polynomial with gender. Background variables are set to their sample means.

Appendix

Appendix Table 1.6 presents the market labour supply regression results for practicing physicians 51 to 74 years of age that are akin to those of Table 1.3 in the paper. General patterns of the association between family status and labour supply for this group are similar to the younger age group. For female physicians, the magnitude of the marriage-hours effect stays the same, whereas that for motherhood (children in the household) decreases substantially for the older age group. Together with other analysis similar to that in Appendix Table 1.7, this suggests that female physicians increase their labor supply as children residing with them grow older. Interestingly, for male physicians, the magnitude of the fatherhood effect becomes larger increasing hours.

Appendix Table 1.6: Family status and labour supply (age 51 to 74)

	Market hours	Home hours	Weeks	Part-time
	1	2	3	4
Female	-1.462	4.039***	-1.564*	0.010
	(2.191)	(1.259)	(0.830)	(0.024)
Married	2.596*	0.717	0.387	-0.018
	(1.515)	(0.859)	(0.477)	(0.019)
Common-law	-0.691	3.044***	0.085	-0.007
	(1.704)	(1.144)	(0.546)	(0.018)
Div./wid./sep.	1.954	3.588***	0.080	-0.019
	(1.660)	(1.149)	(0.519)	(0.014)
Female*married	-5.859**	2.120	0.829	0.085**
	(2.312)	(1.412)	(0.868)	(0.043)
Female*common-law	0.805	-2.657	1.835*	0.031
	(2.833)	(2.120)	(1.005)	(0.051)
Female*div./wid./sep.	-2.349	-1.389	0.362	0.063
	(2.545)	(1.741)	(0.988)	(0.045)
One child	1.270**	2.221***	0.424**	-0.028***
	(0.562)	(0.455)	(0.177)	(0.007)
Two children	1.856***	5.019***	0.315*	-0.024***
	(0.606)	(0.569)	(0.192)	(0.007)
Three or more children	3.828***	8.370***	0.464**	-0.039***

	(0.729)	(0.857)	(0.189)	(0.007)
Female*one child	-2.401*	3.301***	-0.136	0.037*
	(1.319)	(1.222)	(0.605)	(0.023)
Female*two children	-1.946	2.840**	0.690	0.029
	(1.298)	(1.417)	(0.505)	(0.024)
Female*three children	-4.352**	0.269	0.225	0.119**
	(1.857)	(2.298)	(0.533)	(0.051)
Surgical specialties	2.763***	-0.247	-0.840***	-0.006
	(0.702)	(0.482)	(0.247)	(0.007)
Medical specialties	-1.238**	-0.333	-0.728***	-0.001
	(0.558)	(0.451)	(0.196)	(0.007)
Other specialties	-0.546	0.110	-0.094	0.003
	(0.764)	(0.744)	(0.323)	(0.010)
Specialists with undeclared specialties	0.363	-0.308	-0.275*	0.003
	(0.490)	(0.404)	(0.164)	(0.006)
Office	-0.320	-0.021	0.380**	0.004
	(0.465)	(0.387)	(0.188)	(0.006)
Other work setting	-3.803***	1.302*	0.460	0.030**
	(0.689)	(0.710)	(0.302)	(0.012)
Urban	-0.731	-1.476***	0.653***	-0.007
	(0.732)	(0.503)	(0.221)	(0.008)
Age	14.965*	-13.379*	-0.656	-0.351***
	(8.646)	(7.139)	(3.330)	(0.101)
Age^2	-0.216	0.209*	0.019	0.005***
	(0.142)	(0.116)	(0.055)	(0.002)
Age^3	0.001	-0.001*	-0.000	-0.000***
	(0.001)	(0.001)	(0.000)	(0.000)
Minority status	1.327**	-0.439	0.031	-0.012**
	(0.553)	(0.466)	(0.183)	(0.006)
Activity limitation status	-6.804***	1.383**	-1.584***	0.069***
	(0.766)	(0.652)	(0.313)	(0.012)
Immigrant status	1.471***	-0.180	0.253*	-0.016***
	(0.448)	(0.365)	(0.145	(0.005)
N	12347	9965	12347	12347
Pseudo/R^2	0.144	0.093	0.052	0.196

Notes: The reference group for specialties is general practitioners, and for work settings is hospital. Columns 1-3 are estimated by OLS and column 4 presents marginal effects from a probit model. Other independent variables include year and province effects. * indicates significance at 10% level, ** at 5%, *** at 1%. Heteroscedasticity consistent standard errors are in parenthesis.

Appendix Table 1.7 reports regression results analogous to Table 1.3 in the paper, but with the variables specified so that they focus on the age of the children. These results suggest that the magnitude of the negative motherhood effect depends on the age of the children. It is largest when there is a pre-school child in the household.

Appendix Table 1.7: Family status and labour supply by children's age category

	Market hours		Home hours	Weeks	Part-time
	1	2	3	4	5
Female	-0.630	-0.802	2.952***	-0.055	0.001
	(0.851)	(0.841)	(0.561)	(0.299)	(0.007)
Married	2.687***	2.445***	5.098***	1.277***	-0.030***
	(0.713)	(0.711)	(0.549)	(0.244)	(0.011)
Common-law	1.133	0.785	6.068***	0.854***	-0.012**
	(0.826)	(0.825)	(0.748)	(0.286)	(0.005)
Div./wid./sep.	0.798	0.743	11.329***	-0.055	0.005
	(1.055)	(1.047)	(1.106)	(0.360)	(0.010)
Female*married	-8.103***	-7.552***	4.112***	-1.490***	0.074***
	(1.035)	(1.023)	(0.936)	(0.373)	(0.021)
Female*common-law	-3.017**	-2.458**	-2.363**	-1.082**	0.025
	(1.187)	(1.173)	(1.150)	(0.451)	(0.018)
Female*div./wid./sep.	-2.604*	-2.218	0.936	-0.215	-0.001
	(1.462)	(1.453)	(1.825)	(0.531)	(0.011)
Presence of child <=5	-0.039	-0.193	12.647***	0.273**	-0.002
	(0.403)	(0.400)	(0.553)	(0.115)	(0.006)
Presence of child 6 to 14	1.510***	1.484***	6.738***	-0.138	-0.008*
	(0.359)	(0.358)	(0.466)	(0.091)	(0.004)
Presence of child 15 to 24	0.821*	0.759*	-0.008	0.107	-0.004
	(0.443)	(0.441)	(0.571)	(0.133)	(0.006)
Female*presence of child <=5	-7.636***	-7.419***	16.837***	-2.399***	0.054***
	(0.661)	(0.650)	(0.975)	(0.266)	(0.014)
Female*presence of child 6 to 14	-2.357***	-2.150***	7.064***	1.159***	0.034***
	(0.580)	(0.574)	(0.863)	(0.219)	(0.009)
Female*presence of child 15 to 24	-1.810**	-1.476**	2.745**	0.598**	0.004
	(0.729)	(0.726)	(1.097)	(0.250)	(0.008)
Surgical specialties		8.194***	-2.628***	-0.148	-0.020***
		(0.600)	(0.745)	(0.173)	(0.003)
Medical specialties		0.493	-0.629	-0.188	-0.006*
		(0.465)	(0.597)	(0.147)	(0.003)

Other specialties		0.160	0.417	0.208	-0.009**
		(0.646)	(0.848)	(0.212)	(0.004)
Specialists with undeclared specialties		1.746***	-1.500***	0.152	-0.007***
		(0.357)	(0.441)	(0.115)	(0.002)
Office is primary work setting		-2.222***	0.779*	0.166	0.012***
		(0.321)	(0.410)	(0.117)	(0.003)
Other primary work settings		-4.535***	1.204	0.676***	0.031***
		(0.580)	(0.774)	(0.190)	(0.008)
Urban		-3.118***	-0.201	0.402***	0.004
		(0.434)	(0.621)	(0.140)	(0.003)
Age	-4.951	-4.958	-0.840	2.723**	0.037
	(3.170)	(3.137)	(3.637)	(1.076)	(0.0259)
Age^2	0.121	0.122	0.055	-0.060**	-0.001
	(0.081)	(0.080)	(0.094)	(0.027)	(0.001)
Age^3	-0.001	-0.001	-0.001	0.000*	0.000
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Minority status	-1.285***	-0.940**	-0.381	-0.110	-0.004
	(0.430)	(0.427)	(0.553)	(0.145)	(0.003)
Activity limitation status	-9.269***	-9.071***	0.941	-1.817***	0.077***
	(0.930)	(0.950)	(1.144)	(0.457)	(0.016)
Immigrant status	1.075***	0.879**	-2.577***	-0.258**	-0.001
	(0.351)	(0.350)	(0.464)	(0.114)	(0.003)
Observations	22407	22407	16845	22407	22407
Pseudo/R^2	0.133	0.150	0.337	0.068	0.232

Notes: The reference group for specialties is general practitioners, and for work settings is hospital. Columns 1-4 are estimated by OLS and column 5 presents marginal effects from a probit model. Other independent variables include year and province effects. * indicates significance at 10% level, ** at 5%, *** at 1%. Heteroscedasticity consistent standard errors are in parenthesis.

In the main analysis as presented in Table 1.3, we have ignored the interaction effects between age and gender since they are not the primary interest of this paper. However, it is possible that the estimates may be biased without controlling for the age-gender interactions. Appendix Table 1.8 shows estimation results with these interactions. Compared with estimates in Table 1.3, there is no remarkable change in the coefficients of family status, and female family status interactions.

Appendix Table 1.8: Family status and labour supply with age interactions

	Market hours	Home hours	Weeks	Part-time
	1	2	3	4
Female	1.647*	2.255***	0.896***	0.006
	(0.955)	(0.774)	(0.331)	(0.008)
Married	2.340***	0.708	1.322***	-0.028**
	(0.774)	(0.540)	(0.263)	(0.012)
Common-law	0.874	4.594***	0.905***	-0.011**
	(0.845)	(0.746)	(0.292)	(0.005)
Div./wid./sep.	1.084	11.560***	0.106	0.007
	(1.060)	(1.116)	(0.358)	(0.010)
Female*married	-5.782***	-0.193	-0.927**	0.043**
	(1.092)	(0.908)	(0.400)	(0.018)
Female*common-law	-1.734	-5.746***	-0.790*	0.011
	(1.201)	(1.168)	(0.462)	(0.014)
Female*div./wid./sep.	-1.320	-7.880***	0.228	-0.011
	(1.503)	(1.796)	(0.545)	(0.007)
One child	0.037	15.317***	0.206	-0.002
	(0.620)	(0.673)	(0.173)	(0.007)
Two children	1.040*	16.731***	0.134	-0.001
	(0.549)	(0.551)	(0.165)	(0.007)
Three or more children	2.748***	18.990***	0.309**	-0.011*
	(0.566)	(0.610)	(0.155)	(0.006)
Female*one child	-7.086***	20.293***	-2.818***	0.063***
	(0.966)	(1.177)	(0.390)	(0.024)
Female*two children	-8.669***	19.707***	-1.343***	0.081***
	(0.832)	(0.969)	(0.311)	(0.025)
Female*three or more children	-11.887***	21.245***	-1.869***	0.194***
	(0.926)	(1.184)	(0.339)	(0.044)

Surgical specialties	8.090***	-2.102***	-0.174	-0.019***
	(0.600)	(0.742)	(0.173)	(0.003)
Medical specialties	0.384	-0.157	-0.234	-0.005
	(0.465)	(0.589)	(0.147)	(0.003)
Other specialties	0.180	0.438	0.216	-0.009**
	(0.650)	(0.856)	(0.211)	(0.004)
Specialists with undeclared specialties	1.684***	-1.398***	0.123	-0.007***
	(0.355)	(0.440)	(0.116)	(0.002)
Physician offices	-2.164***	0.334	0.173	0.011***
	(0.321)	(0.408)	(0.117)	(0.002)
Other work setting	-4.403***	0.756	0.705***	0.030***
	(0.580)	(0.777)	(0.192)	(0.007)
Age	0.089	-0.507***	-0.004	-0.001
	(0.062)	(0.078)	(0.017)	(0.001)
Age^2	0.013***	-0.053***	-0.003**	0.000
	(0.005)	(0.005)	(0.001)	(0.000)
Age^3	-0.002**	0.001	0.001***	0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Female*age	0.513***	-0.679***	0.300***	-0.001
	(0.103)	(0.138)	(0.040)	(0.001)
Female*age^2	-0.023***	-0.018*	-0.008***	-0.000***
	(0.008)	(0.011)	(0.003)	(0.000)
Female*age^3	-0.002	0.004**	-0.002***	0.000
	(0.001)	(0.002)	(0.001)	(0.000)
Urban	-3.071***	-0.466	0.417***	0.005
	(0.434)	(0.606)	(0.140)	(0.003)
Minority status	-0.874**	-0.600	-0.102	-0.004
	(0.427)	(0.549)	(0.145)	(0.003)
Activity limitation status	-9.096***	0.997	-1.820***	0.080***
	(0.953)	(1.127)	(0.458)	(0.016)
Immigrant status	0.990***	-2.514***	-0.205*	-0.001
	(0.350)	(0.462)	(0.114)	(0.003)
N	22407	16845	22407	22407
Pseudo/R^2	0.151	0.344	0.067	0.239

Notes: The reference group for specialties is GP and the reference group for work settings is hospital. Columns 1-3 are estimated by OLS and column 4 presents marginal effects from a probit model. Other independent variables include year effects and province effects. Here age is defined as the difference between age and the mean of age. * indicates significance at 10% level, ** at 5% level, *** at 1% level. Heteroscedasticity consistent standard errors are in parenthesis.

Sensitivity analyses for Table 1.3 from the paper regarding model specification.

Appendix Table 1.9: Tobit and negative binomial models (marginal effects)

	Tobit		Interval reg.	Negative binomial	
	Market hours	Weeks	Home Hours	Market hours	Weeks
	1	2	3	4	5
Female	-0.587	0.093	2.418***	-0.751	0.049
	(0.866)	(0.449)	(0.566)	(0.826)	(0.305)
Married	2.203***	2.150***	0.889*	1.953***	1.241***
	(0.793)	(0.414)	(0.536)	(0.724)	(0.261)
Common-law	0.739	1.425***	4.586***	0.578	0.832***
	(0.868)	(0.470)	(0.730)	(0.832)	(0.299)
Div./wid./sep.	0.185	-0.819	11.934***	-0.068	-0.263
	(1.083)	(0.546)	(1.092)	(1.011)	(0.362)
Female*married	-5.990***	-1.473**	-0.278	-5.546***	-0.964**
	(1.125)	(0.613)	(0.890)	1.021	(0.399)
Female*common-law	-1.843	-1.181*	-5.751***	-1.620	-0.824*
	(1.237)	(0.704)	(1.136)	(1.186)	(0.463)
Female*div./wid./sep.	0.443	1.561*	-9.150***	1.252	0.977*
	(1.516)	(0.818)	(1.804)	(1.528)	(0.556)
One child	-0.179	0.174	14.599***	-0.186	0.127
	(0.634)	(0.328)	(0.659)	(0.578)	(0.173)
Two children	0.309	-0.266	16.414***	0.167	-0.148
	(0.556)	(0.297)	(0.542)	(0.511)	(0.165)
Three or more children	1.873***	0.075	18.736***	1.518***	-0.054
	(0.572)	(0.295)	(0.598)	(0.529)	(0.155)
Female*one child	-6.796***	-3.150***	21.059***	-7.087***	-2.618***
	(1.009)	(0.591)	(1.249)	(0.906)	(0.385)
Female*two children	-6.912***	-0.624	18.961***	-7.329***	-0.638**
	(0.837)	(0.496)	(0.955)	(0.742)	(0.307)
Female*three children	-9.534***	-1.149**	19.935***	-9.478***	-0.860***
	(0.924)	(0.534)	(1.185)	(0.799)	(0.326)
Surgical specialties	7.998***	-0.033	1.825**	7.818***	-0.202
	(0.616)	(0.312)	(0.750)	(0.598)	(0.173)
Medical specialties	0.407	-0.213	-0.107	0.511	-0.218
	(0.477)	(0.257)	(0.596)	(0.470)	(0.147)
Other specialties	0.200	0.566	0.736	0.546	0.224
	(0.666)	(0.376)	(0.879)	(0.660)	(0.211)
Specialists with undeclared specialties	1.716***	0.532***	-1.246***	1.876***	0.140
	(0.366)	(0.207)	(0.441)	(0.362)	(0.116)
Office	-2.250***	-0.502***	0.441	-2.529***	0.142

	(0.330)	(0.195)	(0.408)	(0.325)	(0.117)
Other work setting	-4.555***	1.979***	0.791	-4.855***	0.691***
	(0.604)	(0.361)	(0.794)	(0.557)	(0.194)
Urban	-3.076***	0.792***	-0.716	-3.134***	0.415***
	(0.446)	(0.234)	(0.609)	(0.448)	(0.140)
Age	-10.627***	2.007	15.856***	-10.812***	1.241
	(3.181)	(1.700)	(3.469)	(3.073)	(1.081)
Age^2	0.272***	-0.037	-0.362***	0.278***	-0.021
	(0.081)	(0.043)	(0.089)	(0.078)	(0.027)
Age^3	-0.002***	0.000	0.003***	-0.002***	0.000
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)
Minority status	-0.919**	-0.126	-0.496	-0.738*	-0.097
	(0.442)	(0.243)	(0.561)	(0.422)	(0.146)
Activity limitation status	-9.449***	-1.998***	0.975	-8.890***	-1.760***
	(1.015)	(0.652)	(1.140)	(0.956)	(0.458)
Immigrant status	1.042***	-0.426**	-2.478***	0.996***	-0.207*
	(0.359)	(0.196)	(0.469)	(0.349)	(0.114)
N	22407	22407	16845	22407	22407

Notes: The reference group for specialties is GP and the reference group for work settings is hospital. Columns 1 & 2 present marginal effects (at the mean) from tobit models. Column 3 presents interval regression results where interval is defined maximum and minimum of home hours. Columns 4 & 5 present marginal effects (at the mean) from negative binomial models. Other independent variables include year effects and province effects. * indicates significance at 10% level, ** at 5% level, *** at 1% level. Heteroscedasticity consistent standard errors are in parenthesis.

Appendix Tables 1.10 and 1.11 are explorations of the effect of the difference between the age of a physician and that of her/his spouse, with Appendix Table 1.11 being easier to interpret although it is a less flexible specification. While the age difference is associated with labour supply, it has little effect on the marriage and children variables. For Appendix Table 1.10 the age gap is specified as the difference between the age of the physician and his/her spouse and it has an interesting relationship with labour supply as seen in Appendix Table 1.10 (see also Appendix Table 1.11).

Appendix Table 1.10: Labour supply and spouse age

	Market hours	Home hours	Weeks	Part-time
	1	2	3	4
Female	-6.383***	3.992***	-0.793***	0.030***
	(0.800)	(0.688)	(0.286)	(0.010)
Physician age-spouse age	-0.090*	0.316***	-0.055***	0.001
	(0.054)	(0.074)	(0.014)	(0.001)
(Physician age-spouse age)^2	-0.009**	0.011**	-0.002	0.000
	(0.004)	(0.006)	(0.001)	(0.000)
(Physician age-spouse age)^3	0.000	-0.000	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Female *(physician age-spouse age)	-0.075	-0.011	0.059*	0.000
	(0.089)	(0.135)	(0.034)	(0.001)
Female*(physician age-spouse age)^2	0.023***	-0.007	0.003	-0.000
	(0.008)	(0.013)	(0.003)	(0.000)
Female*(physician age-spouse age)^3	0.001**	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Common-law	-1.230**	3.666***	-0.386**	0.008
	(0.608)	(0.796)	(0.197)	(0.009)
Female*common-law	3.476***	-5.450***	0.015	-0.011**
	(0.880)	(1.184)	(0.375)	(0.005)
One child	-0.108	16.058***	0.117	-0.006
	(0.650)	(0.684)	(0.180)	(0.006)
Two children	0.240	18.046***	-0.167	-0.004
	(0.576)	(0.567)	(0.159)	(0.006)
Three or more children	1.670***	20.711***	-0.114	-0.013**

	(0.590)	(0.627)	(0.157)	(0.006)
Female*one child	-7.885***	18.273***	-2.895***	0.079***
	(1.064)	(1.197)	(0.421)	(0.029)
Female*two children	-7.837***	17.801***	-0.937***	0.097***
	(0.870)	(0.936)	(0.323)	(0.026)
Female*three or more children	-10.355***	19.032***	-1.148***	0.215***
	(0.940)	(1.155)	(0.347)	(0.045)
Surgical specialties	7.991***	-2.637***	-0.282	-0.017***
	(0.638)	(0.825)	(0.183)	(0.003)
Medical specialties	0.610	0.013	-0.207	-0.008***
	(0.502)	(0.672)	(0.156)	(0.003)
Other specialties	-0.005	0.412	0.294	-0.010***
	(0.703)	(0.994)	(0.224)	(0.004)
Specialists with undeclared specialties	1.576***	-1.480***	0.117	-0.007***
	(0.384)	(0.499)	(0.120)	(0.002)
Physician offices	-1.866***	0.465	0.148	0.009***
	(0.352)	(0.468)	(0.123)	(0.002)
Other work setting	-4.402***	0.493	0.660***	0.030***
	(0.648)	(0.922)	(0.206)	(0.008)
Urban	-2.692***	-0.231	0.405***	0.005**
	(0.464)	(0.682)	(0.149)	(0.003)
Age	-9.320***	19.197***	1.930*	0.040
	(3.516)	(4.217)	(1.154)	(0.026)
Age^2	0.240***	-0.455***	-0.037	-0.001
	(0.089)	(0.108)	(0.029)	(0.001)
Age^3	-0.002***	0.003***	0.000	0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Minority status	-0.828*	-0.709	-0.206	-0.005*
	(0.475)	(0.658)	(0.156)	(0.003)
Activity limitation status	-7.813***	0.073	-0.963**	0.051***
	(1.069)	(1.322)	(0.448)	(0.014)
Immigrant status	1.123***	-2.856***	-0.052	-0.001
	(0.381)	(0.535)	(0.117)	(0.002)
N	18688	14074	18688	18688
Pseudo/R^2	0.169	0.309	0.065	0.268

Notes: The reference group for specialties is general practitioners, and for work settings is hospital. Columns 1-3 are estimated by OLS and column 4 presents marginal effects from a probit model. Other independent variables include year and province effects. * indicates significance at 10% level, ** at 5%, *** at 1%. Heteroscedasticity consistent standard errors are in parenthesis.

To facilitate interpretation, for Appendix Table 1.11, the age gap is defined to be positive in all cases. It is defined as spouse's age minus physician's age if the spouse is older with this variable set to zero when the spouse is younger, and the converse for younger spouses. It is also restricted to be linear to facilitate a general understanding of the slope. Controlling for other covariates, male physicians work fewer hours if married to a younger spouse, but their work hours are not affected when married to an older one. For female physicians the sum of the two coefficients is the total effect and they work more hours if they marry an older spouse (F-test statistically significant at the 1% level), and there is no net effect on hours if married to a younger spouse (sum of the two coefficients is not statistically different from zero).

Appendix Table 1.11: Spouse's age and physician market hours

	Market hours
	1
Female	-6.727***
	(0.856)
(Spouse age - physician age) if spouse older	-0.081
	(0.110)
Female*(Spouse age - physician age) if sp older	0.298**
	(0.126)
(Physician age - spouse age) if spouse younger	-0.217***
	(0.058)
Female*(Physician age - spouse age) if sp younger	0.220
	(0.177)
Common-law	-1.182*
	(0.608)
Female*common-law	3.474***
	(0.880)
One child	-0.129
	(0.651)
Two children	0.204
	(0.579)
Three or more children	1.618***
	(0.594)
Female*one child	-7.820***
	(1.064)
Female*two children	-7.760***
	(0.874)
Female*three or more children	-10.264***

	(0.946)
Surgical specialties	7.983***
	(0.638)
Medical specialties	0.609
	(0.502)
Other specialties	-0.006
	(0.703)
Specialists with undeclared specialties	1.578***
	(0.384)
Office	-1.861***
	(0.352)
Other work setting	-4.399***
	(0.647)
Urban	-2.703***
	(0.464)
Age	-9.337***
	(3.516)
Age squared	0.241***
	(0.089)
Age cubed	-0.002***
	(0.001)
Minority status	-0.811*
	(0.475)
Activity limitation status	-7.803***
	(1.069)
Immigrant status	1.150***
	(0.382)
N	18688
R^2	0.169

Notes: The reference group for specialties is general practitioners, and for work settings is hospital. Other independent variables include year and province effects. * indicates significance at 10% level, ** at 5%, *** at 1%. Heteroscedasticity consistent standard errors are in parenthesis.

Alternative samples and specifications for Table 1.5 in the text are presented in Appendix Table 1.12.

