ESSAYS ON THE ECONOMICS OF VOLUNTEERISM, CHARITY, AND HEALTHCARE

ESSAYS ON THE ECONOMICS OF VOLUNTEERISM, CHARITY, AND HEALTHCARE

By

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A Thesis

Submitted to the School of Graduate Studies

in Partial Fulfillment of the Requirements

for the Degree

Doctor of Philosophy

McMaster University

DOCTOR OF PHILOSOPHY (2012) McMaster University

(Department of Economics) Hamilton, Ontario

Title: Essays On the Economics of Volunteerism, Charity, and Healthcare

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Number of Pages: x, 138

ABSTRACT

This thesis studies the impacts of three government policy interventions in Canada on individuals' behaviour and attempts to bound structural coefficients implied by economics theories using the estimated treatment effects. While the last chapter is on the healthcare market, the first three chapters focus on individuals' charitable behaviour, especially volunteer behaviour. A compulsory volunteer policy in Ontario is investigated from theoretical and empirical perspectives in chapters one and two respectively. In a theoretical overlapping generation model with social capital accumulation, we find that such a policy likely increases total public good provision and the social capital level. However, whether it increases long-run volunteering by those no longer subject to the policy depends crucially on the size of a public good demand elasticity. Chapter two empirically examines the impact of a “compulsory volunteerism” policy for adolescents on subsequent behaviour in Ontario, which mandates 40-hours of community service for high school students as a requirement for graduation. We estimate that: 1) the compulsory volunteer policy increased volunteer participation during high school; 2) those affected by the policy likely volunteered less than they otherwise would have after high school completion; 3) young people in Ontario who were not directly affected by the policy volunteered less after its introduction.

The third chapter examines the impact of tax policy changes on individuals' volunteer behaviour and attempts to analyze the relationship between donations of time and money. We develop a model where individuals are heterogeneous in their labour market and volunteer productivities, and in their tastes, which shows that positive cross sectional correlation between

donations of money and time may occur because of individual-specific effects even though each individual would regard such donations as substitutes. Exploiting the exogenous variation in the tax price introduced by a series of tax policy changes in Canada, we find that individuals make more time donations as the tax price of charitable donations increases, which casts doubt on earlier findings in cross sectional data that monetary and time donations are complements and suggests that they may be substitutes as most theories would imply.

The last chapter exploits changes in Canadian public health insurers' reimbursement schedules regarding chiropractic services to identify the impacts of subsidies for providers and patients. Over the past two decades, fiscal pressures have seen these services partly or completely "delisted" from public health insurance programs. Despite a large sample of individuals, there are challenges for inference in this situation where the source of exogenous variation derives from a small number of jurisdiction-level policy changes. To address them, we employ aggregation, a wild cluster bootstrap that provides asymptotic refinement, and other approaches. The results show appreciable decreases in providers' incomes and in utilization with the latter concentrated among low and middle income patients. But, chiropractors also augment their labour supply, perhaps increasing administration, marketing/promotion, or time per patient visit.

ACKNOWLEDGEMENTS

First and foremost, I would like to thank my supervisor Professor Michael R. Veall. I am extremely lucky to have had you as my advisor at the lowest moment of my doctoral program. It is your constant guidance, encouragement and support that made it possible for me to complete the program. You are a great mentor.

I would also like to thank my committee member Professor Arthur Sweetman for opening a whole new door in my research program. I am truly fortunate to have worked with you. I am a better economist because of it. I also wish to thank my committee member Professor Lonnie Magee. You have been so kind and patient to me, and always have done more for me than I ever expected.

Thanks to my friends and classmates in the PhD program: Keqiang Hou, Taha Jamal, Cong Li, Jinhu Li, Qing Li, Evan Meredith, Jiong Tu, and Chao Wang. Thank you for discussing research ideas with me and coming to my seminars. I have learned so much from you. My sincere appreciation to my friends outside the PhD program: Ting Huang, Jingjing Zhang, and Huizi Zhao. You have been there for me through the good times and the bad.

Finally, I would like to express my deepest gratitude to my dear family for your love and unconditional support.

PREFACE

The fourth chapter of this thesis was prepared jointly with Professor Arthur Sweetman and it has been submitted for journal publication. I was primarily responsible for the empirical analysis and participated in all other stages of the study.

Contents

Introduction 1

Chapter 1 Preferences, social capital, and compulsory volunteering 8

1.1 Introduction ... 8

1.2 The model ... 9

1.3 Comparative static analysis ... 12

1.4 Conclusion and discussion ... 14

Appendix: ... 17

Chapter 2 Does compulsory volunteering affect subsequent behaviour? Evidence from a quasi experiment in Canada 22