Appendix Table 1.12: Family market and home hours of work regressions by family types

	Extensive margin	SUR		Intensity		Specialization	
	1	2	3	4	5	6	7
Male physician family	Spouse unemployed or out of labour force	Male phy. market hours	Spouse market hours	Market hours sum (Hus.+wife)	Home and market hours sum (Hus.+wife)	Market hours difference (Hus.-wife)	Home and market hours difference (Hus.-wife)
Common-law	-0.039***	-0.666	3.879***	3.188***	3.982*	-5.270***	-1.134
	(0.015)	(0.690)	(0.611)	(1.206)	(2.333)	(1.054)	(1.319)
One child	0.143***	1.300*	-8.926***	-6.890***	51.007***	10.360***	-18.424***
	(0.021)	(0.690)	(0.610)	(1.290)	(2.207)	(1.112)	(1.331)
Two children	0.124***	2.219***	-10.026***	-7.006***	55.564***	11.455***	-19.010***
	(0.016)	(0.599)	(0.534)	(1.138)	(1.854)	(0.961)	(1.149)
Three or more children	0.137***	3.902***	-11.451***	-6.594***	67.384***	14.265***	-23.518***
	(0.018)	(0.618)	(0.554)	(1.173)	(1.971)	(1.000)	(1.177)
Spouse with bachelor degree	-0.025***	-0.563	0.425	0.314	-0.160	-0.622	1.300
	(0.009)	(0.429)	(0.377)	(0.768)	(1.546)	(0.676)	(0.981)
Spouse with Master or PhD	-0.041***	-0.149	3.903***	4.044***	0.663	-4.778***	2.117**
	(0.010)	(0.465)	(0.410)	(0.837)	(1.659)	(0.740)	(1.020)
N	10736	8723		6303	6303	6303	6303
Pseudo/R^2	0.036	Corr.of residuals=0.134***		0.053	0.244	0.122	0.093

Appendix Table 1.12 (con't): Family market and home hours of work regressions by family types

Female physician family	Spouse unemployed or out of labour force	Spouse market hours	Female phy. market hours	Market hours sum (Hus.+wife)	Home and market hours sum (Hus.+wife)	Market hours difference (Hus.-wife)	Home and market hours difference (Hus.-wife)
Common-law	0.020	-1.757**	0.286	-1.733	-0.636	-2.518**	-0.945
	(0.014)	(0.753)	(0.822)	(1.402)	(2.440)	(1.204)	(1.376)
One child	0.000	-3.642***	-9.137***	-14.755***	49.788***	5.772***	-1.103
	(0.013)	(0.789)	(0.863)	(1.616)	(2.617)	(1.431)	(1.362)
Two children	-0.005	-1.224*	-8.868***	-12.062***	52.637***	7.558***	-1.240
	(0.012)	(0.723)	(0.798)	(1.321)	(2.175)	(1.218)	(1.253)
Three or more children	-0.027**	-0.848	-9.466***	-12.489***	59.624***	8.066***	-0.308
	(0.013)	(0.821)	(0.907)	(1.556)	(2.873)	(1.393)	(1.455)
Spouse with bachelor degree	-0.014	0.733	-1.419*	-0.108	-0.304	2.924**	0.482
	(0.010)	(0.680)	(0.740)	(1.395)	(2.428)	(1.295)	(1.317)
Spouse with Master or PhD	-0.044***	2.107***	-1.901***	0.666	-4.969**	4.656***	-1.536
	(0.010)	(0.671)	(0.730)	(1.370)	(2.353)	(1.261)	(1.313)
N	4170	3838		3138	3138	3138	3138
Pseudo/R^2	0.056	Corr.of residuals=0.091***		0.070	0.237	0.049	0.021

Appendix Table 1.12 (con't): Family market and home hours of work regressions by family types

Physician-physician family	Male phy. market hours	Female phy. market hours	Market hours sum (Hus.+wife)	Home and market hours sum (Hus.+wife)	Market hours difference (Hus.-wife)	Home and market hours difference (Hus.-wife)
Common-law	-1.754	1.570	-1.669	4.550	-3.132*	-0.528
	(1.413)	(1.508)	(2.569)	(4.255)	(1.696)	(1.804)
One child	-4.415***	-9.449***	-13.666***	49.060***	5.162**	-7.903***
	(1.535)	(1.643)	(3.478)	(4.483)	(2.219)	(2.310)
Two children	-1.867	-11.617***	-15.756***	50.914***	10.225***	-6.655***
	(1.356)	(1.477)	(3.020)	(3.614)	(1.955)	(2.160)
Three or more children	-1.567	-13.888***	-17.978***	56.886***	13.103***	-6.911***
	(1.427)	(1.581)	(3.146)	(4.186)	(2.037)	(2.267)
N	1799		1421	1421	1421	1421
R^2	Corr.of residuals=0.354***		0.086	0.192	0.119	0.028

Notes: Variables as Table 1.3. * indicates 10%, ** 5%, *** 1%. Heteroscedasticity consistent standard errors are in parenthesis.

Chapter 2

Mandatory Universal Drug Plan, Access to Health Care and Health: Evidence from Canada

2.1 Introduction

Unlike physician and hospital services in Canada, coverage for prescription drugs dispensed outside hospitals falls outside the Canada Health Act. Yet pharmaceutical drugs play an increasingly important role in treating many health conditions. This increased role of drugs among health care treatments is reflected in expenditure. The share of drug expenditure in total health expenditure increased from 9.5% in 1985 to 16.3% in 2010 (Canadian Institute for Health Information 2011). Among OECD countries, Canada had the second-highest level of total drug expenditure per capita after the United States in 2008 (Canadian Institute for Health Information 2011). Canada's provincial governments provide public drug programs for some population groups, primarily seniors and social assistance recipients. Most non-elderly Canadians who have drug insurance obtain it through employee benefit plans. However, a significant number of Canadians are un-insured or under-insured against the costs of prescription drugs (Applied Management 2000; Kapur and Basu 2005). About one in ten Canadians who receive a prescription report cost-related non-adherence, and the lack of drug insurance coverage appears to be a key reason behind this phenomenon (Law et al. 2012).

The rising financial burden associated with prescription drugs has raised concerns about Canadians falling through the gaps in the patchwork of public and private plans. A number of recommendations for a national pharmacare program have been proposed, such as expanding the first-dollar universal coverage to include prescription drugs (National Forum on Health 1997) or protecting Canadians against catastrophic drug expenses through a catastrophic drug plan (Romanow Commission on the Future of Health Care in Canada 2002; Standing Senate Committee on Social Affairs, Science and Technology 2002). The national pharmaceuticals strategy report calls for further analysis on the impact and feasibility of maintaining a private payer role in a catastrophic drug coverage framework (Federal/Provincial/Territorial Ministerial Task Force 2006). In the United States, the growing financial burden of prescription drug expenditures by the elderly led the federal government to add the Part D drug benefit to Medicare (The Medicare Prescription Drug, Improvement, and Modernization Act of 2003). Beyond North America, many OECD countries, such as Australia, New Zealand, United Kingdom, France and Sweden, provide universal coverage for prescription drugs (Gagnon 2010).

While many Canadian citizens and politicians believe that some form of universal drug coverage is needed, empirical evidence to inform the design of such a policy in the

Canadian context is limited.[1] In this study, we exploit a policy change in the province of Quebec, Canada to evaluate whether and how a mandatory universal drug program affects drug insurance coverage, health care utilization and health outcomes of the general population. In 1997, Quebec introduced a mandatory, universal drug program using a mixed public and private design. Under the program, those eligible for private insurance must obtain coverage privately; anyone who is not eligible for private coverage must obtain public insurance from the government. An attractive feature of this mixed public and private scheme is that it can be introduced without dramatically restructuring the current drug financing system that exists in most provinces. Quebec's experience can provide valuable insights to inform policy debates regarding national drug coverage in Canada.

Using Canadian National Population Health Survey longitudinal data from 1994 to 2003, we find that the universal drug program increased drug insurance coverage by 33 percent. The program also increased medication use by 13 percent and general practitioner visits by 11 to 13 percent, but it had no statistically significant effect on specialist visits and hospitalization. The policy generated positive health gains concentrated among less healthy individuals. Analysis by pre-policy drug insurance status reveals that previously uninsured individuals experienced a large increase in the probability of taking any medication and visiting a general practitioner. Further analysis by the presence of chronic conditions shows that, compared to those without any chronic condition, the chronically ill experienced a much larger increase in the extensive margins

[1] There are some studies focusing on the distributional effects of provincial drug programs on out of pocket drug expenditure (e.g., Alan et al. 2005), or on predicting prescription drug expenditure (e.g., Fraser Group and Tristat Resources 2002; Demers et al. 2008). However, none of them explores the utilization and health impact of the drug programs.

of medication and general practitioner utilization, and a larger improvement in health outcomes.

The remainder of the paper is organized as follows. In section 2 we provide background on previous work and the policy change we are studying. In section 3, we discuss our data and descriptive statistics. Empirical methods are discussed in section 4. Section 5 gives the results of our analysis. Finally the conclusions are drawn in section 6.

2.2 Background

2.2.1 Previous Work

There is a rich literature examining the effects of drug insurance/health insurance on utilization. It is well documented that individuals without insurance have lower levels of utilization than those with insurance; further, among individuals with insurance, those who face cost-sharing have lower utilization than those with full insurance. Zweifel and Manning (2000) and McWilliams (2009) review the evidence with respect to health insurance in general; Lexchin and Grootendorst (2004) and Goldman et al. (2007) review the evidence with respect to drug insurance. The RAND Health Insurance Experiment estimated the price elasticity of demand for health care to be in the order of -0.2 (Newhouse 1993). Estimates of the price elasticity of prescription drugs vary from -0.02 to -0.80 (Contoyannis et al. 2005; Gemmill et al. 2007). Closely related to our work are some recent U.S. studies that attempt to estimate the effects of the introduction of Medicare Part D on drug utilization.[2] For the elderly, Medicare Part D has been found to increase monthly drug use by 6 to 13 percent (Yin et al. 2008; Lichtenberg and Sun 2007;

[2] A comparison of the Quebec universal drug program and the Medicare Part D can be found in Pomey et al. 2007.

Liu et al. 2011) or 11 to 37 percent among the previously uninsured (Kaestner and Kan 2010; Schneeweiss et al. 2009). However, studies of Medicare Part D focus on the elderly and may not generalize to the rest of the population. In addition, enrolment in Medicare Part D is not mandatory while our study focuses on a policy design that enforces mandatory insurance coverage through legislation.

Drug insurance expansion may also generate spillover effects on non-drug health care service utilization. If prescription drugs and non-drug services are substitutable (complementary), increased access to drug insurance will lead to less (more) use of the non-drug services. Therefore, optimal drug insurance design should also take into account how changes in drug use might affect overall health service utilization and costs (Newhouse 2006). Such spillover effects in the broader context of free physician and hospital services have received less attention in the Canadian pharmacare policy discussion. A small number of studies have investigated the spillover effects of drug insurance on physician services with mixed findings. Some evidence suggests a complementary relationship (Stabile 2001; Allin and Hurley 2009; Winkelmann 2004) while other evidence points to a substitutive one (Li et al. 2007). Existing studies also demonstrate that higher cost-sharing or a copayment for prescription drugs is associated with greater use of inpatient and emergency medical services (Tamblyn et al. 2001; Hsu et al. 2006; Chandra et al. 2010). This study explores the spillover effects of drug insurance expansion in a public health system where physician and hospital services are free to patients, which is the case most relevant to Canada.

Finally, comparatively few studies have examined the effects of prescription drug insurance on health status and functioning.[3] If drug insurance causes an increase in drug and non-drug service utilization, the health impact depends on whether the increase in utilization is for effective high-value services to the patient. The existing literature on the link between drug insurance/cost sharing and health shows higher cost sharing is associated with more adverse events among the chronically ill, however, evidence on general health outcomes is rather limited (Goldman et al. 2007). The recently proposed Value-Based Insurance Design advocates that copayment rates be set based on the expected health value of the clinical services, so as to mitigate the adverse health consequences associated with cost-sharing for services or drugs (e.g., Fendrick and Chernew 2006). By reducing patient copayments for high-value services, the value-based insurance plans can achieve improved health outcomes for a given level of health care expenditure. The current evidence suggests that reducing copayments can improve medication compliance among the chronically ill (e.g., Choudhry et al. 2010; Maciejewski et al. 2010). If the mandatory drug program increases access to effective prescription drugs or physician services of high value to the beneficiaries, it may generate positive health gains. On the other hand, if the increased drug or physician utilization is not effective treatment, it may not have any health impact.

2.2.2 The Institutional Setting

Outpatient prescription drugs in Canada are covered by a mix of public and private plans. The federal government administers prescription drug programs for six

[3] For the impact of general health insurance on health outcomes, recent studies have found consistently positive and often statistically significant effects across a range of outcomes (see McWilliams 2009 for a recent review of this literature).

specific population groups.[4] Provincial and territorial governments provide public drug insurance to defined population subgroups, mainly for social assistance recipients, senior citizens, and residents with specific diseases that require high-cost prescription drugs.[5] Eligibility and cost sharing arrangements in these public plans vary across provinces, and there have been some moves to decrease out-of-pocket payment in the past decade (Daw and Morgan 2011).

Drug insurance coverage for the general population, i.e. the non-elderly not on social assistance, varies across provinces. Approximately 90% of those with private insurance obtain it through group plans associated with their employer, union or professional association (Hurley and Guindon 2011). Public drug programs for the general population also exist in some provinces, but with high deductibles they only provide protection against catastrophic drug expenses. A detailed description of these programs can be found in Grootendorst (2002), Daw and Morgan (2011), and Phillips (2009). During the period of our study there were minimal changes of drug insurance policies for the general population in provinces other than Quebec.[6]

Drug insurance in Quebec before 1997 reflected these general patterns: the public drug program covered welfare recipients, citizens aged 65 and over, and patients with certain serious illnesses. The public plan fully covered drug costs for low-income seniors and all welfare recipients; medium- and high-income seniors were subject to a $2

[4]These programs are: (1) First Nations, Inuit, and Innu people; (2) members of the Department of National Defence; (3) some veterans and their families through Veterans Affairs Canada; (4) members of the RCMP; (5) some incarcerated individuals in federal correctional facilities; and (6) some individuals eligible through Citizenship and Immigration.

[5] For example, the Special Drug Programs in Ontario covers persons with cystic fibrosis, HIV, renal disease and schizophrenia, and other specific conditions. The Special Beneficiary Drug Coverage in Saskatchewan covers persons under the paraplegic, cystic fibrosis, and chronic end-stage renal disease programs.

[6] The only meaningful changes were that Ontario introduced the Trillium Drug Program in 1995, which is a catastrophic drug plan with a deductible amount equal to 4% of the household's total net income, and Manitoba changed its catastrophic plan from a fixed dollar amount to a percentage of income in 1996.

copayment per prescription up to a maximum of $100 per year. There was no public coverage for the rest of the population, who relied on private insurance or paying out-of-pocket. The Act Respecting Prescription Drug Insurance was passed in 1996 and went into effect in two stages (Gazette Officielle Du Quebec 1996). The policy reform started with increased user fees for previously insured welfare recipients and senior citizens in August 1996. The mandatory universal drug program for every resident in Quebec was implemented on 1 January 1997. Under this program, all residents of Quebec were required by law to have drug insurance coverage. The public prescription drug insurance plan, administered by the Régie de l'assurance maladie du Québec (RAMQ), covers all Quebecers who are not eligible for a private plan. All persons under age 65 eligible for a private plan are required to join that plan and ensure coverage for their spouse and children. [7] Private plans are usually available through employment or through membership in professional associations and unions. Coverage varies across private plans but all private insurers are required to provide minimum coverage standards that are equivalent to those offered by the provincial public plan. Persons who are eligible for a private plan cannot be covered by the public plan. Table 2.1 displays the policy changes for the elderly, the welfare recipients and the general population during our study period.

The public plan charges an income-dependent premium that is collected by the Ministère du Revenu du Québec (now known as Revenu Québec) through the income tax system. In addition, the public plan includes a monthly deductible and coinsurance payment. Currently, the annual premium of the public plan varies from $0 to $579

[7] Persons who turn 65 may either retain their private plan or join the public plan.

depending on net family income, the monthly deductible is $16.25, and the coinsurance is 32% up to a maximum out-of-pocket expenditure per month of $82.66.

The Act also stipulates that "no group insurance or employee benefit plan providing coverage for accident, illness or invalidity may be established unless it also includes coverage for pharmaceutical services and medications at least equal to the coverage under the basic plan" (Gazette Officielle Du Quebec 1996). This provision prevents employers from dropping only the pharmaceutical component of their extended health benefit packages. This may affect other supplemental health insurance coverage. If employers find it costly to bundle a drug plan with other health plans, they may drop the other plans or even drop all health plans from their benefit packages.

2.3 Data and descriptive statistics

2.3.1 Data

The main data set for this analysis is the master files of the longitudinal household component of the National Population Health Survey (NPHS), a biennial nationally representative survey conducted by Statistics Canada.[8] The target population of the NPHS includes community-based household residents in the ten provinces, excluding populations on Indian Reserves, Canadian Forces Bases and some remote areas in Quebec and Ontario. The sample was created by first selecting households and then within each household, choosing one member 12 years of age or older to be the

[8] The NPHS was composed of three parts: the survey of households, health institutions and the North. The institutional component surveyed long-term residents in health care facilities; the north component surveyed household residents in Yukon and the Northwest Territories. The first three cycles of the NPHS (1994/1995, 1996/1997 and 1998/1999) are both cross-sectional and longitudinal. Beginning in Cycle 4 (2000/2001), the survey became strictly longitudinal and the survey of the North was conducted by Canadian Community Health Survey.

longitudinal respondent. It also includes 2,022 persons who were under the age of 12 in the first cycle and previously interviewed as part of the 1994/1995 National Longitudinal Survey of Children and Youth (NLSCY).[9] Therefore, the NPHS longitudinal sample includes 17,276 persons from all ages in 1994/1995. The NPHS asks a series of questions related to health status, use of health services, chronic conditions and activity restrictions, and demographic and socio-economic status.

We use the 1994/1995, 1996/1997, 1998/1999, 2000/2001 and 2002/2003 cycles of NPHS for our analysis. Of the 17,276 people who were first interviewed in 1994/1995, we excluded 2,022 people who were younger than 12 years old because most of the outcome variables are only available for respondents aged 12 and older. We dropped 4,086 people older than 56 in the first cycle since the policy change for those aged 65 and over was different from the rest of the population. This resulted in a sample of 11,168 people aged 12 to 56 in 1994/1995, who were followed for up to eight years. The data were then re-shaped as person-year data containing 55,840 observations (11,168 times five cycles). We further deleted 3,824 observations reporting welfare benefits as their income source in any of the survey cycles since the reform had different implications for this previously covered group. We further eliminated individuals missing important health care utilization information and ended up with 42,609 observations.[10] We discarded 2,255 observations in 1997, which was a transition year, from this analysis.[11] The final data set consists of an unbalanced, non-contiguous panel of 10,623 people

[9] This sample of children was administered the NLSCY questionnaire in 1994–1995 and included in the NPHS sample from 1996/1997.
[10] The sample is lost mostly because of longitudinal attrition. About 80% of the individuals in our sample provided a full response to all five cycles of NPHS. In one robustness check, estimation results are very similar when we use the full-response sample.
[11] Another reason for deleting these observations is to consistently measure annual physician utilization before and after the policy change.

(40,354 person-year observations), who were non-elderly not on social assistance and between the ages of 12 and 56 in 1994/1995 (20 to 64 at their last interview in 2002/2003).

2.3.2 Outcome Variables

Our measure of prescription drug insurance coverage is self-reported: the survey asked each respondent if s/he had insurance that covered all or part of the cost of prescription medications, including any private, government or employer-paid insurance plans. Drug insurance status is an indicator variable coded as one if a person reported having drug insurance and zero otherwise. The survey similarly asked about insurance that covered the costs of eye-glasses, semi-private/private hospital room charges and dental expenses. A binary variable is created for each type of supplemental insurance. We created a composite binary variable indicating whether an individual has any non-drug insurance. This will enable us to examine the overall effect of the drug insurance policy on other supplemental insurance coverage.

Measures of health care utilization include drug utilization, physician visits and hospitalizations. We construct two measures of drug utilization. The first is the number of distinct medications taken in the previous month. The survey asked a series of questions on whether the respondent took a specific medication in the previous month. The number of medications is the sum of the different types of medications. Among respondents who took any medications in the previous month, the survey further asked how many different medications s/he took during the previous two days. Hence, we also include a measure of number of distinct drugs taken in the previous two days. Note that these questions refer to both prescription and non-prescription drugs. Ideally we would separate these two, but separate measures are not available in the pre-policy cycles of the data. If the policy induces individuals to substitute prescription drugs for non-prescription drugs, our estimates would measure the net effects on total medication use and underestimate the impact of the policy on prescription drug use. We measure general practitioner (GP) visits and specialist visits separately as the number of visits with each type of physician in the previous 12 months. Finally, all of the medication and physician utilization variables were truncated at their 99.5 percentile to eliminate outliers.[12] The policy may have an effect on the extensive margin of utilization if a lack of drug insurance inhibits the patient from taking medication or visiting a doctor. Therefore, for all medication and physician utilization measures, we further look at the extensive margin of utilization: whether a respondent had at least one medication or physician visit. Hospitalization measures whether a respondent had an overnight inpatient hospital stay in the previous 12 months.

Health outcome measures are the health utility index (HUI3) and self-assessed health status. The Health Utility Index Mark 3 (HUI3) is a generic measure of health status and health-related quality of life that is widely used in clinical studies, population health surveys and economic evaluations (Horsman et al. 2003). It is a function of eight attributes that can vary from no impairment to severely impaired: vision, hearing, speech, mobility, dexterity, feelings, cognition and pain. For the HUI, 1.0 represents perfect health and 0.0 represents death. The observed values range from -0.360 to 1.000 in increments of 0.001 (negative scores reflect health states considered worse than death)

[12] There may be outliers in the self-reported medication use or physician visits. For example, there are a few respondents reporting more than 300 GP visits in the previous 12 months. The truncation made the summary statistics in Table 2.2 a bit smaller than those from the original data. This truncation also slightly underestimated the policy effects shown in Table 2.4.

(Furlong et al. 1999). For self-assessed health status, we create a dichotomous variable: excellent or very good health is coded as 1 and good, fair, or poor health is coded as 0. The use of self-assessed health status as a measure of health is common in the empirical literature and it has been found to be an independent predictor of mortality and morbidity (Idler and Benyamini 1997).[13]

2.3.3 Descriptive Statistics

Figures 2.1 to 2.7 depict trends in prescription drug coverage, selected measures of medication utilization and other health care utilization, and health status among the general population aged 12 to 64 in Quebec compared to the rest of Canada. Figure 2.1 shows that there was an increase in self-reported prescription drug insurance coverage in all provinces from 1996 to 2003. It is apparent, however, that Quebec started with a lower baseline level of coverage than the other provinces, and that right after the 1997 reform it experienced a dramatic increase in coverage from 63 percent to 86 percent. Insurance coverage in Quebec continued to rise in the years after the policy change, reaching 93 percent in 2002/2003. The less than 100 percent coverage is possibly due to bias in self-reported insurance status, gaps between public and private plans, or failure to register for the public plan.

Figures 2.2 and 2.3 show the trends of medication use in the previous month. For the proportion of those taking at least one medication in the previous month, a distinct trend break in Quebec starting in 1998 is observed. The trend for number of medications taken in the previous month in Quebec is only slightly different from the rest of Canada, although the level is somewhat lower in Quebec. There is little difference between the

[13] See Crossley and Kennedy (2002) for an assessment of the reliability of self-assessed health status.

trends of the proportion of people with at least one GP visit between Quebec and the rest of Canada (Figure 2.4). The total number of GP visits in Quebec increased starting 1998/1999 while it did not increase in the other provinces until the last cycle of our data (Figure 2.5). Figures 2.6 and 2.7 display the trends of health status measured by HUI and self-assessed health status. The time series of both health measures show similar trends in Quebec and the rest of Canada. Although the figures depict raw differences without taking into account observed or unobserved heterogeneity, they nevertheless suggest some evidence of a trend break in Quebec after the policy change in 1997.

Table 2.2 presents the summary statistics of our sample in Quebec and the rest of Canada before and after the policy change. The Quebec general population is similar to the rest of Canada in terms of age, gender and marital status. There are no remarkable differences in Quebec relative to the rest of Canada in education and income levels.[14] A larger increase in medication use, GP visits and HUI is observed in Quebec after the policy change.

2.4 Empirical Strategy

We use a difference-in-differences (DD) approach to compare outcomes in Quebec versus other Canadian provinces. The pre-policy period includes the 1994/1995 and 1996/1997 cycles of the NPHS data and the post-policy period includes the three cycles from 1998/1999 to 2002/2003. The treatment group is the general population in Quebec before and after the policy change and the comparison group is the general population in the other provinces of Canada. Given the similar institutional contexts across provinces

[14] A panel of individuals aged 12 to 56 were followed over time so an upward shift in income and education levels were observed in the sample.

within the Canadian health care system, the population in other provinces can serve as a valid comparison group for the Quebec population. We employ the following equation to estimate the impact of the reform on insurance coverage, health care utilization and health outcomes:

$$y_{ipt} = f(\alpha + \delta_1 QB_i + \delta_2 Post_t + \beta(Post * QB)_{it} + \delta_3 Otherprov_p + \varphi Year_t + \gamma X_{ipt} + \varepsilon_{ipt})$$

where y is an outcome variable for individual i in province p at time t, QB is a dummy variable that takes the value of one for individual i in Quebec and zero for the other provinces, and $Post$ takes the value of one for the years after 1997 and zero otherwise. The coefficient, β, on the interaction term ($Post * QB$) gives the impact of the reform, i.e., the change in the outcome before and after the reform in Quebec relative to the other provinces after allowing for the same trend. Province and year dummies are included to control for province and year effects. Other independent variables (X_{ipt}) include age, sex, marital status, income, education and month of the interview in each survey cycle. In some of the specifications, individual fixed effects are added to control for unobserved individual heterogeneity to identify the policy effect using only the "within-individual variation". Detailed descriptions of the independent and dependent variables are shown in Appendix Table 2.9 of Appendix A.

Pooled negative binomial models and Poisson models with individual-specific effects are specified for the medication and physician utilization variables since they are count variables.[15] Ordinary Least Squares (OLS) and/or fixed effects linear probability models are employed for all indicator variables. Although a continuous measure, HUI is

[15] There are some complications with applying nonlinear models in a DD framework (Puhani 2012). The treatment effect is the incremental effect of the coefficient of the interaction term but the identification is provided by a nonlinear parametric restriction on the cross difference instead of a common trend assumption.

highly skewed to the left since a large proportion of the population have HUI close or equal to one. Failure to account for the bounded nature of HUI may result in biased and inconsistent estimates, however, studies show that the OLS estimator performs well for bounded preference-based scores, such as EQ-5D index and HUI (Asakawa et al. 2012; Petrou and Kupek 2009; Sullivan and Ghushchyan 2006). Therefore, OLS and fixed effects models are used for the HUI measure. Finally, for all outcome variables, two specifications are estimated. The first is a base model including only province and year dummies, which estimates an unconditional policy effect. Subsequent regressions include controls for individual characteristics and present conditional results. Controlling for additional individual characteristics can allow for compositional changes in the treatment and comparison groups and improve efficiency by reducing the variance of the error term.

The skewed nature of the distribution of health care utilization raises the question of whether the policy has a different effect on low or high users. To further examine the impact of the policy at different points of the distribution of health care utilization, we estimate models of the probability of an individual exceeding specific medication use or physician visit cutoffs. For GP visits, for example, we estimate models for the probability of one or more visits, two or more visits and so on, up to fifteen visits. For the HUI we estimate a DD quantile regression by deciles of the continuous HUI measure to explore the policy effects.

One key identifying assumption in DD analysis here is that the trends in the outcomes of Quebec and the other provinces would have been the same in the absence of the reform. One challenge is to find good comparison groups that have similar preintervention trends to the treatment group. As already mentioned, Table 2.2 shows that demographic and socio-economic characteristics are generally similar in Quebec and the rest of Canada. In our main model specification we control for these observables. The graphic evidence we presented in figures 2.1 to 2.7 also show similar trends in the outcomes in Quebec and the rest of Canada. During our study period 1994 to 2003, there were minimal changes of drug insurance policy among the general population in the other provinces. The minor policy changes regarding catastrophic drug benefit in Ontario and Manitoba will bias our estimates downward if they had any positive effect on the outcomes in those provinces. British Columbia's introduction of the Fair PharmaCare program is at the very end of our study period. Our study period is also prior to or at the very early stages of primary care renewal in Ontario, Alberta, British Columbia and Quebec (Hutchison et al. 2011). Falsification tests and various model specifications were conducted to test the identifying assumption and check the robustness of our estimates. The results suggest that the common trend assumption holds up well. (Refer to section 5.4 for details).

A number of recent papers have raised concerns about inference in DD estimation (Bertrand et al. 2004; Donald and Lang 2007; Cameron et al. 2008). The main problem is that the standard errors computed by conventional estimation methods are too small if the error term in the estimating equation includes unobserved group-year effects (here the group is province). These unobserved group-year effects are usually serially correlated for the same group and lead to inflated t-statistics. If the number of groups is large enough, clustering the standard errors at group level (not group-year level) may take account of this problem (Angrist and Pischke 2009). When there are few groups, several methods have been proposed (e.g. Cameron et al. 2008; Donald and Lang 2007). Donald and Lang (2007) point out that in the case of few groups, the degrees of freedom used in inference should be number of groups minus number of group-constant variables rather than number of individual observations. To address these issues, we compare the standard errors clustered at the province level and individual level. We find that in most cases the standard errors are larger when clustered at the individual level, therefore, we report our main results with standard errors clustered at the individual level. For the Poisson models with individual-specific effects, we report the bootstrapped standard errors.[16]

Even if the number of groups is large, there is still a problem of imprecisely estimated treatment effects if there are only a few treated groups. Conley and Taber (2011) use the nonparametric distribution of the untreated groups to perform inference on the treated parameter and this approach also takes into account the problem of serial correlation in group-year effects. We employ the method by Conley and Taber (2011) to construct consistent confidence intervals for our treatment estimates. (See Appendix B for details of this method).

2.5 Results

2.5.1 Difference-in-differences Estimation Results

Drug Insurance and Other Supplemental Insurance Coverage

The first issues we address are whether the reform increases drug insurance coverage and how it affects other supplemental insurance coverage. Table 2.3 reports DD regression

[16] All models were estimated in STATA version 11 using the cluster option. For Poisson models with individual-specific effects there is no option for cluster-robust in STATA version 11 so we use the vce(bootstrap) option.

results with self-reported status of drug insurance and other supplemental health benefits as the dependent variables. Estimates of the policy effects from OLS regressions with two specifications are shown in columns 1 and 2, and those from fixed effects linear models are in columns 3 and 4.[17] Comparing the four columns, the point estimates of the policy effects do not change notably when control variables are added to the specifications, or when we use a fixed effects rather than an OLS specification. The first row shows that the universal drug program has been successful in expanding drug coverage among the general population. The estimated increase in drug insurance coverage of 21 percentage points represents a 33 percent increase relative to the pre-policy coverage in Quebec. The additional rows in Table 2.3 report the estimates of the policy effects on other supplemental insurance coverage. All coefficient estimates for dental insurance are close to zero and not statistically significant. In contrast, eye-glasses insurance coverage fell by four percentage points while hospital insurance rose by six percentage points. Overall, the policy increased the probability of holding any non-drug insurance by about five percentage points.

The finding that the drug program had no impact on dental insurance should not be surprising since survey evidence shows that dental coverage is usually through stand-alone policies and is not included in the extended health care plans (Hurley and Guindon 2011). On the other hand, hospital services and prescription drugs are the most common services covered by extended health insurance while there is variation across plans for other services, such as vision care. The providers may find it costly to bundle a drug plan with both hospital and eye-glasses plans, so they may drop the latter, which are possibly

[17] For compactness, coefficient estimates of other variables are not presented but they are available upon request. Similarly, only estimates of the policy effects are presented in tables 2.4 to 2.7.

at their discretion. The positive impact on hospital coverage and on overall non-drug coverage implies that the public program did not fully crowd out private drug insurance. This result is to be expected since private insurance sponsors were not allowed to provide any health insurance package unless drug coverage was included.

Medication Utilization, Other Health Care Utilization, and Health Outcomes

Panel A in Table 2.4 presents the DD coefficient estimates from models of medication use and other measures of health care utilization. Each row shows the coefficient estimates of the policy effects together with their standard errors clustered at the individual level (in parentheses) for each dependent variable. Columns 1 and 2 report results from two specifications of negative binomial models, while columns 3 and 4 are results from Poisson models with individual fixed effects. Once again, estimates from different model specifications are generally similar. The first row shows that, after the introduction of the universal drug program, there was a notable increase of medications used in the previous month. The coefficient estimate of 0.12 indicates that the universal drug program led to a 13 percent increase in medication use in the previous month, net of any substitution between prescription and non-prescription medications.[18] There was no significant effect on medication use during the previous two days, as is shown in the second row of Table 2.4. The drug program also led to an 11 to 13 percent increase in the annual number of GP visits but there was no statistically significant effect on specialist utilization.

[18] In Poisson and negative binomial regressions, coefficient estimates can be interpreted as an $(\exp(\beta_i)-1)\times 100\%$ changes in y, given a change of 0 to 1 in the independent variable.

Panel B of Table 2.4 reports the linear probability regression results for all indicator variables of utilization.[19] In both the OLS and fixed effects linear models, coefficient estimates of the policy effects are still very similar in the base model and the model with individual control variables. The mandatory drug program led to an increase in the probability of any medication use in the previous month by 2.4 to 3.4 percentage points. A positive but non-significant effect was observed for the probability of taking any medication in the past two days conditional on positive use in the previous month. The likelihood of visiting a GP increased by about 2 percentage points while there was little effect on the likelihood of visiting a specialist. Finally both OLS and fixed effects models failed to reveal any statistically significant effects on the likelihood of having an inpatient admission in the previous year.

Panel C of Table 2.4 presents the estimated impact of the mandatory drug program on health status. The program had a small positive effect on self-assessed health status and HUI. Previous studies document that an increase of 0.03 or greater in HUI score is considered as clinically important, and an increase of 0.01 is generally important (Horsman et al. 2003; Grootendorst et al. 2000). The estimated impact on HUI is around 0.010, representing an important improvement in health status.

2.5.2 Heterogeneous Effects at Different Points of the Distribution

The coefficients from a series of fixed effects linear probability models are shown in Figure 2.8. The estimates of the policy impact on the probability of different number of

[19] Marginal effects at the mean from probit/logit models for these variables were found to be very similar to the coefficient estimates from the linear probability models.

medications (up to four) and GP visits (up to 15) are reported.[20] The estimates represent the effects on the probability of more medication use or GP visits at the cutoff points. The positive effects vary at different points of the distribution for all outcomes. For medications, the impact is the largest at the probability of taking two or more medications last month. For physician visits, the impact on the probability of more GP visits is the largest at three visits.

DD conditional quantile regression estimates by deciles of HUI are plotted in Figure 2.9. We find that the impact on health is monotonically decreasing and is virtually zero from the median to the ninth decile. This finding should not be surprising given that more than 50% of the population in the sample have HUI greater than 0.97. It should be noted that the positive impact on HUI is 0.035 at the first decile, representing a clinically important improvement in health status. At the 20th and 30th percentiles, the policy effects are around 0.015 and still statistically significant. These results suggest that it is mainly the less healthy individuals that experienced an improvement in health status. For policy purposes, this finding is crucial since a mandatory drug program can generate substantial health gains for those with poor health status, who are usually the target of public drug programs.

2.5.3 Subgroup Analysis by Drug Insurance Status, Chronic Condition and Income

One important consideration in understanding the potential effects of the universal drug policy is how it may affect sub-population groups differently. If patients in different subgroups tend to use different combinations of inputs, such as medications, physician

[20] Effects on specialist visits are rarely significant so we present the effects on GP only.

and hospital services, to produce health and if health care is more effective for some patients than others, then the benefits of health insurance should vary accordingly. Thanks to the panel nature of the data, individuals who were previously uninsured can be identified and subgroup analysis by previous insurance status is conducted to further identify heterogeneous impacts of the policy. Similarly, policy effects by chronic condition and household income groups are estimated. We examine the heterogeneous effects by looking at the interactions between these subgroups and the main treatment variable.[21]

Table 2.5 presents the estimation results including interactions between the treatment variable and individual's pre-policy drug insurance status. Panel A presents results of drug and physician utilization, Panel B results of the probability of any medication use, a physician visit and hospitalization, and Panel C results of health status. In Panel A, we continue to find evidence of positive policy effects on medication use and GP visits. The effects are larger for the previously uninsured, but they are not statistically significant. In Panel B, the coefficients of the pre-policy insurance status interactions suggest that the positive effects on the probability of taking medications or visiting a GP are larger for the previously uninsured. As is shown in column 1 of Panel C, the coefficient on the interaction term suggests that the policy impact on the HUI of the previously uninsured is not statistically different from that of the previously insured. Results in column 2 of Panel C reveal that the previously uninsured are more likely to report excellent or very good health after the policy change.

[21] Only estimates from negative binomial models and OLS models are reported in the following tables since estimates from models with individual fixed effects are not qualitatively different.

Table 2.6 reports the estimated effects including interactions of individuals with any chronic condition at the time of the survey. Panel A presents results of medication and physician use, Panel B results of the probability of any medication use, a physician visit and hospitalization, and Panel C results of health status. In Panel A, the regression estimates reveal that the chronically ill show a larger increase in medication use in the past two days compared to those without any chronic condition. As we mentioned above, medications taken in the past two days is an indicator of current medication use. The large increase in this measure suggests evidence of increase in medication compliance among the chronically ill. The interaction effects are also positive for medication use in the previous month and GP visits but not statistically significant. In Panel B, for the probability of medication use, the policy effect is much larger for the chronically ill. For example, the policy effect on the probability of taking medications in the past two days is five percent higher for the chronically ill. Estimates for the likelihood of a GP visit are also larger among the chronically ill. From Panel C we find that there is a larger positive effect on the HUI of the chronically compared to those without any chronic condition. The policy effect on self-assessed health status is not statistically different between those with and without a chronic condition.

We further examine the effects of the policy including income interactions for individuals with family income less than $30,000 and the results are presented in Table 2.7. The policy and income interaction variables are never statistically significant, suggesting no differential policy effects by income for all outcome variables.[22]

[22] We have also tried other income category specifications and the standard errors are too large to reveal any statistically significant differential effects.

2.5.4 Sensitivity and Robustness Analysis

We can test the identifying assumption of our DD estimation by falsification (placebo) tests for the years before the policy change. We define 1994/1995 as the pre-policy years and 1996 as the post-policy year in the falsification tests. The test results (shown in Table 2.8) suggest that none of the coefficients is statistically significant except the coefficients in the specialist utilization models, which are significantly negative. In general, the falsification tests do not reject the common trend hypothesis in our models. Another concern is whether the minor policy change in the public catastrophic drug plans in Ontario and Manitoba bias our estimates. To address this issue, we include specific time trends for Ontario and Manitoba in the list of independent variables. The estimated effects of interest are largely unchanged. In an alternative check, we dropped observations in Ontario and Manitoba from our sample. The policy effect estimates in all models are very similar to those presented in tables 2.3 and 2.4.

Several other specifications were estimated to check the robustness of our estimates. As described in Appendix Table 2.9, all the medication and physician utilization variables are truncated at the 99.5 percentiles. We tried the original values without truncation and the estimates are larger and more significant than those in Table 2.4. The results are essentially unchanged when we truncate them at 99 percentiles. Further, including health status, such as self-assessed health status or chronic condition status, as control variables in the utilization equations does not change the estimates substantially. Controlling for initial health status in the first cycle of the survey also makes little changes to our estimates. It may be possible that access to specialists is different from access to GPs, who act as gatekeepers in the health care system, so we included an indicator variable for whether the respondent has a regular family doctor or not in the specialist visit equation. However, there is no meaningful difference in the estimates. Finally coefficients and confidence intervals using the Conley and Taber (2011) method shown in Appendix B are consistent with the results in tables 2.3 and 2.4.

2.6 Conclusion and Discussion

Canada continues to debate the merits of introducing a national pharmacare program. The debate needs to be informed by evidence from Canada regarding the effectiveness of expanded drug insurance coverage. This study examined the impact of Quebec's mandatory universal drug program on insurance coverage, health care utilization and health outcomes. We find that the introduction of the mandatory drug program substantially increased drug coverage among the general population. The program also increased medication use and GP visits. No statistically significant effects were found for specialist visits and hospitalization. Furthermore, we find that the improvement in access to health care and health outcomes is concentrated among the previously uninsured and those with a chronic condition. Most importantly, less healthy people experienced a large improvement in their health status.

The findings provide useful information for the implementation of a universal pharmacare program in Canada. A public drug program covering those who do not have private drug plans can improve access to medications, increase GP visits, and generate substantial health gains for the chronically ill and less healthy people. In the long run, this may lead to reduction in overall health care utilization. The mixed public and private program provides a feasible solution to the design of a national pharmacare program. Given public concerns about the cost of a pure public drug program and the status quo of private insurance industries, the incremental change of adding a public drug plan on top of the private plans is promising. The positive spillover effects on physician visits, which are free in Canada, can also shed light on the policy concern that private drug insurance contributes to the pro-rich inequity in physician utilization (Allin and Hurley 2009). Offering drug insurance coverage to the low income people, who are less likely to have private drug insurance, may help to reduce the pro-rich inequity in physician utilization.

Future research can build on this study in several ways. For example, we have examined the impact of the drug program on health care utilization and health outcomes. Future research can explore the impact on out-of-pocket drug expenditure and total drug expenditure. In addition, if there is data for prescription and non-prescription drug utilization, the policy impact on the substitution between them is another interesting research question.

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Table 2.1: Description of Quebec's mandatory universal drug program policies

	Dates	Annual premium	Coinsurance	Monthly deductible	Max. monthly contribution
Welfare recipients	Prior to Aug 1996		Full coverage		
	Aug 1996 to Dec 1996	None	25%	None	$16.66
	Jan 1997 to 2003	None	25%	$8.33	$16.66
Low income seniors	Prior to Aug 1996		Full coverage		
	Aug 1996 to Dec 1996	None	25%	None	$16.66
	Jan 1997 to 2003	None	25%	$8.33	$16.66
Other seniors	Prior to Aug 1996	None	$2/prescription	None	$100
	Aug 1996 to Dec 1996	$0-$175	25%	None	$41.66/$62.50
	Jan 1997	$0-$175	25%	$8.33	$41.66/$62.50
	2003	$0-$460	25%/28%	$8.33/$9.60	$16.66/$46.17/$69.92
General population	Before 1997		No public coverage		
	1997 to 1999	$0-$175	25%	$8.33	$62.49
	2000	$0-$350	25%	$8.33	$62.49
	2001	$0-$385	25%	$8.33	$62.49
	2002	$0-$422	27.4%	$9.13	$68.50
	2003	$0-$460	28%	$9.60	$69.92
	2004	$0-$494	28.5%	$10.25	$71.42
	2005	$0-$521	28.5%	$11.90	$71.42
	2006	$0-$538	29%	$12.10	$73.42
	2007	$0-$557	30%	$14.10	$75.33
	2008	$0-$570	31%	$14.30	$77.21
	2009	$0-$585	32%	$14.95	$79.53
	2010	$0-$600	32%	$16.00	$80.25
	2011	$0-$563	32%	$16.00	$80.25
	2012	$0-$579	32%	$16.25	$82.66

Notes: General population refers to those non-elderly (aged 64 and younger) not on social assistance and senior refers to those aged 65 and over. Low income senior refers to those on full Guaranteed Income Support (GIS) and other senior citizens refer to those on partial GIS or not on GIS. The maximum monthly contribution for other seniors after 1996 depends on GIS status. Coinsurance and monthly deductible for other seniors in 2003 also depend on GIS status. The rates in the lower panel are usually effective from July to June in the next year. Here all the rates are as of December. All of the above are free for children of persons insured under the public plan if they are under age 18 or if they are full- time students between ages 18 and 25.

Source: the Régie de l'assurance maladie du Québec (RAMQ)

Table 2.2: Sample means

	QB		Rest of Canada	
Independent variable	before 1997	after 1997	before 1997	after 1997
Demographic				
Age	34.558	39.464	34.440	39.267
Male	0.517	0.516	0.512	0.502
Marital status				
Single	0.339	0.279	0.327	0.260
Married	0.589	0.626	0.606	0.643
Wid./Sep./Div.	0.072	0.095	0.067	0.097
Education attainment				
Less than secondary school graduation	0.278	0.181	0.239	0.138
Secondary school graduation	0.138	0.131	0.162	0.157
Some post secondary education	0.240	0.260	0.265	0.308
Post secondary graduation	0.344	0.428	0.334	0.397
Household income				
Lowest income	0.121	0.067	0.095	0.062
Lower middle income	0.298	0.228	0.279	0.186
Upper middle income	0.438	0.425	0.423	0.394
Highest income	0.144	0.281	0.203	0.357
Dependent variable				
Medication use				
Previous month				
Total number	1.146	1.520	1.501	1.765
Yes/no	0.716	0.795	0.778	0.824
Previous two days				
Total number	0.687	0.923	0.754	1.025
Yes/no	0.429	0.509	0.481	0.545
Physician visits in the previous 12 months				
GP				
Total number	1.967	2.140	3.164	3.075
Yes/no	0.666	0.708	0.775	0.793
Specialist				
Total number	0.808	0.829	0.771	0.758
Yes/no	0.288	0.317	0.223	0.244
Inpatient hospital in the previous 12 months				
Yes/no	0.077	0.069	0.068	0.058
Health status				
Health Utility Index (HUI3)	0.918	0.933	0.904	0.909
Self-assessed health status	0.707	0.684	0.709	0.664
N	2801	4398	13195	19960

Notes: Data from the National Population Health Survey. Displayed are the sample means for each variable. There are two waves (1994/1995, 1996/1997) of data before the policy and three waves (1998/1999, 2000/2001, 2002/2003) of data after the policy. The sample is further split into Quebec and the rest of Canada. Observations in 1997 are not included since it is a transitional period. For self-assessed health status, excellent and very good health is coded as 1 and good, fair, and poor health is coded as 0. Refer to Appendix Table 2.9 for detailed descriptions of the variables.

Table 2.3: Estimated effects of the Quebec mandatory drug policy on drug, dental, eye-glasses and hospital insurance coverage

Dependent variable	OLS		Fixed Effects		N
	(1)	(2)	(3)	(4)	
	Basic	Controls	Basic	Controls	
Drug insurance	0.2209***	0.2180***	0.2141***	0.2126***	31059
	(0.0181)	(0.0175)	(0.0181)	(0.0178)	
Dental insurance	-0.0092	-0.0114	-0.0035	-0.0067	31064
	(0.0187)	(0.0179)	(0.0178)	(0.0174)	
Eye-glasses insurance	-0.0444**	-0.0475***	-0.0386**	-0.0414**	30741
	(0.0190)	(0.0184)	(0.0184)	(0.0182)	
Supplemental hospital insurance	0.0693***	0.0638***	0.0724***	0.0685***	29597
	(0.0189)	(0.0178)	(0.0179)	(0.0174)	
Any non-drug supplemental insurance	0.0535***	0.0487***	0.0571***	0.0537***	30808
	(0.0176)	(0.0166)	(0.0167)	(0.0164)	

Notes: Data from the National Population Health Survey (1994 to 2003). Information on drug insurance status and other insurance status is not available in the 1994/1995 wave data so there is one pre-policy wave of data in the regressions above. Each dependent variable is a binary variable indicating whether the respondent has the insurance or not. The results presented here list the difference-in-difference estimates for each of the outcomes listed in the first column. The basic models in columns (1) and (3) include only province and year dummies. Columns (2) and (4) further controls for age, age^2, sex, marital status, income, education, month of interview, year effects and province effects. Standard errors in parentheses are clustered at the individual level in all models. Standard errors in parentheses are clustered at the individual level in all models. * indicates significance at 10% level, ** at 5% level, *** at 1% level.

Table 2.4: Estimated effects of the Quebec mandatory drug policy on medication use, physician visits and hospitalization

Dependent Variable

Panel A: medication use and physician visits

	Negative Binomial		Fixed Effects Poisson		N
	Basic	Controls	Basic	Controls	
	(1)	(2)	(3)	(4)	
Total number of medications used in the previous month	0.1196***	0.1231***	0.1208***	0.1200***	40354
	(0.0277)	(0.0276)	(0.0222)	(0.0218)	
Total number of medications used in the previous two days	-0.0152	-0.0289	0.0512	0.0441	32391
	(0.0519)	(0.0523)	(0.0362)	(0.0370)	
Total number of GP visits in the previous 12 months	0.1105**	0.1244***	0.1046***	0.1077***	40354
	(0.0444)	(0.0446)	(0.0368)	(0.0376)	
Total number of specialist visits in the previous 12 months	0.0441	0.0526	0.1169	0.1187	40354
	(0.0937)	(0.0979)	(0.0861)	(0.0844)	

Panel B: indicators of medication use, GP visits, specialist visits and hospitalization

	OLS		Fixed Effects		N
	Basic	Controls	Basic	Controls	
Medication used in the previous month (yes/no)	0.0328**	0.0340**	0.0237*	0.0237*	40354
	(0.0133)	(0.0133)	(0.0137)	(0.0136)	
Medication used in the previous two days (yes/no)	0.0139	0.0130	0.0210	0.0185	32391
	(0.0177)	(0.0172)	(0.0183)	(0.0183)	
GP visits in the previous 12 months (yes/no)	0.0242*	0.0241*	0.0167	0.0150	40354
	(0.0136)	(0.0135)	(0.0141)	(0.0141)	
Specialist visits in the previous 12 months (yes/no)	0.0081	0.0106	0.0074	0.0091	40354
	(0.0138)	(0.0137)	(0.0143)	(0.0142)	
Inpatient hospital stay in the previous 12 months (yes/no)	0.0017	0.0022	0.0088	0.0082	40354
	(0.0083)	(0.0082)	(0.0086)	(0.0086)	

Panel C: health outcomes

	OLS		Fixed Effects		N
	Basic	Controls	Basic	Controls	
Self-assessed health status	0.0229*	0.0233*	0.0147	0.0157	40354
	(0.0133)	(0.0132)	(0.0132)	(0.0131)	
HUI	0.0104**	0.0096**	0.0067*	0.0069*	40354
	(0.0041)	(0.0041)	(0.0039)	(0.0039)	

Notes: Data from the National Population Health Survey (1994 to 2003). The results presented here list the difference-in-difference estimates for each of the outcomes listed in the first column and each row represents a different dependent variable. The basic models in columns (1) and (3) include only province and year dummies. Columns (2) and (4) further controls for age, age^2, sex, marital status, income, education, month of interview. Standard errors in parentheses are clustered at the individual level in all models. * indicates significance at 10% level, ** at 5% level, *** at 1% level.

Table 2.5: Estimated effects of the Quebec mandatory drug policy by prescription drug insurance status before the policy change

Panel A: medication use and physician visits (negative binomial)

	Total number of medications used in the previous month	Total number of medications used last two days	Total number of GP visits in the previous 12 months	Total number of specialist visits in the previous 12 months
Post*qb*no insurance	0.0500	0.0504	0.0222	0.1038
	(0.0424)	(0.0726)	(0.0687)	(0.1295)
Post*qb	0.0991***	-0.0549	0.1169**	0.0206
	(0.0318)	(0.0586)	(0.0526)	(0.1097)
No insurance	-0.1316***	-0.1640***	-0.1414***	-0.1789***
	(0.0200)	(0.0340)	(0.0282)	(0.0570)
N	38751	31194	38751	38751

Panel B: indicators of medication use, GP visits, specialist visits and hospitalization (OLS)

	Medication use in the previous month (yes/no)	Medication use in the previous two days (yes/no)	GP visits in the previous 12 months (yes/no)	Specialist visits in the previous 12 months (yes/no)	Inpatient hospital stay in the previous 12 months (yes/no)
Post*qb*no insurance	0.0337*	0.0374	0.0327*	-0.0185	-0.0060
	(0.0191)	(0.0259)	(0.0198)	(0.0203)	(0.0107)
Post*qb	0.0165	0.0021	0.0140	0.0197	0.0029
	(0.0149)	(0.0191)	(0.0152)	(0.0152)	(0.0094)
No insurance	-0.0388***	-0.0415***	-0.0420***	-0.0232***	-0.0057
	(0.0087)	(0.0110)	(0.0083)	(0.0077)	(0.0040)
N	38751	31194	38751	38751	38751

Panel C: health outcomes (OLS)

	HUI	Self-assessed health status
Post*qb*no insurance	-0.0093	0.0476**
	(0.0060)	(0.0216)
Post*qb	0.0133***	0.0073
	(0.0046)	(0.0156)
No insurance	0.0074**	-0.0093
	(0.0035)	(0.0104)
N	38751	38751

Notes: Data from the National Population Health Survey (1994 to 2003). Coefficients estimates in panel A are from negative binomial models and others from OLS models. Other control variables include age, age^2, sex, marital status, income, education, month of interview, year effects and province effects. Standard errors in parentheses are clustered at the individual level in all models. * indicates significance at 10% level, ** at 5% level, *** at 1% level. Since information for insurance status is not available in the 1994/1995 wave, we made the assumption that the insurance status in 1996 is the same as that in 1994.

Table 2.6: Estimated effects of the Quebec mandatory drug policy by the presence of chronic condition

Panel A: medication use and physician visits (negative binomial)

	Total number of medications used in the previous month	Total number of medications used last two days	Total number of GP visits in the previous 12 months	Total number of specialist visits in the previous 12 months
Post*qb*chronic	0.0495	0.1787***	0.0599	-0.0408
	(0.0374)	(0.0695)	(0.0613)	(0.1209)
Post*qb	0.0806**	-0.1823**	0.0764	0.0737
	(0.0366)	(0.0736)	(0.0563)	(0.1213)
Chronic	0.5602***	0.7751***	0.6722***	0.8745***
	(0.0156)	(0.0283)	(0.0236)	(0.0513)
N	40323	32369	40323	40323

Panel B: indicators of medication use, GP visits, specialist visits and hospitalization (OLS)

	Medication use in the previous month (yes/no)	Medication use in the previous two days (yes/no)	GP visits in the previous 12 months (yes/no)	Specialist visits in the previous 12 months (yes/no)	Inpatient hospital stay in the previous 12 months (yes/no)
Post*qb*chronic	0.0270	0.0540**	0.0551***	0.0177	0.008
	(0.0171)	(0.0221)	(0.0185)	(0.0180)	(0.00947)
Post*qb	0.0168	-0.0241	-0.0081	-0.0011	-0.003
	(0.0173)	(0.0212)	(0.0173)	(0.0154)	(0.00866)
Chronic	0.1696***	0.2173***	0.1362***	0.1449***	0.037***
	(0.0071)	(0.0090)	(0.0068)	(0.0065)	(0.00341)
N	40323	32369	40323	40323	40323

Panel C: health outcomes (OLS)

	HUI	Self-assessed health status
Post*qb*chronic	0.0139***	-0.0151
	(0.0049)	(0.0182)
Post*qb	0.0026	0.0337**
	(0.0043)	(0.0163)
Chronic	-0.0552***	-0.1671***
	(0.0027)	(0.0079)
N	40323	40323

Notes: Data from the National Population Health Survey (1994 to 2003). Coefficient estimates in panel A are from negative binomial models and others from OLS models. Other control variables include age, age^2, sex, marital status, income, education, month of interview, year effects and province effects. Standard errors in parentheses are clustered at the individual level in all models. * indicates significance at 10% level, ** at 5% level, *** at 1% level.

Table 2.7: Estimated effects of the Quebec mandatory drug policy by low income group

Panel A: medication use and physician visits (negative binomial)

	Total number of medications used in the previous month	Total number of medications used last two days	Total number of GP visits in the previous 12 months	Total number of specialist visits in the previous 12 months
Post*qb*low income	-0.0097	-0.0037	-0.0124	-0.0817
	(0.0408)	(0.0667)	(0.0678)	(0.1053)
Post*qb	0.1259***	-0.0279	0.1282**	0.0766
	(0.0305)	(0.0573)	(0.0505)	(0.1042)
N	40354	32391	40354	40354

Panel B: indicators of medication use, GP visits, specialist visits and hospitalization (OLS)

	Medication use in the previous month (yes/no)	Medication use in the previous two days (yes/no)	GP visits in the previous 12 months (yes/no)	Specialist visits in the previous 12 months (yes/no)	Inpatient hospital stay in the previous 12 months (yes/no)
Post*qb*low income	0.0136	0.0292	0.0077	-0.0043	0.014
	(0.0192)	(0.0244)	(0.0194)	(0.0195)	(0.0118)
Post*qb	0.0300**	0.0045	0.0218	0.0118	-0.002
	(0.0146)	(0.0191)	(0.0147)	(0.0149)	(0.00847)
N	40354	32391	40354	40354	40354

Panel C: health outcomes (OLS)

	HUI	Self-assessed health status
Post*qb*low income	0.0038	0.0023
	(0.0062)	(0.0213)
Post*qb	0.0085	0.0226
	(0.0042)	(0.0144)
N	40354	40354

Notes: Data from the National Population Health Survey (1994 to 2003). Coefficient estimates in panel A are from negative binomial models and others from OLS models. Other control variables include age, age^2, sex, marital status, income, education, month of interview, year effects and province effects. Standard errors in parentheses are clustered at the individual level in all models. * indicates significance at 10% level, ** at 5% level, *** at 1% level.

Table 2.8: Falsification tests

	Negative binomial	Fixed effects Poisson	N
Total number of medications used in the previous month	-0.0107	0.0068	15996
	(0.0414)	(0.0404)	
Total number of medications used in the previous two days	-0.0692	-0.0850	12373
	(0.0743)	(0.0555)	
Total number of GP visits in the previous 12 months	-0.0068	0.0138	15996
	(0.0724)	(0.0540)	
Total number of specialist visits in the previous 12 months	-0.4064***	-0.2317*	15996
	(0.1426)	(0.1191)	
	OLS	Fixed effects linear	N
Medication used in the previous month (yes/no)	0.0026	-0.0191	15996
	(0.0202)	(0.0216)	
Medication used in the previous two days (yes/no)	-0.0238	-0.0426	12373
	(0.0256)	(0.0296)	
GP visits in the previous 12 months (yes/no)	-0.0003	0.0144	15996
	(0.0217)	(0.0240)	
Specialist visits in the previous 12 months (yes/no)	-0.0242	-0.0049	15996
	(0.0196)	(0.0213)	
Inpatient hospital stay in the previous 12 months (yes/no)	0.0142	0.0111	15996
	(0.0134)	(0.0149)	
HUI	-0.0069	-0.0122*	15996
	(0.0057)	(0.0054)	
Self-assessed health status	-0.0080	0.0077	15996
	(0.0207)	(0.0226)	

Notes: The estimates are coefficients of an interaction variable of Quebec and an indicator variable taking the value of one if in year 1996. The regression use 1994/1995 and 1996 data only. The test for insurance status is not possible since it is only available in the 1996 data. Standard errors in parentheses are clustered at the individual level in all models. * indicates significance at 10% level, ** at 5% level, *** at 1% level.

Figure 2.1: Prescription drug insurance coverage

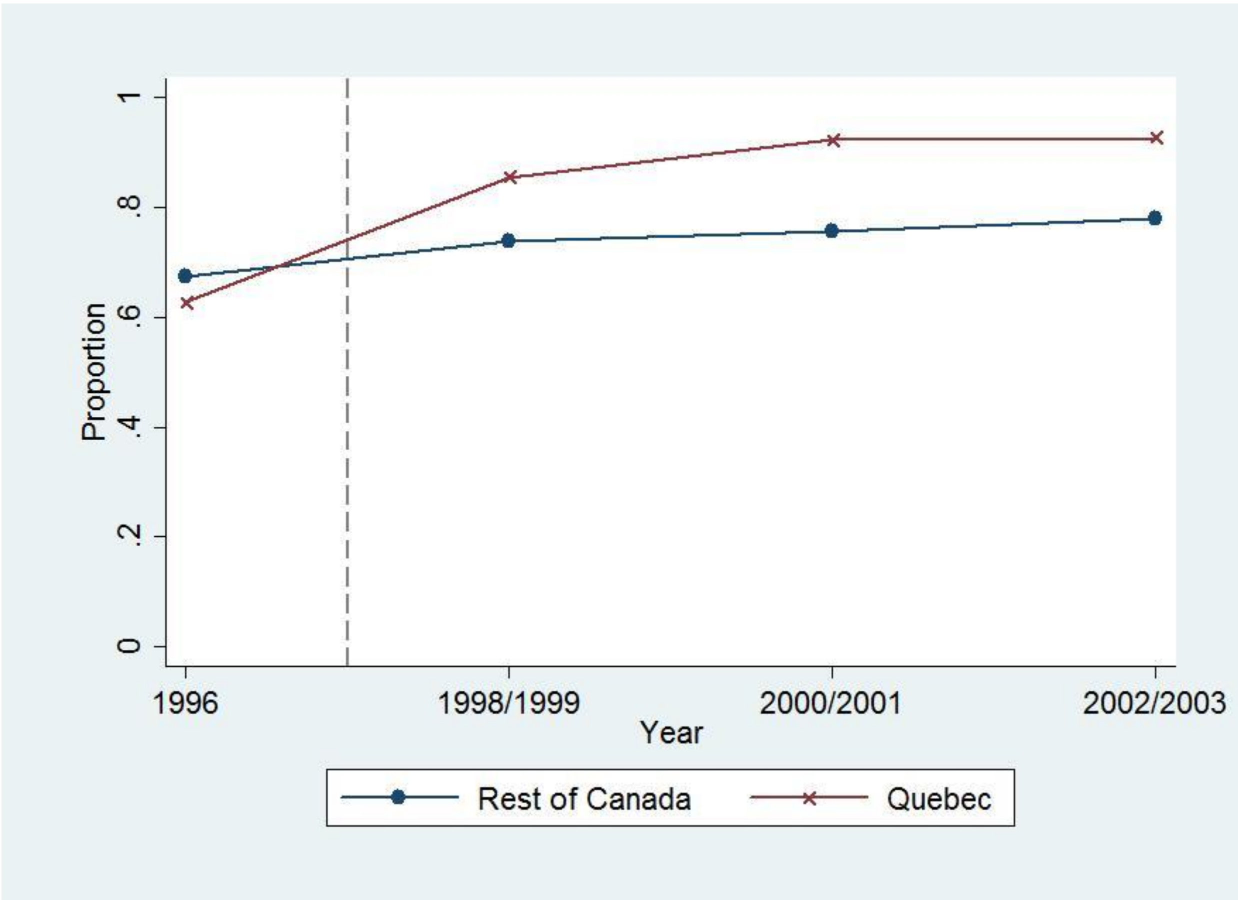


Figure 2.2: Proportion with medication use in the previous month

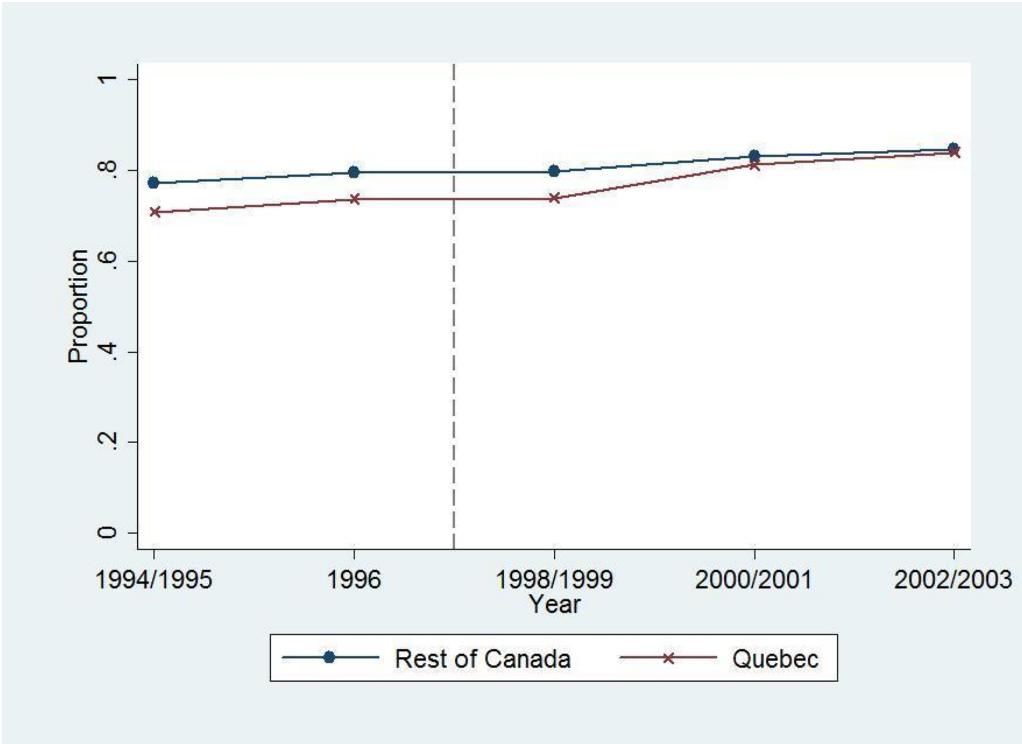


Figure 2.3: Number of medications used in the past month

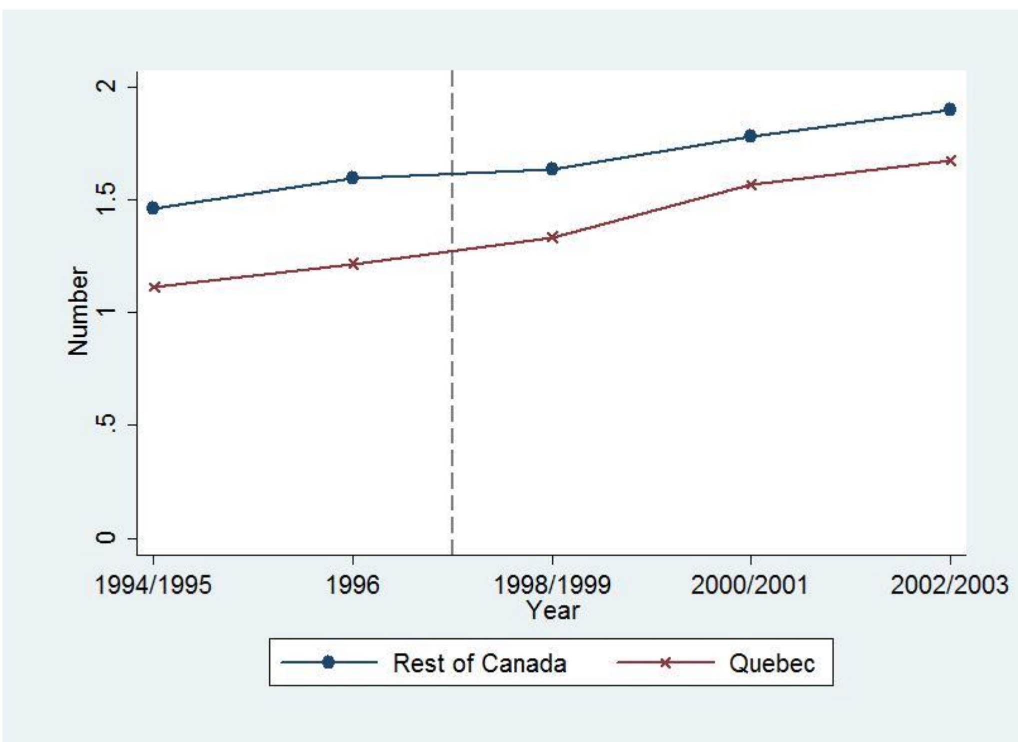


Figure 2.4: Proportion with a GP visit in the previous 12 months

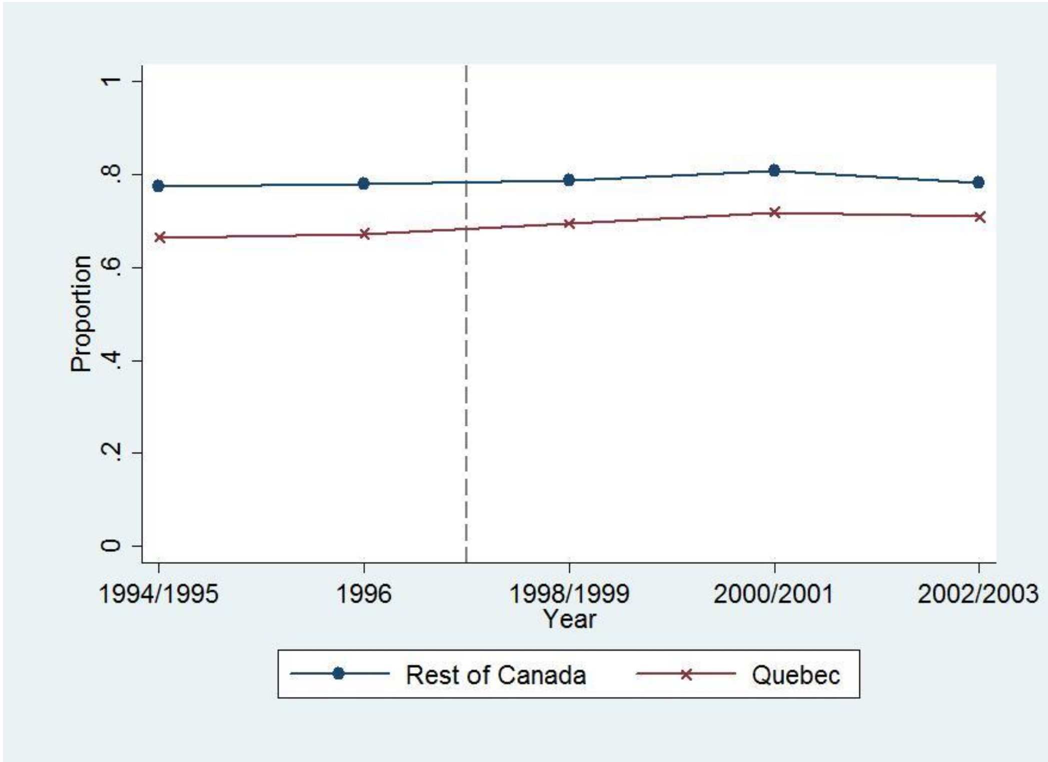


Figure 2.5: Number of GP visits in the previous 12 months

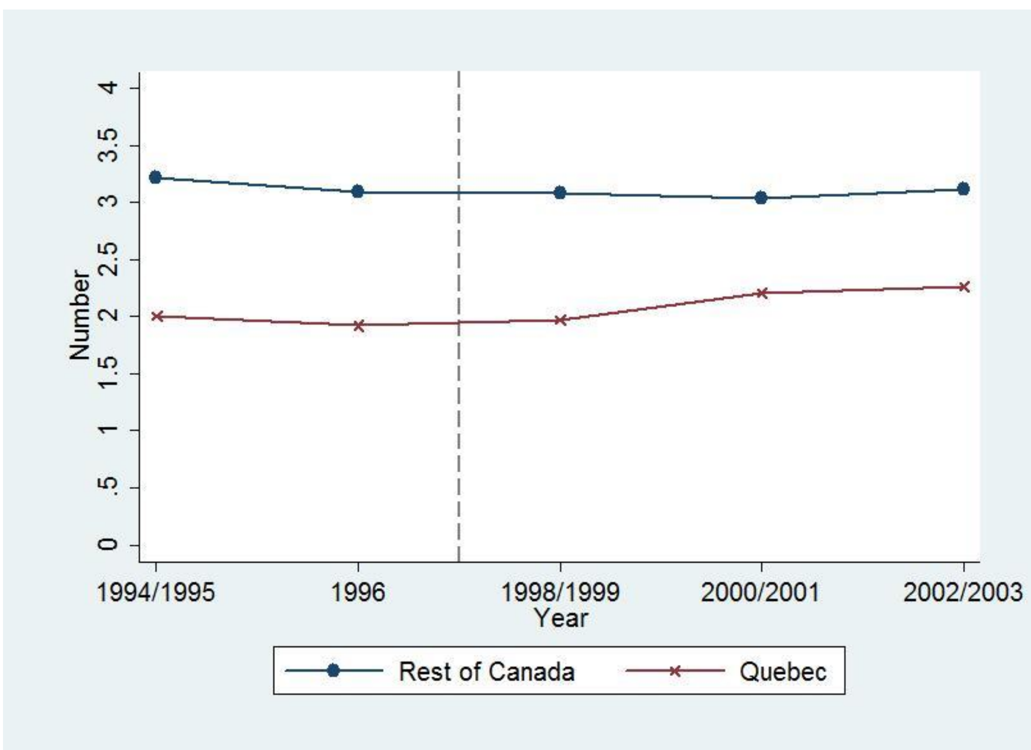


Figure 2.6: Health Utility Index (HUI)

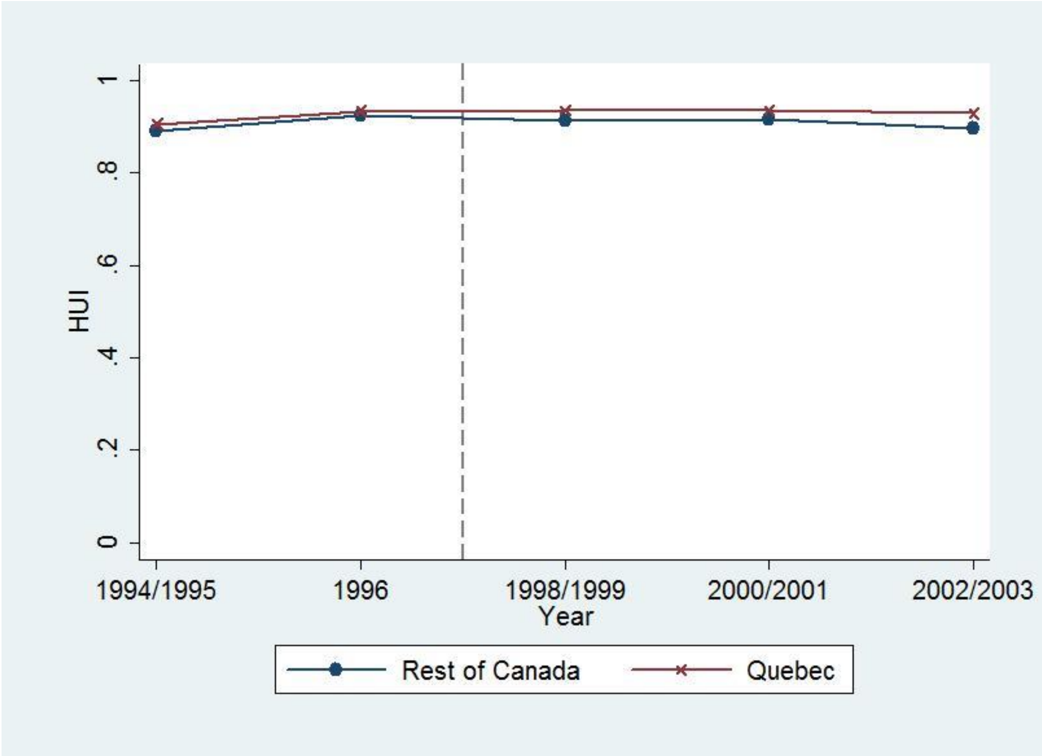


Figure 2.7: Proportion reporting excellent or very good health

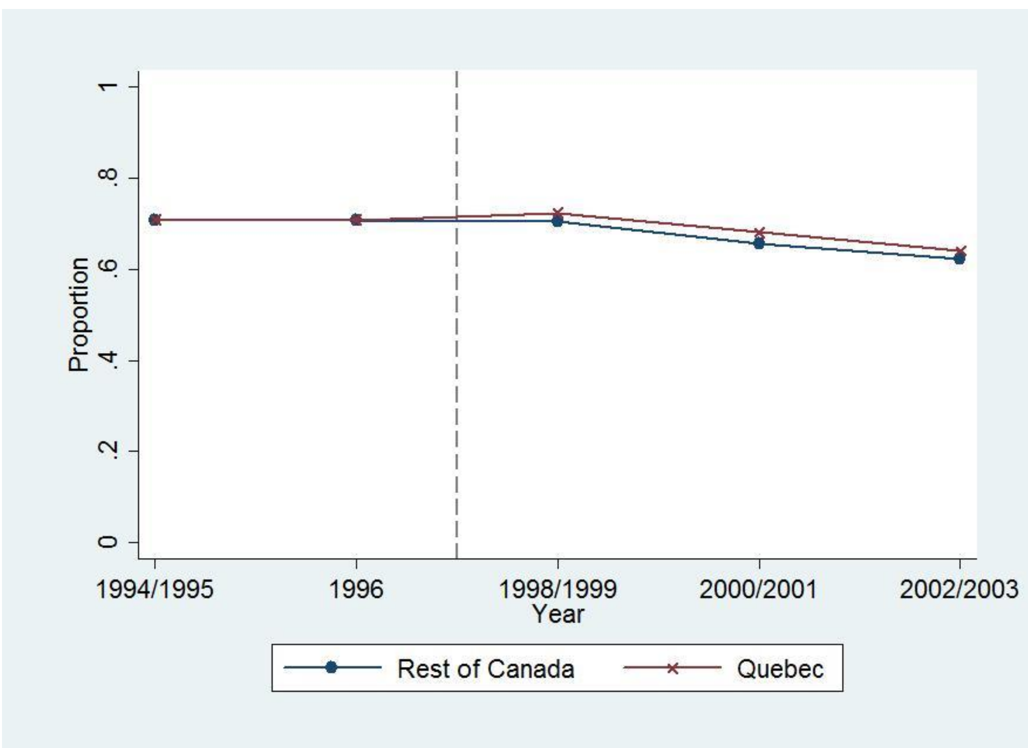


Figure 2.8: Effects on medication utilization and physician visits at different cutoff points

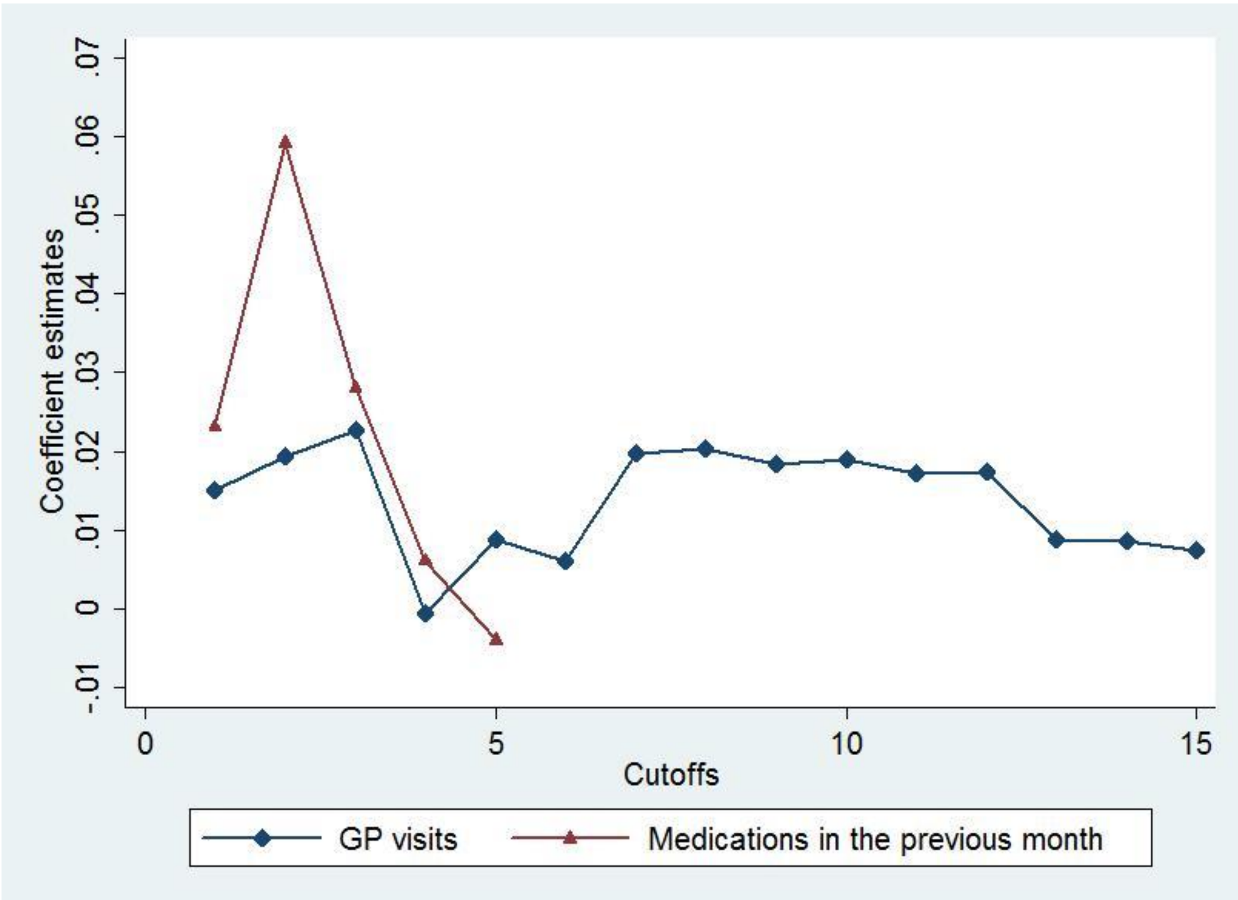
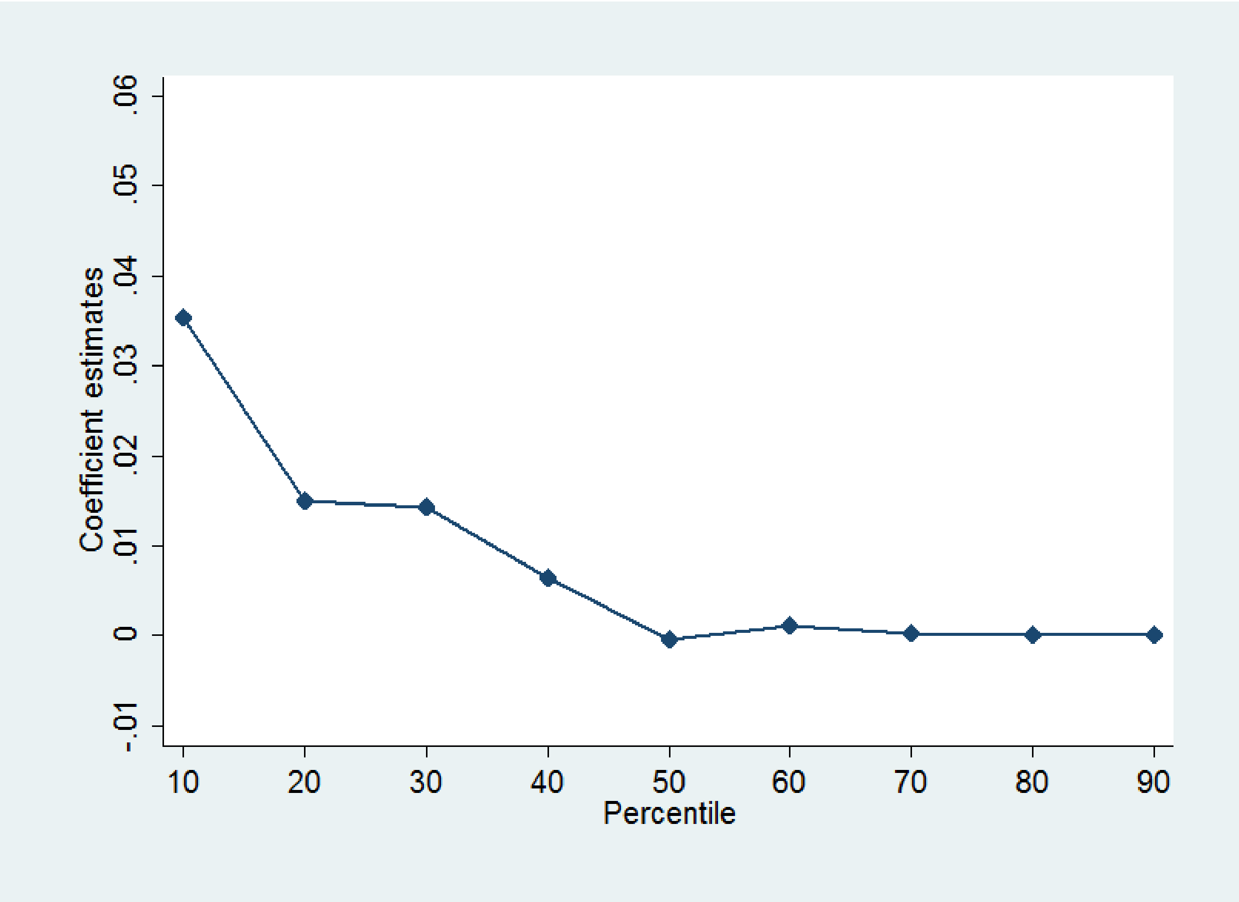


Figure 2.9: Effects on HUI at different percentiles



Appendix A

Appendix Table 2.9: Variable Description

Variable Name	Variable Details
Independent variables	
Age	age of the respondent
Male	an indicator variable taking the value of one if male
Married or common-law	an indicator variable taking the value of one if married or common
Wid./Sep./Div.	an indicator variable taking the value of one if widowed/separated/divorced
Single	an indicator variable taking the value of one if single
Lowest income	an indicator variable taking the value of one if household income is less than \$15,000 for 1 or 2 persons, less than \$20,000 for 3 or 4 persons, less than 30,000 for 5 or more persons
Lower middle income	an indicator variable taking the value of one if household income is \$15,000 to \$29,999 for 1 or 2 persons, \$20,000 to \$39,999 for 3 or 4 persons, \$30,000 to \$59,999 for 5 or more persons
Upper middle income	an indicator variable taking the value of one if household income is \$30,000 to \$59,999 for 1 or 2 persons, \$40,000 to \$79,999 for 3 or 4 persons, \$60,000 to \$79,999 for 5 or more persons
Highest income	an indicator variable taking the value of one if household income is \$60,000 for 1 or 2 persons, \$80,000 or more for 3 persons or more
Less than secondary school graduation	an indicator variable taking the value of one if less than secondary school graduation
Secondary school	an indicator variable taking the value of one if secondary school graduate
Some post-secondary	an indicator variable taking the value of one if some post-secondary education
Post-secondary graduate	an indicator variable taking the value of one if post-secondary graduate
Intermth1-intermth12	indicator variables taking the value of one if interviewed in January, February, …..December
Chronic	an indicator variable taking the value of one if any chronic condition
Low income	an indicator variable taking the value of one if household income is in the lowest income group or in the lower middle income group
Dependent variables	
Drug insurance	an indicator variable taking the value of one if the respondent has insurance that covers all or part of the cost of prescription medications, including any private, government or employer-paid insurance plans
Dental insurance	an indicator variable taking the value of one if the respondent has insurance that covers that covers all or part of the dental expenses, including any private, government or employer-paid insurance plans

Eye-glasses insurance	an indicator variable taking the value of one if the respondent has insurance that covers all or part of the cost of eye glasses or contact lenses, including any private, government or employer-paid insurance plans
Supplemental hospital insurance	an indicator variable taking the value of one if the respondent has insurance that covers all or part of hospital charges for a private or semi-private room, including any private, government or employer-paid insurance plans
Any non-drug supplemental insurance	an indicator variable taking the value of one if the respondent has dental insurance or eye-glasses insurance or supplemental hospital insurance and taking the value of zero if s/he has none of the above
Number of medications used in the previous month	The sum of different types of medications taken in the previous month. The questionnaire asks a series of questions: In the past month, did you take pain relievers such as aspirin or Tylenol (including arthritis medicine and anti-inflammatories), tranquilizers such as Valium, diet pills, anti-depressants, codeine, Demerol or morphine, allergy medicine, asthma medications, cough or cold remedies, penicillin or other antibiotic, medicine for the heart, medicine for blood pressure, diuretics or water pills, steroids, insulin, pills to control diabetes, sleeping pills, stomach remedies, laxatives. The number of medications is the sum of these different types of medications if the respondent answered yes. Medications including birth control pills and hormones for menopause or aging symptoms were only asked among women of certain age groups so were not counted for consistency. Thyroid medication which was not available in the first cycle was also excluded from the counts.
Number of medications used during the previous two days	Among respondents who took positive medications in the previous month, the survey further asked how many different medications he/she took during the last two days.
Number of GP visits in the past 12 months	Number of times in the past 12 months the respondent had seen or talked on the telephone about his/her physical, emotional or mental health with a family doctor or general practitioner
Number of specialist visits in the past 12 months	Number of times in the past 12 months the respondent had seen or talked on the telephone about his/her physical, emotional or mental health with other medical doctor (such as a surgeon, allergist orthopedist, gynaecologist or psychiatrist)
Medication use in the previous month	an indicator variable taking the value of one if the respondent took any medication in the previous month
Medication use during the previous two days	an indicator variable taking the value of one if the respondent took any medication in the previous two days conditional on taking positive medications in the previous month
GP visits in the previous 12 months (yes/no)	an indicator variable taking the value of one if the respondent made at least one GP visit in the past 12 months
Specialist visits in the previous 12 months (yes/no)	an indicator variable taking the value of one if the respondent made at least one other medical doctor visit in the past 12 months
Inpatient hospital stay in the previous 12 months (yes/no)	an indicator variable taking the value of one if the respondent had been a patient overnight in a hospital in the past 12 months
HUI	Health Utility Index
Self-assessed health status	an indicator variable taking the value of one if the respondent classified his/her own health as excellent or very good and zero if good, fair or poor

Notes: For number of medications and physician visits, a very small number of respondents reported extreme values, e.g., more than 100 GP visits in the past 12 months. Therefore, we have truncated these variables at the 99.5 percentile value. Household income groups are provided as one categorical variable by Statistics Canada. It has missing values sometimes and we use the income category from last cycle as a proxy.

Appendix B

Conley and Taber (2011) use a two-step estimator to estimate the treatment effects in a difference-in-differences framework. The estimating equations are as follows:

$$y_i = \lambda_{pt} + \gamma X_i + \varepsilon_i \qquad (1)$$
$$\lambda_{pt} = \alpha D_{pt} + \theta_p + \upsilon_{pt} \qquad (2)$$

where i index an individual who is observed in group p at a time period t , y_i is the dependent variable for individual i , D_{pt} is the policy variable, and X_i is individual-specific regressors. We estimate λ_{pt} in equation (1) using a regression of y_i on a full set of indicators for group*time and X_i. In the second step, the estimated λ_{pt} are used as the outcome variable in equation (2). The inference is performed using the Conley and Taber (2011) method. We tried four models in the first step estimation: Negative Binomial (NB) and Poisson with individual fixed effects, OLS and fixed effects, as is shown in the table below. Therefore, we view this not as a substitute for our preferred model specification but rather a complement to check the robustness of the results.

Appendix Table 2.10: Two-step estimates and consistent confidence intervals

Dependent variable			
Panel A: drug and other insurance			
	Two Step (First step OLS)	Two Step (First step fixed effects)	N
Drug insurance	0.2128	0.2166	31059
	[0.1621, 0.2741]	[0.1690,0.2717]	
Dental insurance	-0.0105	0.0003	31064
	[-0.0410, 0.0267]	[-0.0393, 0.0334]	
Eye-glasses insurance	-0.0688	-0.0584	30741
	[-0.0961, -0.0100]	[-0.0807, -0.0129]	
Supplemental hospital insurance	0.0535	0.0642	29597
	[0.0184, 0.0815]	[0.0439, 0.0849]	
Any non-drug supplemental insurance	0.0514	0.0615	30808
	[0.0200, 0.0720]	[0.0250, 0.0902]	
Panel B: medication use and physician visits			
	Two Step (First step NB)	Two Step (First step fixed effects Poisson)	N
Total number of medications used in the previous month	0.0953	0.1161	40354
	[0.0219,0.1534]	[0.0662,0.1717]	
Total number of medications used in the previous two days	-0.0341	0.0475	32391
	[-0.0580,0.0032]	[0.0130,0.0880]	
Total number of GP visits in the previous 12 months	0.0857	0.0969	40354
	[0.0132,0.1582]	[0.0612,0.1480]	
Total number of specialist visits in the previous 12 months	0.0122	0.1036	40354
	[-0.0324,0.0992]	[0.0168,0.2007]	
Panel C: indicators of medication use, GP visits, specialist visits and hospitalization			
	Two Step (First step OLS)	Two Step (First step fixed effects)	N
Medication used in the previous month (yes/no)	0.0353	0.0288	40354
	[0.0167,0.0678]	[0.0081,0.0478]	
Medication used in the previous two days (yes/no)	0.0032	0.0046	40354
	[-0.0150,0.0228]	[-0.0107,0.0276]	
GP visits in the previous 12 months (yes/no)	0.0102	-0.0006	40354
	[-0.0037,0.0306]	[-0.0216,0.0237]	
Specialist visits in the previous 12 months (yes/no)	0.0008	0.0003	40354
	[-0.0144,0.0248]	[-0.0188,0.0260]	
Inpatient hospital stay in the previous 12 months (yes/no)	-0.0038	0.0022	40354
	[-0.0180,0.0074]	[-0.0033,0.0138]	
Panel D: health outcomes			
	Two Step (First step OLS)	Two Step (First step fixed effects)	
HUI	0.0098	0.0080	40354
	[0.0057, 0.0203]	[0.0026,0.0159]	
Self-rated health status	0.0218	0.0113	40354
	[0.0019,0.0436]	[-0.0114,0.0261]	

Notes: The 80% confidence intervals given in brackets are constructed using the method by Conley and Taber (2011).

Chapter 3

Delisting Eye Examination from Public Health Insurance: Evidence from Canada

3.1 Introduction

Public health insurers have to make decisions on what health care services should be publicly funded given the resource constraints. The decision-making process varies across institutional settings and services. Flood et al. (2006) analyze the process regarding what services should be publicly funded in Canada. Public health insurers often delist services to contain health care costs. In essence, delisting shifts the costs from the public sector to the private sector and makes access to the delisted services conditional on an individual's ability to pay. This can raise equity concerns that the vulnerable population may have compromised access to the services. Evidence for the causal impact of such listing and delisting policy changes on patients and providers can help to inform public decision making.

Beginning in the early 1990s, provincial governments in Canada started to delist a series of health care services, including eye exams, from the basket of publicly funded services. Many of these decisions were aimed at containing public sector costs without fully understanding how they would influence patient access to health services and health outcomes. There is a limited literature studying the effects of delisting health care services in a public health system. Stabile and Ward (2006) examine the delisting effects across a range of services from only the patient side, showing that the effects are not uniform across services or across populations. Two other studies document that after the delisting of eye exams in Ontario, eye care utilization decreased among socially disadvantaged people or patients with diabetes (Jin et al. 2012; Kiran et al. 2013). Analysis of delisting chiropractic and physical therapy services can be found in Sweetman and Yang (2012) and Landry et al. (2006).

More broadly, there is a large research literature looking at the effects of user charges/cost-sharing (Schokkaert and Van De Voorde 2011; McGuire 2012). The existing evidence consistently shows that user charges have a negative effect on health care utilization. The RAND Health Insurance Experiment showed that the price elasticity of health care demand was around -0.20 (Newhouse 1993).[23] Some Canadian studies from the 1970s on the Saskatchewan user charges for physician services showed that utilization declined among lower income groups but increased among higher income groups (Beck 1971; Beck 1974; Beck and Horne 1980). A set of recent studies have examined cross-effects of user charges or insurance coverage, with a focus on the cross-effects of coverage for prescription drugs (Goldman and Philipson 2007; Chandra et al.

[23] Aron-Dine et al. (2013) discuss the application of the experimental estimates in practice.

2010). With multiple health care services, changes in user charges for one service may influence demand for other services. The system of differential user charges for different health care services can be designed to induce the choice of a more efficient health care package (Schokkaert and Van De Voorde 2011). From a policy perspective, this system-wide approach of optimal insurance design should not be confined to cross-effects between services. For the same service, indirect effects may exist when differential user charges are applied to different population groups who share the same health service providers in a system. Strategic behavioral responses by providers and patients may generate unexpected consequences. Therefore, it is necessary to assess the impacts of user charges using a systematic approach.

Delisting policies can also affect the labour market outcomes of service providers. Sweetman and Yang (2012) investigates delisting from the supply side. They find that delisting chiropractic services decreases chiropractors income by 15 percent and that they work one additional week per year. If the delisting policy leads to a notable decrease in demand, service production or providers' work effort will adjust. In the case of delisting eye exams, before delisting, optometrists were reimbursed for publicly insured services based on a common fee schedule.[24] After delisting, optometrists provide services to public patients who are still covered by provincial governments, and to private patients who either pay out of pocket or are covered by private insurers. Prices charged to private patients may drop possibly due to the decline in demand after delisting. On the other hand, prices may go up since the monopsony power of the government is partially removed. A survey conducted by the Consumers' Association of Canada found an

[24] In some provinces (e.g., Alberta, Manitoba and Ontario), the fees for publicly funded services are negotiated between the provincial government and optometrist association.

average 30% increase in the cost of routine adult eye exams five months after this service was delisted in Alberta (Consumers' Association of Canada (Alberta) 1996). Therefore, the policy impact on optometrists' work hours, service provision and income is an empirical question. It is useful for health human resource planning purposes to understand the impact of delisting on providers' labour market outcomes.

In this study we investigate the impact of delisting routine eye exams on patient eye care utilization and optometrists' labour market outcomes. We exploit the policy changes across provinces and over time to estimate the impact of delisting eye exams from the supply- and demand- sides. Eye examinations are an interesting example for studying delisting in Canada. Firstly, the clear-cut delisting policies implemented in different provinces at different periods provide credible sources of exogenous variation, which can help to identify the causal impact of the policy changes. Secondly, non-routine-exam eye care services provided by ophthalmologists and other physicians are publicly funded in Canada. Shifting primary eye care financing into the private sector may create intended or unintended interactions between the public and private sectors. Finally, unlike the delisting of chiropractic and physical therapy services, routine eye exams do not have close substitutes. This may cause the policy impact of delisting eye exams to be different from that of delisting other health services.

Demand side analysis using the National Population Health Survey and Canadian Community Health Survey data suggests that delisting of eye exams for the working age population decreased the probability of using eye care among this population group. However, the number of visits among the working population who continued to use eye care services was not affected. Further, the negative impact at the extensive margin was large for the low income population. We also find suggestive evidence that the delisting policies targeted at the working age population were associated with increased eye care utilization among the elderly patients. Using the optometrist sample from the Canadian census data we find that the delisting decreased optometrists' weekly work hours and increased their annual work weeks. There was no statistically significant effect on optometrists' income.

The paper is organized as follows. In section 2, we describe the institutional background for eye health care in Canada and the delisting policies since the 1990s. Section 3 describes the data and section 4 gives a descriptive analysis. The empirical strategy is discussed in section 5. We present the empirical results in section 6. Finally, the conclusions are drawn in section 7.

3.2 Institutional Background

The eye care team in Canada comprises ophthalmologists, optometrists and opticians (Canadian Ophthalmological Society 2013).[25] Ophthalmologists are medical doctors who complete specialized residency training in eye diseases. They perform comprehensive eye exams, conduct surgery, and prescribe and administer medication. Access to specialist eye care by ophthalmologists usually requires a referral from optometrists or other health professionals.[26] Optometrists usually serve as the entry point into the eye health care system. They examine patients' eyes, diagnose vision problems and prescribe treatment to conserve, improve and correct vision and other ocular disorders. Optometrists usually refer to ophthalmologists or other health professionals patients with eye diseases or other

[25] Other health professionals also provide eye care services, including family physicians, emergency physicians, pediatricians, orthoptists and ophthalmic medical assistants.
[26] For example, in Manitoba ophthalmologists only accept new patients on referral from an optometrist or physician (Manitoba Association of Optometrists).

conditions that require medical treatment. Optometrists require seven to eight years of post-secondary education to obtain their professional designation, Doctor of Optometry (OD) (Canadian Association of Optometrists 2013). To practice optometry in Canada, a candidate must meet the licensing requirements of a provincial or territorial college of optometry. Opticians are licensed professionals trained to design, fit, and dispense eyeglasses, contact lenses, low vision aids, and prosthetic ocular devices. Family physicians and other health care providers also play an important role in eye care. However, given the physician shortage in Canada and the requirements of specialized equipments to perform all but the most basic eye care, the bulk of eye care is likely to be carried by optometrists and ophthalmologists.

Canadian Medicare finances medically necessary eye care services provided by ophthalmologists and other physicians, but public coverage for eye care provided by non-ophthalmologists, such as routine eye exams, varies across population groups and provinces. Table 3.1 describes the policy changes of public coverage for eye exams from 1990 to 2010. Eye exams for the prime working age population aged 20 to 64 were fully delisted in eight provinces during the study period 1991 to 2010. In three (British Columbia, Manitoba and Ontario) out of the eight provinces, restrictions on the frequency of publicly covered eye exams – partial delisting – were implemented before the full delisting. In the other five provinces, there were restrictions on frequency at the beginning of the study period. During this time period eye exams were never publicly covered for all population groups in Prince Edward Island; eye exams for the prime working age population were never covered in New Brunswick. Ontario was the last province to delist eye exams for this population group, doing so in November 2004.

Children's eye exams have always been covered by the public plan in six provinces: British Columbia, Alberta, Saskatchewan, Manitoba, Ontario and Quebec; they were fully delisted in Newfoundland and New Brunswick in the early 1990s. In Nova Scotia, eye exams for children aged 10 to 19 were delisted in 1997 but were still covered for children aged 9 and younger. For the elderly population aged 65 and over, eye exams are publicly covered in British Columbia, Alberta, Manitoba, Ontario, Quebec and Nova Scotia; they were fully delisted in Saskatchewan and Newfoundland in the early 1990s and never covered in New Brunswick.

Publicly covered eye exams were free for patients at the time of use before delisting. After they were delisted from the public plans, patients had to pay fully out-of-pocket or potentially pay co-payments if they had private insurance covering eye exams. The increase in payments/co-payments is expected to exert a negative influence on utilization. It should also be noted that eye exams are preventive care that may help to diagnose eye diseases. If delisting decreases eye exam utilization, specialist eye care use may also decrease in the short run because patients are less likely to be seen and diagnosed with eye problems, yet it may go up in the long run as patients' eye conditions worsen. On the other hand, patients may substitute publicly covered non-exam eye care for eye exams. Given the referral system for specialists and the shortage in ophthalmologists in particular, the substitution effect may be minimal.

From the supply side, the decrease in number of public patients after the delisting may be partially compensated by an increase in private patients. If there is excess capacity after the delisting, optometrists can see more public patients who are still covered or provide more services to them. Therefore, the overall impact of delisting on their work hours and earnings is an empirical question.

3.3 Data

The 1994/1995 to 1998/1999 public use files of Statistic Canada's National Population Health Survey (NPHS) and 2000/2001 to 2009/2010 Canadian Community Health Survey (CCHS) are employed to analyze the effects of delisting on patient eye care utilization. The NPHS is a biennial survey conducted by Statistics Canada. The sample was created by first selecting households and then, within each household, choosing one member 12 years of age or older to be the longitudinal respondent. The NPHS asks a series of questions related to health status, use of health services, chronic conditions and activity restrictions, and demographic and socio-economic status. The first three cycles of NPHS (1994/1995, 1996/1997 and 1998/1999) were both cross-sectional and longitudinal. The public use NPHS data we employ for this analysis were cross-sectional. Beginning in 2000/2001, the NPHS became strictly longitudinal and the cross-sectional sample was carried on by CCHS. NPHS and CCHS have similar survey designs and cover the same population.

Eye care utilization is measured by the number of eye care visits in the previous 12 months. The survey question asks "In the past 12 months, how many times have you seen, or talked on the telephone, about your physical, emotional or mental health with: a family doctor or general practitioner, an eye specialist (such as an ophthalmologist or optometrist), any other medical doctor, etc.?" If the delisting policies lead to any substitution between routine eye exams and other eye care visits, the response to this question would only measure the net effects on eye care utilization.

The policy changes varied across age groups and provinces over time as shown in Table 3.1, which covers the time period 1990-2010. The NPHS started from 1994/1995 so we can only look at policy changes that occurred during the period 1994 to 2010. Our main analysis focuses on the prime working age population aged 20 to 64 since there were several delisting policy changes for them during the study period (shown in Appendix Table 3.10).[27]

The primary data for supply side analysis are the optometrist samples from Canada's 1991, 1996, 2001, and 2006 censuses. Detailed occupational and educational information, as well as information on labour market outcomes are collected in the census. The sample for our analysis is restricted to individuals aged 25 to 64 who reported their occupation as "optometrist" and whose education includes a degree in "medicine, dentistry, veterinary medicine or optometry". The outcomes for optometrists include annual earnings (wages plus positive self-employment earnings), work weeks and hours of work in the reference week.

Another source of information is administrative data from the Ontario Ministry of Health and Long-term Care. This dataset includes all eye care services billed by optometrists, ophthalmologists and other health care providers for the Ontario population from 2003 to 2010. The data contain 19 months before the delisting and several years after Ontario's delisting in November 2004. A new fee code for optometry services was added in March 2007; therefore we focus on data between November 2004 and February 2007. There were 1,515 optometrists and 457 ophthalmologists who billed the

[27] There were no policy changes for the elderly population and only one for children aged 10 to 19 during the study period. The cut-off age of the definition of children varied across provinces (Table 1). In addition, the data only cover population aged 12 and over, which provides a sample of children that is too small for an analysis of children.

government for publicly covered eye care services. We deleted 184 optometrists and 53 ophthalmologists who billed only in the before- or after-periods. To focus on providers in active practice, we further dropped 36 optometrists and 21 ophthalmologists who billed for less than half of the 47 months in our data. The final sample contains 1,295 optometrists and 383 ophthalmologists. Outcome measures for optometrists and ophthalmologists are the number of publicly covered patients and services per month, and the proportion of patients with more than one visit per month or per year.

3.4 Descriptive Analysis

3.4.1 Inter-provincial Comparison

Table 3.2 shows eye care utilization for the population aged 20 to 64, with no adjustment for age, during the study period 1994 to 2010. On average, the unadjusted annual number of eye care visits increased by 0.047 visits or 11.7% across the decade. The proportion of people who visited an eye care provider increased from 32.1% to 35.5%; the number of visits among eye care users also increased modestly. Table 3.3 presents the sample means for basic demographic and socioeconomic characteristics for eye care users and non-users. Compared to the non-users, those who used eye care are older and more likely to be female; the users also tend to have higher education and income status.

Figure 3.1 depicts eye care utilization for the population aged 20 to 64 before and after the delisting in Ontario, Manitoba, Alberta and British Columbia. Eye care utilization in all other provinces is also shown for comparison. After the delisting policies, a slight decrease in eye care visits is observed in all four provinces. The decline of eye care visits in Ontario, Manitoba and Alberta seems to be temporary. Figures 3.2

and 3.3 show the extensive and intensive margins of eye care utilization respectively. Patterns of the eye care visit indicator in Figure 3.2 are similar to those of total eye care visits in Figure 3.1. However, there are no strong patterns for the intensive margins of eye care utilization.

3.4.2 Descriptive Analysis – Ontario

Effective November 1, 2004, in Ontario, routine eye exam services provided by both optometrists and physicians for patients aged 20 to 64 were no longer publicly insured. Patients aged 20 to 64 who have certain medical conditions (diabetes mellitus, type 1 or 2, glaucoma, cataract, retinal disease, amblyopia, visual field defects, corneal disease and strabismus) remain eligible for a "major eye exam" every 12 months rendered either by an optometrist or a physician.[28] Patients of this age category may also be insured for a major eye exam if they have a valid "request for eye examination requisition" completed by a physician or registered nurse holding an extended certificate of registration. Patients under age 20 or over age 64 continue to be eligible for periodic oculo-visual assessments once every 12 months and for oculo-visual minor assessments.

In Ontario, publicly covered optometry services include periodic oculo-visual assessments and minor/additional assessments. The sample mean number of publicly funded patients and eye exam services by optometrists from April 2003 to February 2007 is plotted in Figure 3.4. (Note that only those optometry services that are covered by the provincial government are shown in the figure.) If a patient pays out of pocket (or is covered by private insurers) for the service, it is not captured in Figure 3.4. There is a sharp decrease in the number of public patients aged 20 to 64 after the delisting in

[28] Two new conditions, recurrent uveitis and optic pathway disease, were added to the list of conditions in July 2008.

November 2004. There appears to be a slight increase in the number of patients aged 65 and over and services provided to them after the delisting. The trends for those aged 19 and younger are rather flat during this time period. The pattern for number of services is similar to that of number of patients.

Figure 3.5 depicts the proportion of patients who had more than one visit (vs. only one visit) for an optometrist. It should not be surprising to observe a large increase in the proportion of patients with more than one visit among those aged 20 to 64. Before the delisting, optometrists' public patients include the whole population aged 20 to 64; after the delisting, public patients among this age group are mostly those who have at least one of the eight medical conditions stated above and should have higher utilization than the general working age population. However, the increase in service utilization among the elderly is rather puzzling since they are publicly covered before and after the policy change. It is unlikely that population aging can generate such a sudden increase in eye care utilization. One possible explanation is that the released capacity of optometrists after the delisting is directed to these public patients.

To further quantify changes in the number of public patients and services by optometrists in Ontario, we report their before-and-after sample means in Table 3.4. The upper panel of Table 3.4 presents the number of patients and their frequency of visits to optometrists and the lower panel presents the number of services and a breakdown by service types. Before the delisting, each month optometrists served, on average, 53 young patients age 19 or less, 132 adult patients age 20 to 64, and 50 elderly patients age 65 or over. After the delisting, optometrists were serving fewer young patients and many fewer adult patients but more elderly patients per month (all of whom are public patients). The average number of publicly funded adult patients per month decreased substantially by 111 (84%) and number of young patients decreased by 3 (6%) after the delisting; in contrast, the number of elderly patients increased by 5 (9%). A much larger proportion of publicly covered adult patients made more than one visit to the optometrist in a month or year after the delisting. As we mentioned above, adult patients are mostly drawn from those with at least one of the eight medical conditions. There is also a sizable increase in the proportion of elderly patients who made more than one visit in a month or year. A similar pattern is observed for the number of total services per month. When we look at different types of services by optometrists, for young patients we see a decrease in the number of periodic oculo-visual assessments yet see an increase in the number of follow-up minor assessments. For elderly patients, periodic oculo-visual assessments increased by around 10 percent and minor assessments by 20 percent.

The descriptive analysis reveals some indirect positive effects of the delisting policy on the publicly covered elderly patients. The results may indicate behavioral responses of optometrists or the availability effect after the delisting, or a mixture of the two effects. The evidence that optometrists saw more elderly patients when services for the adult patients were delisted is closely related to an availability effect. Any released extra capacity after the delisting policy improved the availability of optometry services among the elderly population. They may demand more optometry services due to lower travelling or time cost. On the other hand, the rise in the frequency of visits, especially that of follow-up visits, suggests evidence of supplier influence.

For ophthalmologists in figures 3.6 and 3.7, there is a small decrease in the number of adult patients but no notable change in the number of services provided to them after the policy change. There is an upward trend for the number of patients aged 65 and over and services provided to them. In Figure 3.7, we observe a small increase in the proportion of adult patients with more than one visit per month after the delisting.

Table 3.5 reports the before-and-after comparison for ophthalmologists. Ophthalmologists served fewer young and adult patients but more elderly patients after the policy change. We also observe an increase in the proportion of patients with more than one visit per month among all age groups. The increase in the number of services is mostly concentrated among consultations and diagnostic and therapeutic procedures for the elderly patients, and diagnostic and therapeutic procedures for the non-elderly adult patients.

3.5 Empirical Strategy

A difference-in-differences approach is employed to compare outcomes in provinces with and without delisting policy changes over time. The estimation equation is as follows:

$$y_{ipt} = f(\alpha + \beta_1 partial_{pt} + \beta_2 full_{pt} + \delta prov_p + \varphi year_t + \gamma X_{ipt} + \varepsilon_{ipt})$$

where $prov_p$ and $year_t$ are fixed effects for province and year respectively, and X_{ipt} is a set of control variables for individual i of province p in year t. The *partial* delisting variable is set to one in the periods in each province after the partial delisting policy change and before the full delisting policy change, and to zero otherwise (the partial delisting policies were implemented before the full delisting policies). The indicator variable, *full*, is set to one in the periods in each province after the full delisting policy change and to zero otherwise. The *partial* and *full* delisting variables are set to zero during all years for provinces without a policy change in the study period.

The two indicator variables are defined according to Table 3.1 for the supply-side equation. The treatment groups are optometrists in provinces with a partial and/or full delisting policy change and the comparison groups are optometrists in provinces without a policy change during 1991 to 2006. Optometrists in provinces before any policy change can also serve as comparison groups. The two *partial* and *full* indicators are defined according to Appendix Table 3.10 for the demand-side equation. People aged 20 to 64 in provinces with a partial and/or full delisting policy change during 1994 to 2010 serve as the treatment groups; people of the same age category in provinces without a policy change serve as the comparison groups.[29] A detailed description of *partial* and *full* variables is described in Appendix B. For the demand side analysis, we observe only one partial delisting policy change during 1994 to 2010. It is difficult to perform inference in this case. Therefore, we focus only on the full delisting policy effects while including the partial delisting variable as an independent variable. The *partial* and *full* delisting variables refer to policy changes that are different in nature, not “small” or “large” price changes. The estimated coefficients measure the average impact of each policy type.

The dependent variable y is the supply side or demand side outcome of individual i of province p in year t. The functional form for the equation is chosen to account for the nature of the data, which will be discussed below. Dependent variables for the demand side include a binary variable indicating whether the individual used eye care in the previous 12 months and, for a user, the number of eye care visits. Individual characteristics we control for are age categories (20 to 24 years, 25 to 29 years, …, and

[29] The comparison groups are a mixture of people who were always treated in Saskatchewan, Quebec, Nova Scotia and Newfoundland and people who had never been treated in New Brunswick and PEI. Since the difference-in-differences approach uses within-province variation for the estimation, the differences between the always treated and never-treated groups are absorbed into the province fixed effects.

60 to 64 years), sex, education levels and income, all of which are specified as categorical variables. Education has four categories: less than secondary school graduation, secondary school graduation, some post-secondary and post-secondary graduation. Income variables are household income quintile indicators provided by the survey data.[30] A two-part model is used to estimate the extensive and intensive margin outcomes: a probit model for the usage indicator and a zero-truncated poisson model for the conditional use.

For the supply side analysis, the dependent variables are the logarithm of annual earnings, annual working weeks and weekly work hours. Individual characteristics (X_{ipt}) include age, age squared, and sex and marital status indicators. All of the supply side equations are estimated by ordinary least squares (OLS).

Two specifications are employed for each equation: one base model only controls for province and year effects, which estimates an unconditional policy effect; the other further controls for individual characteristics. Including additional individual characteristics variables can allow for compositional changes in the treatment and comparison groups and improve efficiency by reducing the variance of the error term.

Identification in difference-in-differences estimation relies on the common trend assumption, i.e., that the trends in the outcomes of the treatment and comparison groups would be similar in the absence of the intervention. There are several reasons to believe that the common trend assumption holds in our analysis. First, the institutional context of eye care is similar across provinces within Canada. Publicly covered eye exams were free

[30] Another income variable measuring household income levels at 5k or 10k intervals is available in the data. There is essentially no change in the estimates when this income measure is used in one robustness check.

to patients before the delisting (except PEI) and specialist eye care by ophthalmologists were also free and publicly funded in each province. Second, the motivation of the delisting policies was cost containment, which was the same across provinces. Finally, optometrists in each province were paid by a uniform fee schedule for services provided to the publicly covered population and charge a price to the private patients.

Some recent studies have pointed out inference problems in different-in-differences estimation (Bertrand et al. 2004; Donald and Lang 2007). In most cases, the variable of interest varies at a group level, such as across states or provinces and over time. The standard errors computed by conventional estimation methods are too small if the error terms are correlated within groups. One approach is to cluster at the group level. When there are a small number of groups, however, this method tends to over-reject the null hypothesis because it is based on an asymptotic framework that requires a large number of clusters. Several approaches have been proposed to deal with the inference problem with few groups. Donald and Lang (2007) proposed estimation using group means and adjusting for degrees of freedom in inference. Cameron et al. (2008) showed that the wild cluster bootstrap-t procedure performs well in the case of ten clusters. In our analysis, with ten clusters, we employ the bootstrap-t procedure by Cameron et al. (2008) to compute p-values based on the bootstrap distribution.[31]

3.6 Estimation Results

3.6.1 Eye Care Utilization

Results for Population Aged 20 to 64 (Inter-provincial Comparison)

[31]The approach bypasses the generation of standard errors because the t-statistic (not the standard error) is bootstrapped.

Table 3.6 presents results for the eye care visit indicator and eye care utilization of users from the difference-in-differences specification. Columns 1 and 2 present marginal effects at the mean from probit models for the eye care visit indicator; columns 3 and 4 present marginal effects from zero truncated poisson models for eye care visits of users. Columns 1 and 3 are the base models including province and year dummy variables, while columns 2 and 4 further control for individual characteristics. The results show that the full delisting policy had a sizable negative effect on the extensive margin of utilization but no effect on the intensive margin. Full delisting decreased the likelihood of visiting an eye care provider by 5 percentage points (or about 15%). The estimates are very similar across columns, showing that the results are robust to the introduction of additional variables. The differential policy impacts on the extensive and intensive margins of utilization are as expected. Access to ophthalmologic services usually requires referral from an optometrist (or physician) who serves as the entry point into the eye health care system. Delisting eye exams, usually provided by an optometrist, may make it more difficult to enter into the eye care system.

Focusing on other control variables, males are less likely to visit an eye care provider. Level of education is positively related to the probability of visiting an eye care provider but not strongly associated with utilization among users. There is a clear income gradient for the probability of visiting an eye care provider, but conditional on positive utilization, those with higher income use fewer eye care services.

Differential Effects by Income Groups

Table 3.7 presents policy effects across household income quintile groups. Three income indicators are interacted with the full delisting indicator to examine heterogeneous effects. At the same time a quadratic in imputed income (the midpoints of income categories) is included to allow for main income effects on eye care utilization. Column 1 reports marginal effects at the mean from a probit model for the extensive margin. Estimated marginal effects at the mean from a zero truncated poisson model for the intensive margin are shown in column 2. The full delisting had a statistically significant negative effect on all income groups at the extensive margin. It reduced the likelihood of visiting an eye care provider by 6.3 percentage points for the low income group, 5.8 percentage points for the middle income group and 4.8 for the high income group. The hypothesis that the effect of full delisting is the same for the five income quintile groups is rejected at less than the 5% level (the F-statistic is 8.48). Among eye care users, full delisting reduced utilization for those with below average income but increased it for those with higher income. The hypothesis of equal policy effects across income quintiles is rejected at less than the 1% level although the estimates are not statistically significant and have large standard errors.

Spillover Effects

The simple before-and-after analysis from Table 3.4 suggests that the full delisting policy for the working age population in Ontario was associated with increased eye care utilization among the publicly covered elderly patients. To further examine this issue across provinces, we estimated the equation in section 5 for the elderly population using the CCHS 2000/01 to 2009/10 data.[32]

As we mentioned above, there were no policy changes for the elderly population during the study period. If there were no spillover effects, delisting eye exams for the

[32] Here the focus is on the spillover effects of the full delisting policy so we use the 2000/01 to 2009/10 data which covers two full delisting policy changes.

working age population would not directly influence eye care utilization among the elderly population. However, if there is any availability effect and/or supplier behavioural response, the delisting would increase utilization among the elderly.

Table 3.8 presents the estimation results for the elderly population. We find little evidence of spillover effects on the extensive margin for the elderly population. Conditional on positive use, there is a small positive, yet statistically insignificant spillover effect on eye care visits among the elderly population after the delisting. When we further control for the optometrist-population and ophthalmologist-population ratios, the positive effect gets larger and statistically significant. We also interacted the full delisting dummy with provider-population ratios. The positive sign of the coefficient on the interaction term between the full delisting dummy and optometrist-population ratio indicates that the spillover effect in provinces with high density of optometrists is larger than the effect in provinces with low density of optometrists. This suggests that optometrist response is more important since the spillover effect is larger in provinces with fewer access problems.

3.6.2 Optometrists

Optometrists can adjust their work hours and weeks in response to delisting. Table 3.9 shows the estimated policy effects on weekly work hours (columns 1 and 2), annual work weeks (columns 3 and 4) and earnings (columns 5 and 6). Partial delisting had little effect on weekly hours while the full delisting decreased weekly hours by two and a half (6%). On the other hand, both partial and full delisting tend to increase working weeks by one and a half to two weeks per year. One explanation is that optometrists may work more flexibly by increasing weeks and decreasing weekly hours after the delisting. Results from columns 5 and 6 show that there was no statistically significant effect on annual earnings.

The policy impacts on work hours and earnings are relatively small compared to the substantial decrease in the number of public patients aged 20 to 64 before and after the delisting as shown in the descriptive Table 3.4 for Ontario. It is possible that private patients compensated for a large decrease in use among public adult patients after the delisting. At the same time optometrists may serve more publicly insured elderly patients and provide more services to them, which seems to be the case in Ontario (shown in Table 3.4).

3.6.3 Sensitivity Analysis

We perform several robustness checks to evaluate the sensitivity of the main results in Table 3.6. First we use two alternative income measures: income level indicators and imputed income at the midpoints of the category (a quadratic form). The regression results are essentially unchanged. Second we add optometrist-population and ophthalmologist-population ratios as control variables. This specification aims to control for the supply-side resources. The coefficients on the additional variables are very small and never statistically significant. There are no meaningful changes in the estimates. Finally we employ aggregate province-level data for the estimation and the results are shown in Appendix Table 3.11. The key conclusions from Table 3.6 hold up well.

For the supply-side results in Table 3.9, robustness checks have also been conducted to evaluate the sensitivity of the results. Including the optometrist-population ratio does not change the estimates very much although this additional variable is negatively correlated with income. Inference and signs of estimates from aggregate province level data regressions are consistent with those in Table 3.9.

3.7 Discussion and Conclusion

Given the budget constraints, public health insurers sometimes delist health care services at the margin of public health service basket. For policy purposes, it is useful to develop a framework for understanding how delisting policies influence various stakeholders. This paper sets out to evaluate the supply-side and demand-side impacts of delisting one specific health care service – routine eye exams. We empirically investigate the effects of delisting eye exams from Canada's public plans on patient eye care utilization and on providers' labour market outcomes.

Demand side analysis indicates that the full delisting decreased the likelihood of using eye care among the working age population by 5 percentage points (or about 15%). Further, the negative impact at the extensive margin was large for the low income population. Considering the preventive care nature of eye exams, the negative policy impact may raise concerns about the increase of eye care utilization in the long run. We find no evidence of statistically significant effect on eye care among the working age population who continued to use eye care.

We find suggestive evidence that the delisting policies for the working age population were associated with increased eye care utilization among the publicly covered elderly patients. In particular, the evidence from Ontario implies that the elderly made more follow-up visits to an optometrist after the delisting. The findings have important implications for policy makers. When public insurers make the decision on de-insuring a health service for some sub-population groups, they need to take into account spillover effects on other population groups. A system-wide approach of evaluating delisting policy is very important from a policy perspective.

Supply side analysis on optometrists' labour market outcomes shows that the delisting of eye exams decreased optometrists' weekly work hours and raised their work weeks but had no statistically significant effect on their income. This is different from the findings for chiropractors. Sweetman and Yang (2012) find that delisting chiropractic services reduced chiropractors' earned income by 15%, increased their weeks of work, but had no effect on their work hours. Differences in the scope of the two delisting policies may explain some of the discrepancy since the delisting of eye exams applied only to the prime working age population in most cases while delisting chiropractic services applied to the whole population.

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Table 3.1: De-listing eye examination from provincial health plan in Canada (1990 to 2010)

Province	Eye exam coverage in 1990	Delisting eye exam 1990 to 2010	
		Partial delisting	Full delisting
British Columbia	Covered for all ages	In 1993 eye exams were limited to once every year for children under 19 and seniors over 65, and once every two years for adults 19 to 64	In November 2001 adults aged 19 to 64 were no longer covered; no change for children under age 19 and those aged 65 and over
Alberta	Covered for all ages; once every year		In December 1994 adults aged 19 to 64 were no longer covered; no change for children under age 19 and those aged 65 and over
Saskatchewan	Covered for all ages; once every year for children under 18 and once every two years for adults over age 18		In May 1992 (effective June 1, 1992) adults over age 18 were no longer covered; no change for children under 18
Manitoba	Covered for all ages	January 1993 eye exams were limited to once every year for children under 18, and once every two years for adults over age 18	In April 1996 adults 19 to 64 were no longer covered; no change for children under age 19 and those aged 65 and over
Ontario	Covered for all ages; once every year	In April 1998 eye exams were limited to once every two years for adults 20 to 64	In November 2004 adults 20 to 64 are were no longer covered (except patients with diabetes, cataracts, glaucoma etc); no change for children under age 20 and those aged 65 and over
Quebec	Covered for all ages; once every year		In 1992 adults 18 to 41 were no longer covered; in 1993, adults 42 to 64 were no longer covered; no change for children under age 18 and those aged 65 and over
Nova Scotia	Covered for all ages; once every two years		In July 1992 adults 19 to 64 were no longer covered; In January 1997 children 10 to 19 were no longer covered; no change for children under age 10 and those aged 65 and over
New Brunswick	Coverage for children up to 18		July 1992 fully delisted
Newfoundland	Covered for all ages; once every year		April 1991 fully delisted
PEI	Not covered		no change

Sources: provincial associations of optometrists, and government websites.

Table 3.2: Eye care utilization for individuals aged 20 to 64 (1994/95 to 2009/10)

	1994/95	1996/97	1998/99	2000/01	2003	2005	2007/08	2009/10
Eye care visit indicator (1= visit; 0 = no visit)	0.321	0.322	0.335	0.343	0.351	0.356	0.353	0.355
	(0.467)	(0.467)	(0.472)	(0.475)	(0.477)	(0.479)	(0.478)	(0.478)
No. of eye care visits per capita	0.401	0.405	0.429	0.427	0.448	0.446	0.455	0.448
	(0.799)	(0.807)	(0.875)	(0.802)	(0.860)	(0.826)	(0.848)	(0.811)
No. of eye care visits for users	1.250	1.260	1.282	1.244	1.277	1.252	1.289	1.264
	(0.963)	(0.974)	(1.092)	(0.925)	(1.024)	(0.952)	(0.982)	(0.908)
N	12,134	42,236	10,311	78,487	73,649	75,627	72,801	66,086

Notes: Data are from National Population Health Survey (1994/95 to 1998/99) and Canadian Community Health Survey (2000/01 to 2009/10). Standard deviations are in parentheses. Sampling weights are used.

Table 3.3: Characteristics of eye care users and non-users aged 20 to 64

	Users	Non-users	Total
Age	42.774	39.961	40.924
Male	0.451	0.530	0.503
Married	0.702	0.673	0.683
Less than secondary school graduation	0.125	0.155	0.145
Secondary school graduation	0.151	0.181	0.171
Some post-secondary education	0.194	0.202	0.199
Post-secondary graduation	0.530	0.462	0.485
Lowest income	0.078	0.100	0.092
Lower middle income	0.103	0.125	0.118
Middle income	0.197	0.227	0.217
Upper middle income	0.324	0.316	0.319
Highest income	0.297	0.232	0.254
N	152,451	278,880	431,331

Notes: Data are from National Population Health Survey (1994/95 to 1998/99) and Canadian Community Health Survey (2000/01 to 2009/10). All of the variables above are indicator variables. Age is imputed at the midpoints of nine age categories (20 to 24 years, 25 to 29 years, … and 60 to 64 years).

Table 3.4: Publicly covered patients and services by optometrists before and after the delisting in Ontario

(March 2003 to February 2007)

	Number of patients per month				Proportion of patients with more than one visit per month				Proportion of patients with more than one visit per year			
Patient age	Before	After	Difference	% change	Before	After	Difference	% change	One Year Before	One Year After	Difference	% change
<=19	53.367	50.129	-3.238**	-6.07%	0.015	0.021	0.006***	40.00%	0.054	0.065	0.011	19.84%
	(5.671)	(6.461)	(1.830)		(0.001)	(0.003)			(0.043)	(0.055)		
20 to 64	132.443	21.178	-111.265***	-84.01%	0.040	0.078	0.038***	95.00%	0.112	0.173	0.061	53.86%
	(24.323)	(3.079)	(4.627)		(0.003)	(0.015)			(0.076)	(0.120)		
65+	49.575	54.124	4.549**	9.18%	0.044	0.072	0.028***	63.64%	0.182	0.206	0.024	13.03%
	(5.210)	(7.191)	(1.924)		(0.004)	(0.009)			(0.106)	(0.128)		

	Number of services per month				Number of oculo-visual assessments per month				Number of minor oculo-visual assessments per month			
Patient age	Before	After	Difference	% change	Before	After	Difference	% change	Before	After	Difference	% change
<=19	54.257	51.326	-2.931	-5.40%	49.06	45.446	-3.614**	-7.37%	5.197	5.880	0.683***	13.14%
	(5.731)	(6.571)	(1.857)		(5.469)	(6.202)			(0.447)	(0.646)		
20 to 64	138.442	23.419	-115.023***	-83.08%	108.269	17.258	-91.011***	-84.06%	30.172	6.162	-24.011***	-79.58%
	(25.029)	(3.699)	(4.782)		(23.085)	(2.068)			(2.368)	(1.914)		
65+	52.037	58.727	6.690***	12.86%	36.227	39.698	3.471**	9.58%	15.810	19.029	3.219***	20.36%
	(5.579)	(7.864)	(2.092)		(4.249)	(5.995)			(1.396)	(2.476)		

Notes: Data are from the Ontario Ministry of Health and Long-term Care. The data include only publicly funded optometry services before and after the delisting. The delisting of eye exams for those aged 20 to 64 was in November 2004. Standard deviations are in parentheses. *** indicates the before and after sample means are statistically different at 1%, ** at 5% and * at 10%.

Table 3.5: Publicly covered patients and services by ophthalmologists before and after the delisting in Ontario (March 2003 to February 2007)

	Number of patients per month				Proportion of patients with more than one visit per month				Proportion of patients with more than one visit per year			
Patient age	Before	After	Difference	% change	Before	After	Difference	% change	One Year Before	One Year After	Difference	% change
<=19	28.342	26.523	-1.819***	-6.42%	0.278	0.296	0.018***	6.47%	0.368	0.377	0.008	2.21%
	(1.610)	(2.159)	(0.582)		(0.012)	(0.016)			(0.225)	(0.218)		
20 to 64	151.507	146.749	-4.758	-3.14%	0.497	0.528	0.031***	6.24%	0.523	0.560	0.037	7.11%
	(11.369)	(12.669)	(3.616)		(0.007)	(0.009)			(0.198)	(0.185)		
65+	221.171	232.865	11.694	5.29%	0.564	0.582	0.018***	3.19%	0.613	0.635	0.022	3.57%
	(22.817)	(27.201)	(7.591)		(0.006)	(0.009)			(0.185)	(0.182)		

	Number of services per month				Number of oculo-visual assessments per month				Number of consultations and visits per month			
	Before	After	Difference	% change	Before	After	Difference	% change	Before	After	Difference	% change
<=19	36.244	34.111	-2.133***	-5.89%	2.504	2.208	-0.296***	-11.82%	26.596	25.105	-1.491**	-5.60%
	(2.167)	(2.683)	(0.740)		(0.301)	(0.301)			(1.662)	(2.109)		
20 to 64	296.814	308.882	12.068	4.07%	4.7010	.0025	-4.699***	-99.95%	147.028	149.903	2.875	1.96%
	(24.477)	(29.035)	(8.115)		(0.449)	(0.003)			(11.684)	(14.486)		
65+	479.083	542.675	63.592***	13.27%	2.689	2.1814	-0.508***	-18.88%	209.616	235.035	25.419***	12.13%
	(52.491)	(70.186)	(18.934)		(0.471)	(0.363)			(23.011)	(32.03)		

	Number of diagnostic and therapeutic procedures per month			
	Before	After	Difference	% change
<=19	7.144	6.798	-0.346**	-4.84%
	(0.640)	(0.483)		
20 to 64	145.086	158.976	13.89***	9.57%
	(12.801)	(14.650)		
65+	266.778	305.458	38.68***	14.50%
	(29.559)	(38.210)		

Notes: Data are from the Ontario Ministry of Health and Long-term Care. The data include only publicly funded ophthalmology services before and after the delisting. The delisting of eye exams for those aged 20 to 64 was in November 2004. Standard deviations are in parentheses. *** indicates the before and after sample means are different at 1%, ** at 5% and * at 10%.

Table 3.6: Estimates of policy effects on annual eye care visits for individuals aged 20 to 64

	Visits indicator		Conditional visits	
	1	2	3	4
Full delisting	-0.057***	-0.054***	0.009	0.010
	(0.012)	(0.011)	(0.026)	(0.028)
Male		-0.078***		-0.016
		(0.002)		(0.019)
Secondary school		0.015***		-0.016
		(0.005)		(0.033)
Some post-secondary		0.064***		0.002
		(0.003)		(0.016)
Post-secondary		0.083***		0.011
		(0.005)		(0.019)
Lower middle income		0.015***		0.002
		(0.004)		(0.055)
Middle income		0.034***		-0.013
		(0.006)		(0.027)
Upper middle income		0.068***		-0.040***
		(0.004)		(0.015)
Highest income		0.103***		-0.030
		(0.007)		(0.034)
N	431331	431331	152451	152451

Notes: Data from the National Population Health Survey (1994/95 to 1998/99) and Canadian Community Health Survey (2000/01 to 2009/10). Columns 1 & 2 are marginal effects at the mean from probit models; Columns 3 & 4 show marginal effects at the mean from zero-truncated poisson models. Columns 1 & 3 include province, year and the partial delisting indicators only and columns 2 & 4 further controls for 8 five-year age indicators, gender, marital status, education and income quintile indicators. Standard errors clustered at province level are in parentheses. *** indicates significance at 1% level, ** at 5% and * at 10%.

Table 3.7: Estimates of policy effects on annual eye care visits for individuals aged 20 to 64 by income groups

	Visit indicator	No. of visits for patients
	1	2
Full delisting*low income	-0.063***	-0.051
	(0.017)	(0.036)
Full delisting*middle income	-0.058***	-0.028
	(0.013)	(0.019)
Full delisting*high income	-0.048***	0.042
	(0.009)	(0.029)
income	0.003	-0.056***
	(0.003)	(0.013)
income squared	0.001*	0.004***
	(0.000)	(0.001)
Joint hypothesis: all income interactions are equal		
F-statistic	8.48	11.53
P-values	(0.014)	(0.003)
N	431331	152451

Notes: Low income (the first two quintiles), middle income (the third quintile) and high income (the fourth and fifth quintiles) indicators are used in the interaction terms with a quadratic in income included in the regressions (Income is imputed at the mean of each income category). Other variables and model specifications are the same as those in columns 2 and 4 of Table 3.6.

Table 3.8: Estimates of spillover effects on annual eye care visits for individuals aged 65 and over

	Visit indicator		No. of visits for patients		
	1	2	3	4	5
Full delisting	0.012	0.008	0.037	0.071**	0.126**
	(0.014)	(0.013)	(0.027)	(0.032)	(0.057)
Optometrist-population ratio		0.006**		-0.010	-0.062**
		(0.003)		(0.013)	(0.031)
Ophthalmologist-population ratio		-0.002		0.091	0.219
		(0.013)		(0.092)	(0.162)
Optometrist-population ratio*full delisting					0.076**
					(0.037)
Ophthalmologist-population ratio*full delisting					-0.105
					(0.155)
N	110817	110817	64359	64359	64359

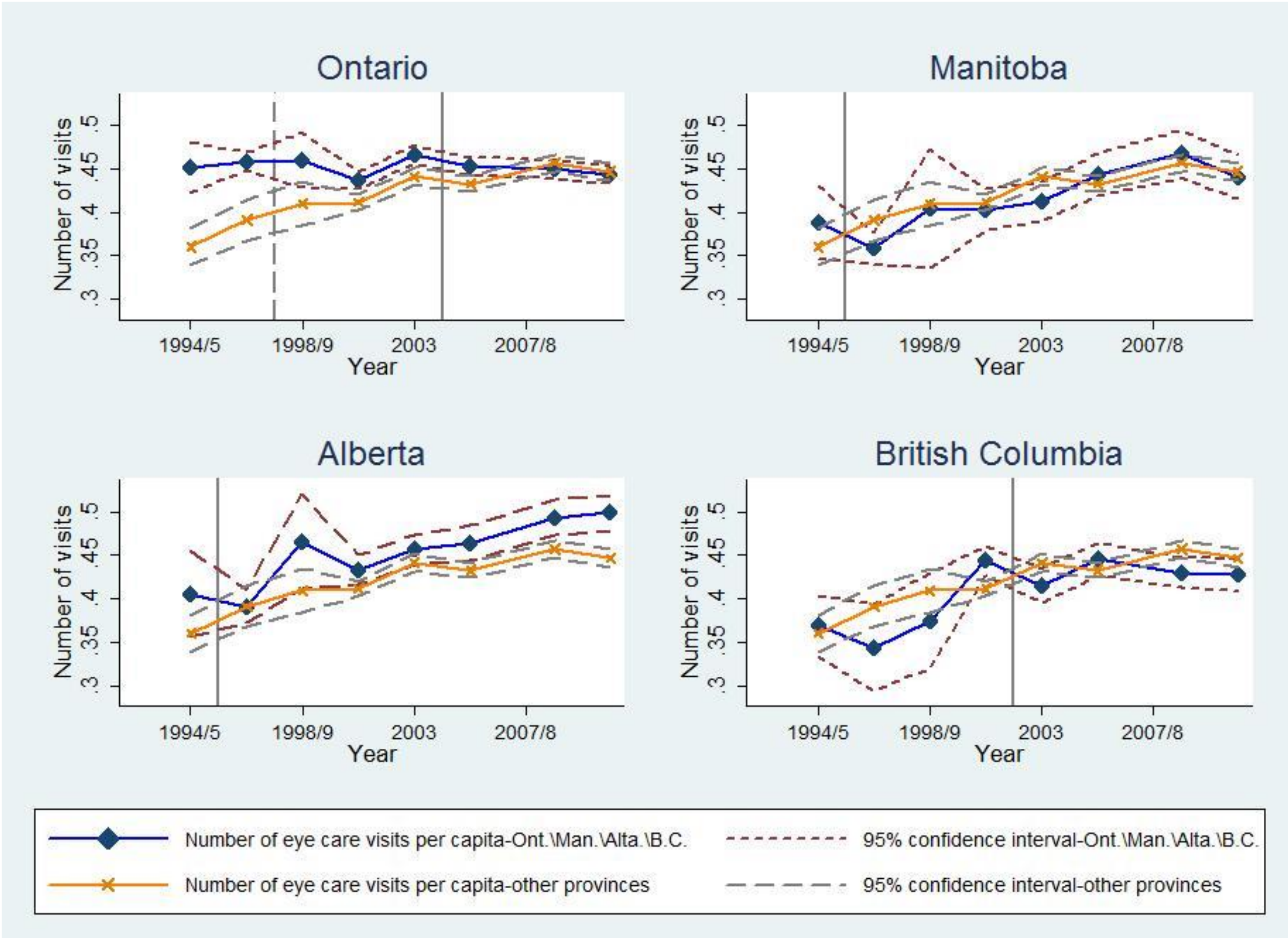
Notes: Data from Canadian Community Health Survey (2000/01 to 2009/10). Specifications in columns 1 & 3 are the same as those in Columns 2 & 4 of Table 3.6. Columns 2 & 4 add optometrist-population and ophthalmologist-population ratios; columns 5 further include interactions between the full delisting dummy and optometrist-population and ophthalmologist-population ratios. Columns 1 & 2 report marginal effects at the mean from probit models; columns 3 & 4 report marginal effects at the mean from zero truncated poisson models; column 5 reports coefficients from a zero truncated poisson model. Standard errors clustered at province level are in parentheses. *** indicates significance at 1% level, ** at 5% and * at 10%.

Table 3.9: Estimates of policy effects on weekly hours, weeks and earnings of optometrists

	Weekly work hours		Annual work weeks		Earnings	
	1	2	3	4	5	6
	Base	Controls	Base	Controls	Base	Controls
Partial delisting	-0.341	-0.195	1.581***	1.727***	-0.018	-0.003
	(0.600)	(0.720)	(0.318)	(0.336)	(0.042)	(0.045)
	[0.601]	[0.825]	[0.000]	[0.000]	[0.651]	[0.957]
Full delisting	-2.544*	-2.603**	1.671**	1.929***	0.033	0.048
	(0.839)	(0.922)	(0.573)	(0.536)	(0.099)	(0.093)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.827]	[0.710]
R-squared	0.061	0.127	0.006	0.098	0.06	0.104
N	2223	2223	2223	2223	2175	2175

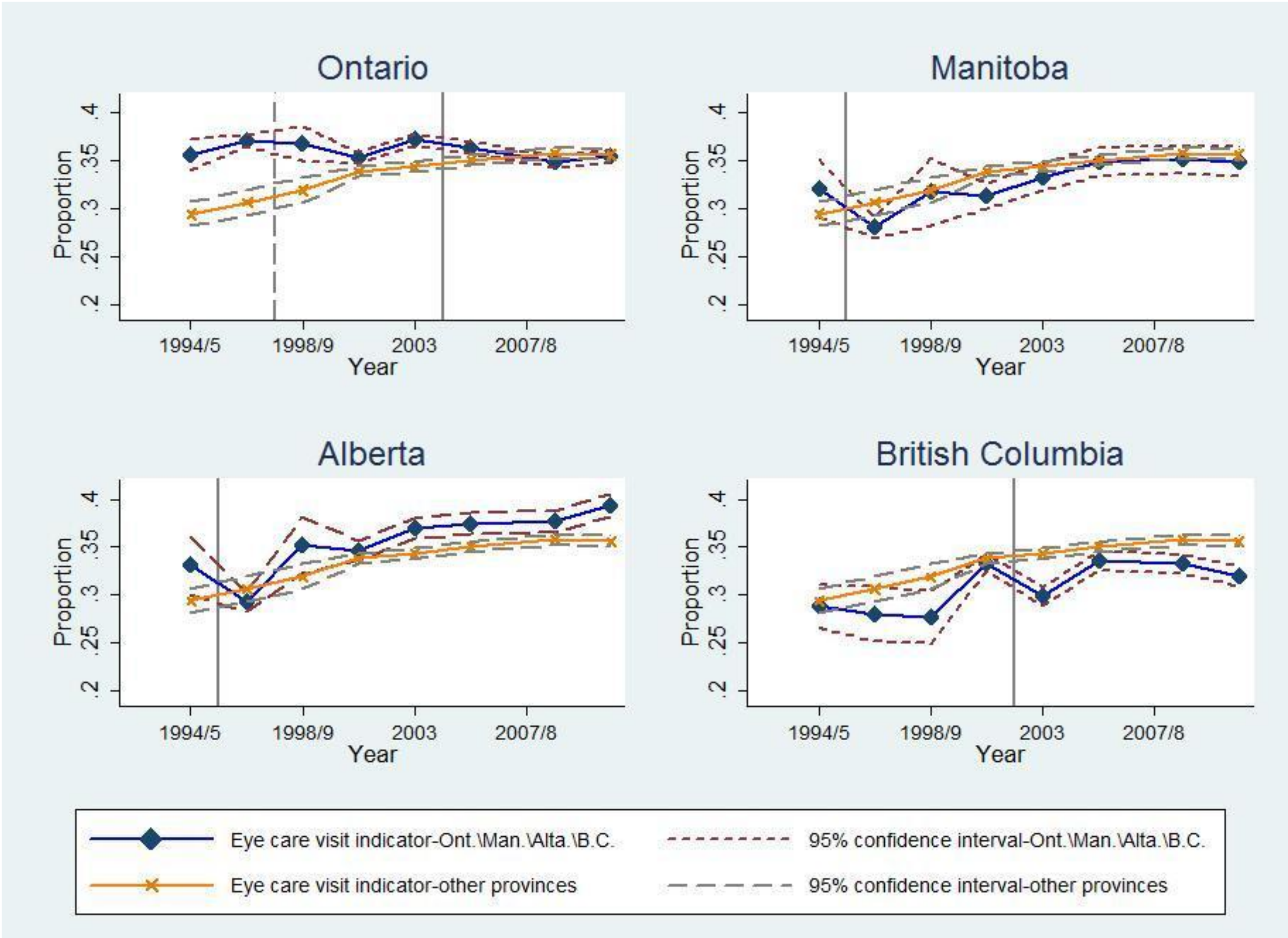
Notes: Census data (1991 to 2006). Standard errors clustered at province level are in parentheses. Bootstrap p-values based on 6,999 repetitions using the wild clustered bootstrapping methods are reported in brackets. The models in columns 1, 3 & 5 include year and province indicators and the models in columns 2, 4 & 6 further controls for age, age squared, and gender and marital status indicators. Earnings is defined as the sum of positive self-employment earnings and wages. *** indicates significance at 1% level, ** at 5% and * at 10%.

Figure 3.1: Number of eye care visits per capita for individuals aged 20 to 64



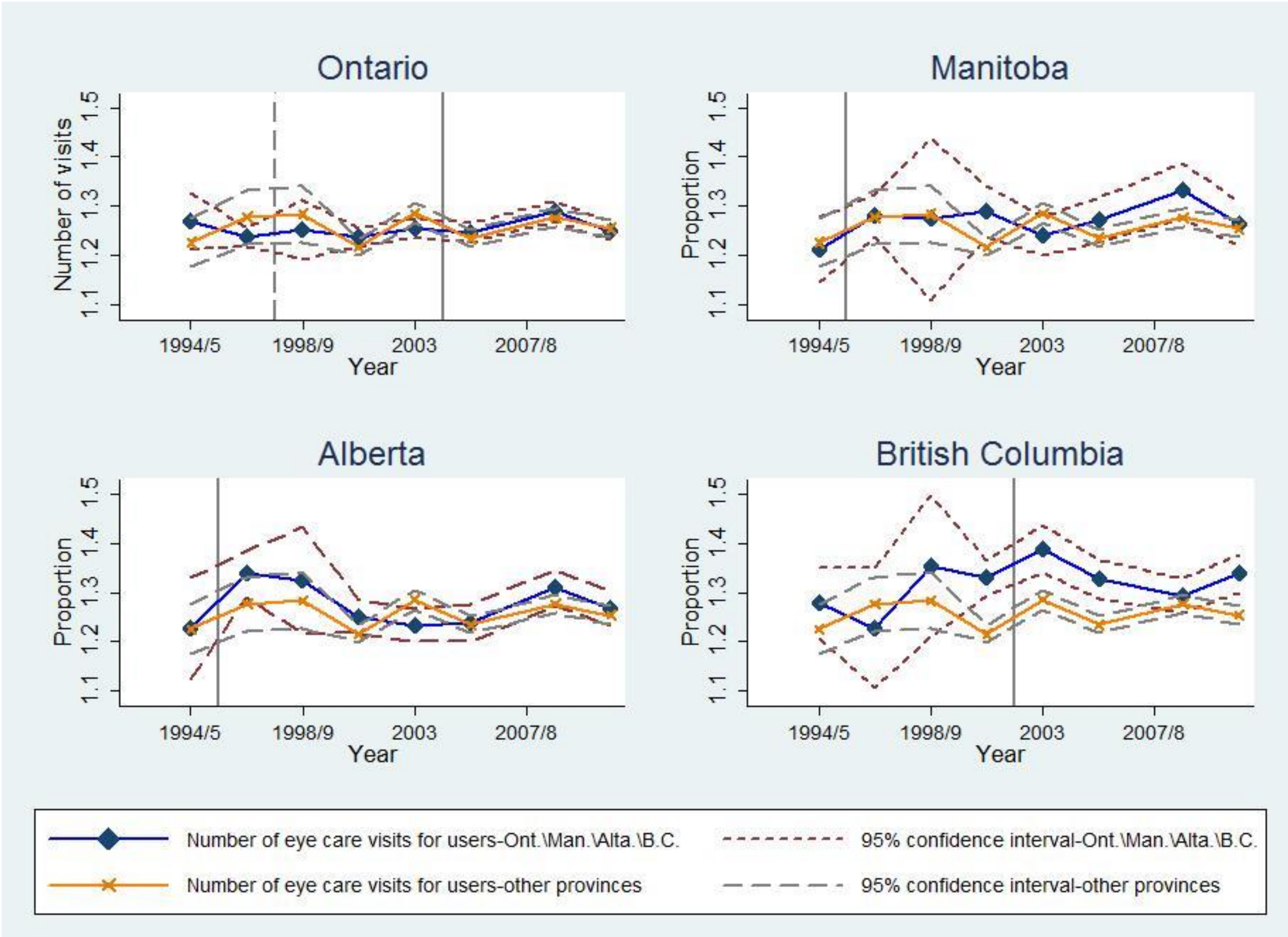
Notes: The timing of the full delisting policy changes is as follows: Ontario-November 2004; Manitoba-April 1996; Alberta-December 1994; British Columbia-November 2001. The only partial delisting policy change happened in Ontario in April 1998. For more details refer to Table 3.1 and Appendix A.

Figure 3.2: Eye care visit indicator for individuals aged 20 to 64



Notes: The timing of the full delisting policy changes is as follows: Ontario-November 2004; Manitoba-April 1996; Alberta-December 1994; British Columbia-November 2001. The only partial delisting policy change happened in Ontario in April 1998. For more details refer to Table 3.1 and Appendix A.

Figure 3.3: Number of eye care visits for users aged 20 to 64



Notes: The timing of the full delisting policy changes is as follows: Ontario-November 2004; Manitoba-April 1996; Alberta-December 1994; British Columbia-November 2001. The only partial delisting policy change happened in Ontario in April 1998. For more details refer to Table 3.1 and Appendix A.

Figure 3.4: Monthly publicly covered patients and services by optometrists in Ontario

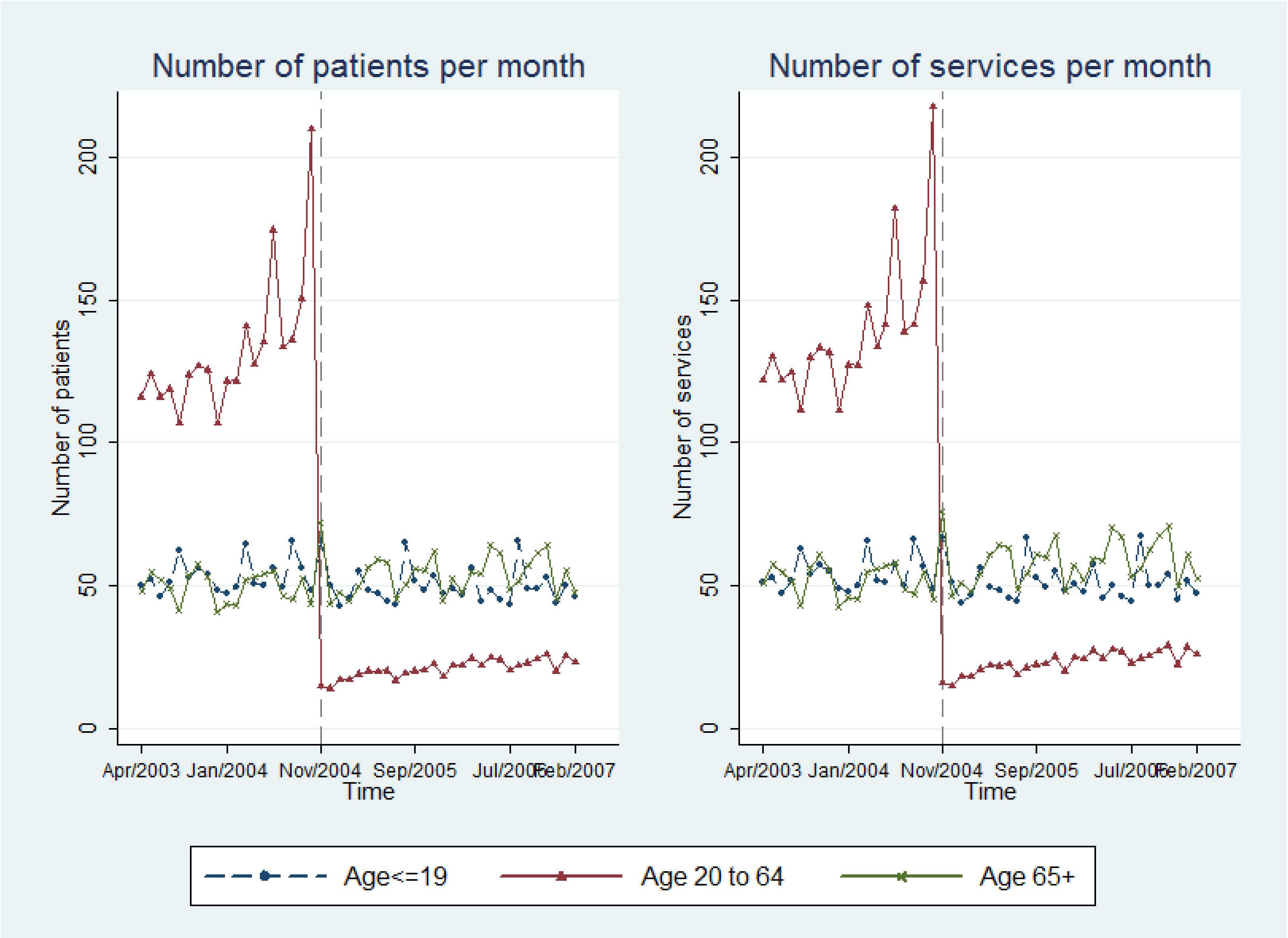


Figure 3.5: Proportion of patients with more than one visit per month by optometrists in Ontario

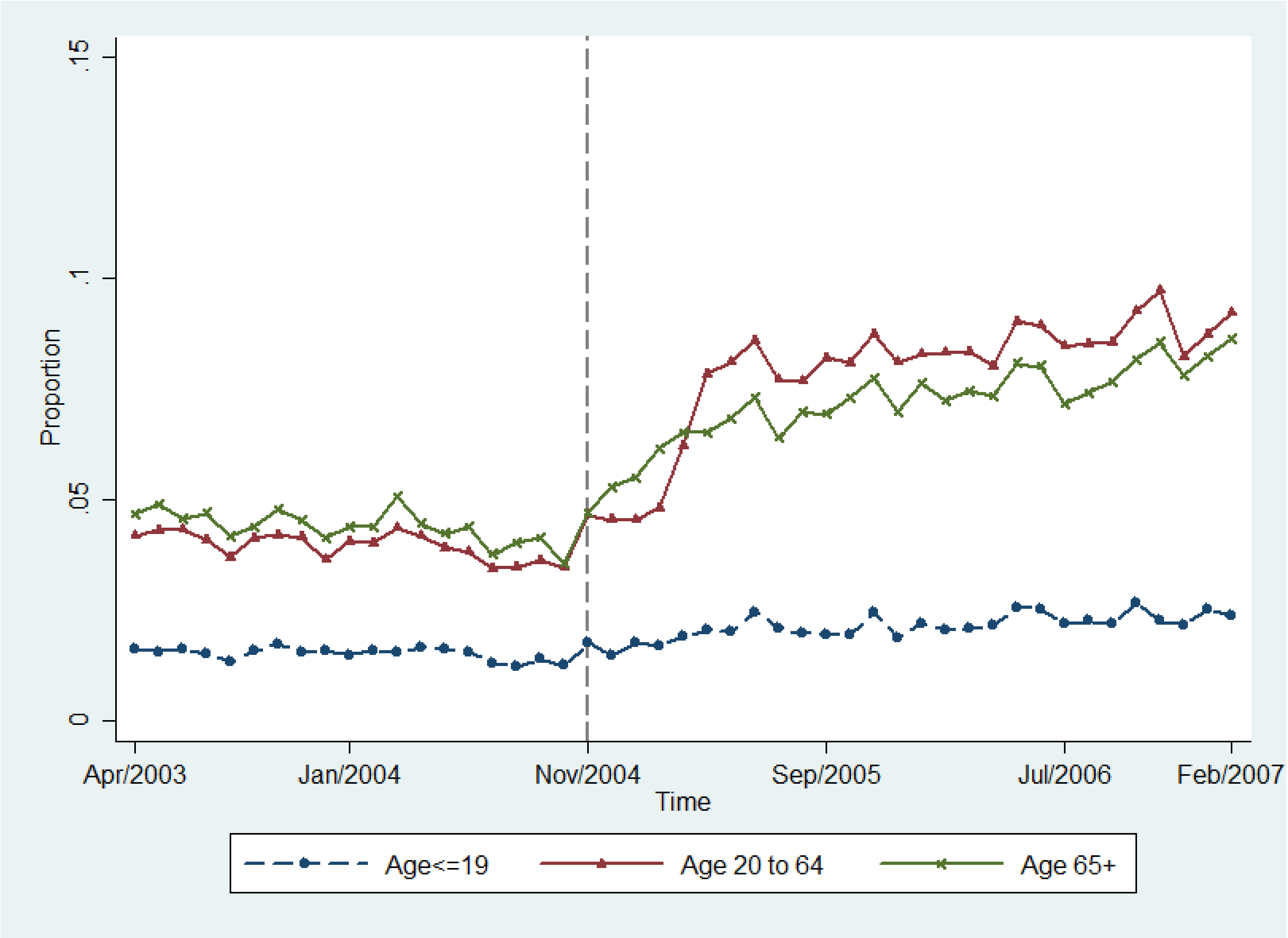


Figure 3.6: Monthly publicly covered patients and services by ophthalmologists in Ontario

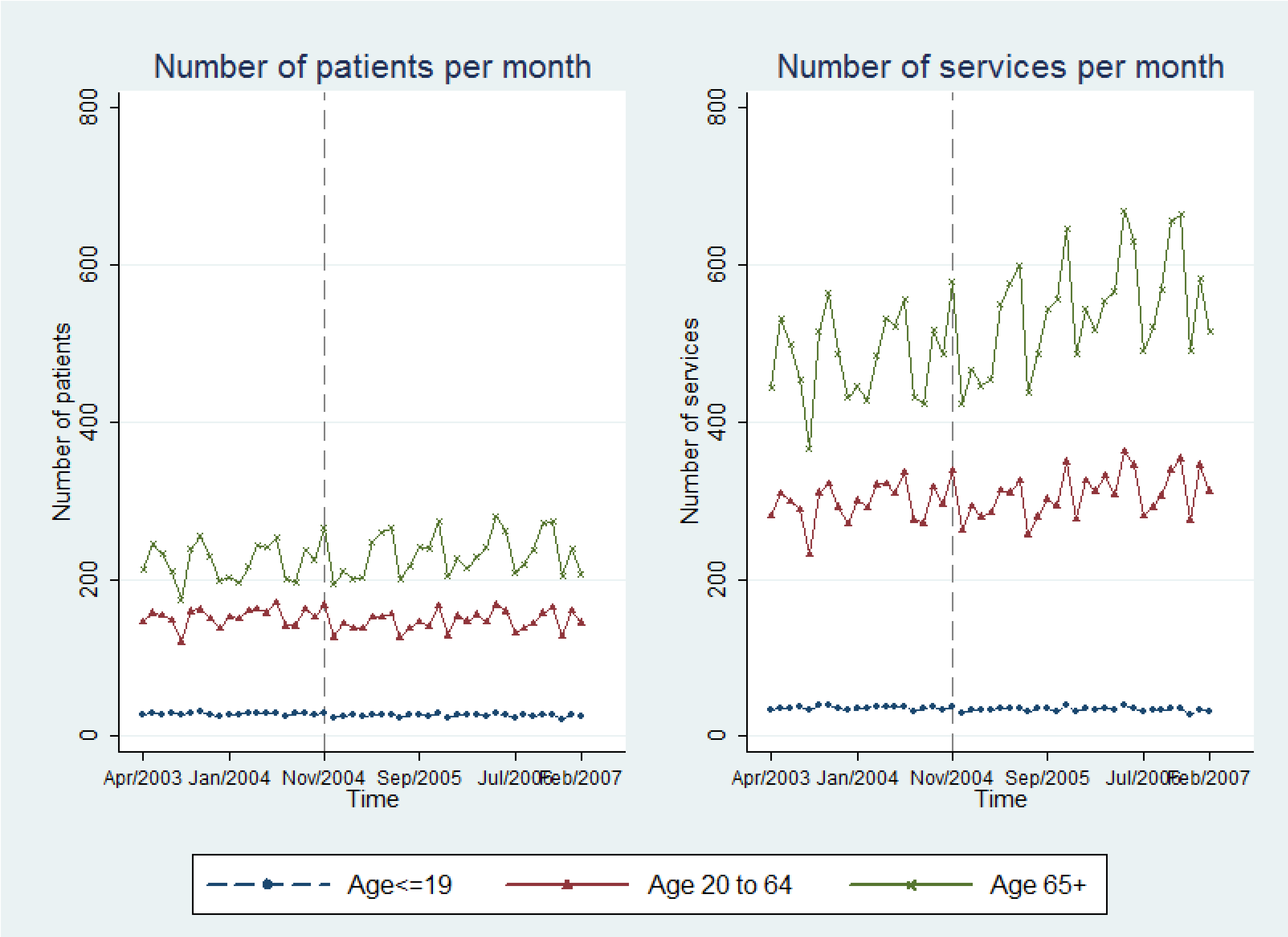
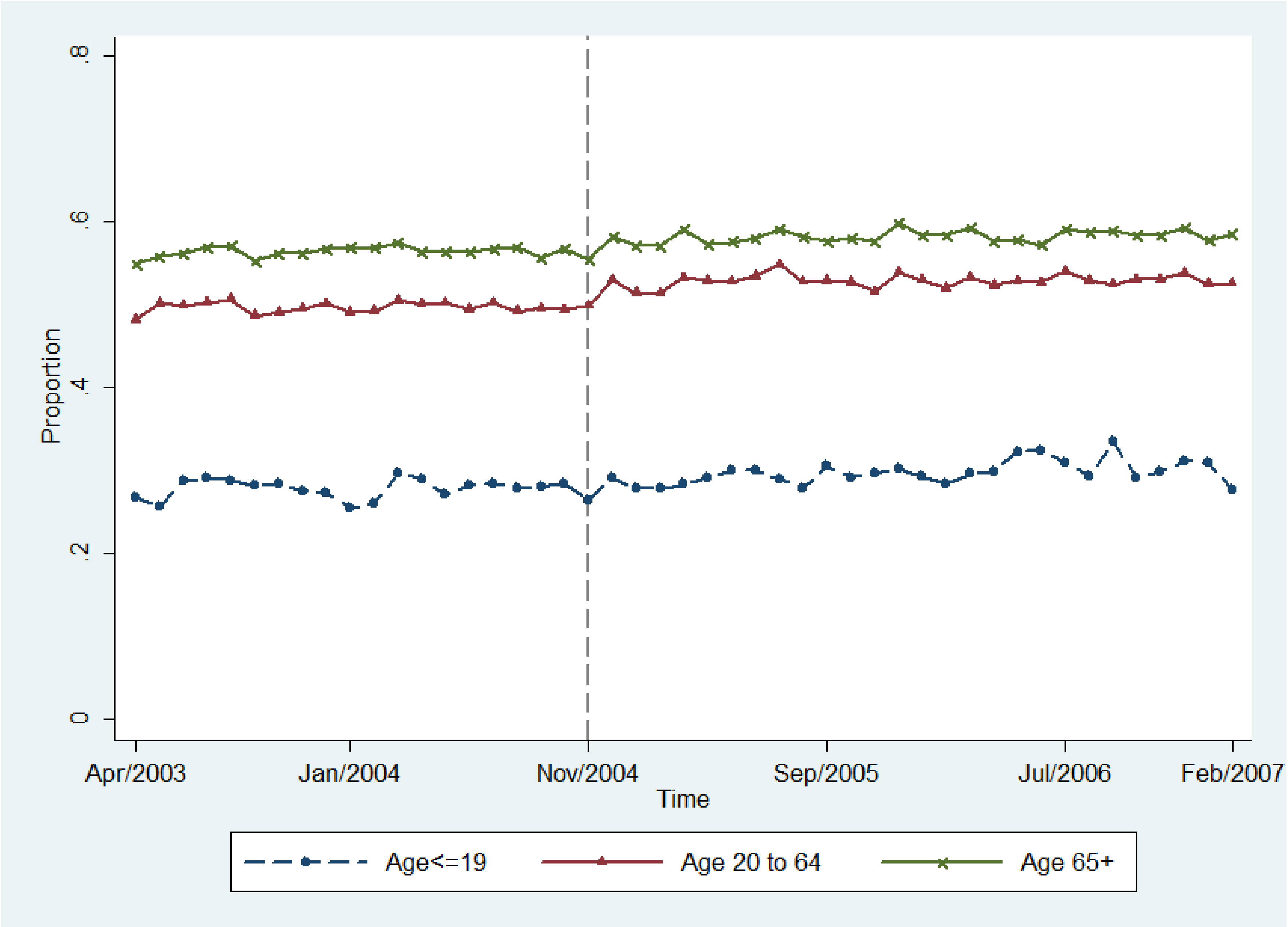


Figure 3.7: Proportion of patients with more than one visit per month by ophthalmologists in Ontario



Appendix A

Definition of *partial* and *full* indicator variables – Supply-Side Analysis (1990 to 2006)

The four cycles of Canada's Census data (1991, 1996, 2001 and 2006) were used for the supply-side analysis. The data contain information on annual earnings and weeks in the previous census year while weekly hours were measured in a reference week in the census year. The variables are defined as follows:

full =1 if province=British Columbia and year>2001;
full =1 if province=Alberta and year>1994;
full =1 if province=Saskatchewan and year>1992;
full =1 if province=Manitoba and year>1996;
full =1 if province=Ontario and year>2004;
full =1 if province=Quebec and year>1992;
full =1 if province=Nova Scotia and year>1992;
full =1 if province=Newfoundland and year>1991;

partial =1 if province=British Columbia and (year>1993&year<=2001);
partial =1 if province=Manitoba and (year>1993&year<=1996);
partial =1 if province=Ontario and year>=1998&year<=2004;
partial =1 if province=New Brunswick and year>1992;

The delisting policy change for children age 10 to 19 in Nova Scotia in 1997 happened after the full delisting. In the robustness checks, there are no meaningful changes in the estimates when including it as *partial* delisting indicator or dropping it.

Definition of *partial* and *full* indicator variables – Demand-Side Analysis (1994 to 2010)

The three cycles of NPHS data (1994/95, 1996/97, 1998/99) and five cycles of CCHS data (2000/01, 2003, 2005, 2007/08, 2009/10) were used for the demand-side analysis. The data collection period for each cycle is as follows:

NPHS 1994/95: June, August, November 1994 and March to June 1995.

NPHS 1996/97: May, July, September 1996 and January to April 1997.

NPHS 1998/99: May, July, September 1998 and January to April 1999.

CCHS 2000/0: September 2000 to early October 2001.

CCHS 2003: January to November 2003.

CCHS 2005: January to December 2005.

CCHS 2007/08: January 2007 to December 2008

CCHS 2009/10: 2009 to 2010.

The policy changes for individuals aged 20 to 64 are shown in the following table.

Appendix Table 3.10: De-listing eye examination for people aged 20 to 64 (1994 to 2010)

Province	Eye exam coverage in 1994	Partial delisting	Full delisting
British Columbia	One visit every two years		In Nov. 2001 no longer covered
Alberta	One visit every year		In Dec. 1994 no longer covered
Saskatchewan	Not covered		No change
Manitoba	Once every two years		In April 1996 no longer covered
Ontario	Once every year	In April 1998 eye exam was limited to once every 2 years	In November 2004 no longer covered except patients with diabetes, cataracts, glaucoma etc.
Quebec	Not covered	No change	
Nova Scotia	Not covered	No change	
New Brunswick	Not covered	No change	
Newfoundland	Not covered	No change	
PEI	Not covered	No change	

Notes: Sources from provincial association of optometrists and government websites. During the period 1994 to 2010, there were no policy changes for people aged 65 and over and only one policy change for children aged 10 to 19. See Table 3.1 for details.

Please note that the reference period for eye care visits is the 12 months prior to the survey date. There is no survey date information in the data. The variables in the demand side equation are defined as follows:

full =1 if province=British Columbia and year>2001;
full =1 if province=Alberta and year>1994;
full =1 if province=Manitoba and year>=1996;
full =1 if province=Ontario and year>=2004;

partial =1 if province=Ontario and year>=1998&year<2004;
partial =1 if province=British Columbia and year<=2001;
partial =1 if province=Manitoba and year<1996;

The estimate for the *partial* delisting indicator is identified using the Ontario policy change only. With one degree of freedom it is difficult to perform inference on the estimate so we focus on the *full* delisting only for the demand side analysis.

Appendix B

Appendix Table 3.11: Estimates of policy effects on annual eye care visits for individuals aged 20 to 64 (Province level data)

	Visits indicator		No. of visits for patients	
	1	2	3	4
Full delisting	-0.056**	-0.047***	0.006	-0.025
	(0.022)	(0.014)	(0.018)	(0.045)
	[0.044]	[0.043]	[0.729]	[0.621]
Male		0.403		0.161
		(0.536)		(1.197)
Secondary school		-0.225		1.230*
		(0.210)		(0.620)
Some post-secondary		-0.172		0.612
		(0.162)		(0.430)
Post-secondary		-0.167		0.784
		(0.213)		(0.600)
Lower middle income		-0.229*		-0.486
		(0.111)		(0.692)
Middle income		0.152		0.066
		(0.142)		(0.415)
Upper middle income		0.051		-0.854
		(0.140)		(0.597)
Highest income		0.096		-0.229
		(0.119)		(0.252)
N	80	80	80	80
R-squared	0.864	0.919	0.572	0.675

Notes: Data from the National Population Health Survey (1994/95 to 1998/99) and Canadian Community Health Survey (2000/01 to 2009/10). All estimates are OLS coefficients based on province-level aggregate data. Columns 1 & 3 include province and year indicators only and columns 2 & 4 further controls for the sample means of all the indicator variables at province level. Standard errors clustered at province level are in parentheses.Bootstrap P-values based on 6,999 repetitions using the wild clustered bootstrapping methods are reported in brackets. *** indicates significance at 1% level, ** at 5% and * at 10%.

Conclusion

This thesis consists of three chapters that empirically investigate important issues in physician labour supply and health insurance policy interventions.

Given that female physicians constitute a growing proportion of the physician workforce, understanding differences between male and female physicians' labour supply behavior is necessary for developing sound health human resource policies. The first chapter examines physician labour supply issue from a family economics perspective. Gendered associations between family status and physician labour supply are explored in the Canadian physician labour market.

Findings from the first chapter show that there is effectively no gender difference in physician labour supply after controlling for family status. Single male and female physicians have statistically indistinguishable hours and weeks of work. Marriage and the presence of children are associated with 10% and 15% to 18% reduction in females' hours of work. In contrast, marriage and the presence of children are associated with a modest and statistically significant 4% and 3% increase in males' hours of work. Combined with the context of a uniform payment schedule, the evidence suggests that gender differences in labour supply appear to be matters of choice regarding leisure and home production, and/or the influence of social norms, rather than gender discrimination in market payment.

When physician and spousal work hours are modeled jointly, the evidence shows that the spouses of both male and female physicians reduce work hours in the presence of children. However, female physicians still bear most of the time cost associated with children. Having a highly educated spouse has little impact on male physician work hours, unless the spouse is a physician. In contrast, having a highly educated spouse reduces female physicians' work hours. Both male and female physicians have lower hours of work when married to another physician.

The first chapter has two major contributions. First, it highlights the importance of incorporating family characteristics into understanding physician labour supply behaviour and provides insight to human resource decision making in the health sector. Second, it sheds light on the gender discrimination issue given the nature of the physician labour market. Equalizing payment in the labour market may not equalize labour supply.

In the second chapter I analyze the impact on utilization and health of extending drug insurance in the population through a universal insurance program. The cost and benefit of introducing a national pharmacare program has been a persistent policy concern in Canada. The second chapter informs the policy debates by evaluating the impacts of a universal drug program in the province of Quebec, Canada.

I show that the introduction of the mandatory drug program substantially increased drug coverage among the general population. The program also increased medication use and GP visits. No statistically significant effects were found for specialist visits and hospitalization. Further, the improvement in access to health care and health outcomes is concentrated among the previously uninsured and those with a chronic condition. Most importantly, less healthy people experienced a large improvement in their health status.

The findings have a number of implications regarding implementation of a universal pharmacare program in Canada. A public drug program covering those who do not have private drug plans can increase medication use and GP visits. It may also benefit the chronically ill and less healthy people by improving their health outcomes. The mixed public and private program provides a feasible solution to the design of a national pharmacare program. Given public concerns about the cost of a pure public drug program and the status quo of private insurance industries, the incremental change of adding a public drug plan on top of the private plans is plausible in the Canadian context.

The third chapter is concerned with evaluating the impacts on service use and provider labour supply of removing public insurance coverage for routine eye exams. An economic framework is developed to estimate the impact of delisting eye exams from the supply- and demand-sides.

Evidence from the demand side analysis shows that the full delisting decreased the likelihood of using eye care among the working age population by 5 to 6 percentage points (about 15%). The substantial negative impact on the extensive margin raise concerns about access to eye care after the delisting. Considering the preventive care nature of eye exams and the mixed public and private financing of eye care, the delisting may increase public eye care utilization and expenditure in the long run. There is suggestive evidence that the delisting policies for the working age population were associated with increased eye care utilization among the publicly covered elderly patients.

Findings from this chapter suggest that a system-wide approach of evaluating delisting policies is very important from a policy perspective.