2.1 Introduction ... 22

2.2 Literature review ... 24

2.3 The compulsory volunteer policy in Ontario ... 27

2.4 Theories and policy issues ... 28

2.5 Data ... 30

2.6 Identification strategy and empirical issues ... 30

2.6.1 The direct effect ... 31

2.6.2. The indirect effect ... 33

2. 6.3. The “double cohort” effect ... 35

2.6.4. Difference-in-differences with clustered sample ... 36

2.7 Results ... 38

2.7.1 The direct effect ... 38

2.7.2 The indirect effect ... 43

2.8 Robustness check: Sample attrition ... 44

2.9 Conclusion ... 44

Appendix: ... 56

Chapter 3 Are contributions of time and money substitutes or complements? 60

3.1 Introduction ... 60

3.2 Literature review ... 62

3.3 A theoretical model on volunteer behaviour ... 65

3.3.1 A comparative static analysis under the tax deduction scenario: ... 66

3.3.2 A comparative static analysis under the tax credit scenario: ... 69

3.3.3 Summary of comparative static results 70
3.4 Identification strategy 71
3.5 Tax policy changes in Canada 73
3.5.1 The 1988 tax reform 74
3.5.2 The switch from tax-on-tax to tax-on-income 75
3.6 Data 76
3.7 Tax price simulation 78
3.8 Results 80
3.8.1 The tax price and tax reforms 80
3.8.2 The tax price and volunteer behaviour 80
3.9 Robustness checks 85
3.10 Conclusion and discussions 85
Appendix: 99
Chapter 4 Chiropractic public subsidy reductions in Canadian healthcare 102
4.1 Introduction 102
4.2 Institutional context 106
4.3 Data and descriptive statistics 108
4.4 Empirical strategy 110
4.4.1 Chiropractor regressions 110
4.4.2 Patient regressions 112
4.4.3 Identification and inference 112
4.5 Results 115
4.5.1 Chiropractors 115
4.5.2 Patients 117
4.6 Discussion and conclusion 119
Appendix: 133
Conclusion 136

List of Tables

Table 2.1:Means and stand errors of selected variables of four groups (cohort A and B) 48
Table 2.2: Education Attainment for age-19s in Ontario (cohort A only) 49
Table 2.3: Direct effect for pooled sample (cohort A and B) 50
Table 2.4: Direct effect for five age groups (cohort A and B) 51
Table 2.5: Direct effect for age-19s (cohort A only) 52
Table 2.6: Indirect effect for pooled sample (cohort A and B) 53
Table 2.7: Robustness check: attrition (cohort A and B) 54
Table 2.A1: Logit model with sampling weight for direct effect for pooled model 56
Table 2.A2: Logit model with sampling weight for direct effect for five age groups 57
Table 2.A3: Logit model with sampling weight for direct effect for five age groups 58
Table 2.A4: Logit model with sampling weight for indirect effect 59
Table 3.1: Means of selected General Social Survey variables for selected survey years[1] 89
Table 3.2: Tax price variation decomposition 90
Table 3.3: Probit estimates of daily volunteer indicator 91
Table 3.4: Tobit estimates of daily volunteer minutes 92
Table 3.5: Probit estimates of annual volunteer indicator 93
Table 3.6: Cross-price elasticity of donations of time 94
Table 3.A1: Probit model of daily volunteer indicator 99
Table 3.A2: Tobit model of daily volunteer minutes 100
Table 3.A3: Probit model of annual volunteer minutes 101
Table 4.1: Delisting policies in Canada from 1990 to 2006 125
Table 4.2: Descriptive statistics of selected census variables by year 126
Table 4.3: Chiropractor utilization over time 127
Table 4.4: Characteristics of chiropractic patients and non-patients 128
Table 4.5: OLS estimates of chiropractor weeks/year and hours/week of work 129
Table 4.6: Estimates of earned income 130
Table 4.7: Estimates of changes in the chiropractor utilization 131
Table 4.8: Estimates of changes in the number of chiropractic visits using individual level 132
Table 4.A1: OLS estimates of annual working weeks 133
Table 4.A2: OLS estimates of weekly working hours 134
Table 4.A3: OLS estimates of earned income 135

List of Figures

Figure 2.1: Annual volunteer participation rate of the four groups 55
Figure 2.2: Difference between the after group and before group over time 55
Figure 3.1: The first-dollar tax price of monetary donations and the marginal rate of tax over the personal income: 1986 vs. 1989 95
Figure 3.2: Tax price and personal income: 1998 vs. 2003 96
Figure 3.3: Average Tax Price and Average daily volunteer participation: 1986 vs. 1992 97
Figure 3.4: Average Donation / Net Income: 1986 vs. 1992 98

INTRODUCTION

The growth of the modern welfare state has stimulated the demand for information about the interaction between government interventions and economic agents in a market environment. This has encouraged the development of new micro data on individuals and firms, and led to the birth and prosperity of a new branch in economics, microeconometrics. Microeconometrics aims at the estimation of individual level economic parameters, the testing of economics theories, and the evaluation of public policy. One of the most important findings from the recent microeconometrics literature is, as summarized by Heckman (2001), "the evidence on the pervasiveness of heterogeneity and diversity in economic life". As a consequence of this discovery, econometricians have become increasingly cautious about making full structural assumptions on the behaviour of economic agents and have employed the treatment effect approach under more and more circumstances.

The treatment effect approach compares the outcome of interest of individuals who have been subject to an intervention (the treatment group) to that of who have not (the control group), assuming that the outcome of the control group is the same as that of the treatment group in the absence of the intervention. Using a counterfactual framework where the outcome is defined by Marshallian demand functions, $Y_t = g_t(X, e_t)$ and $Y_c = g_c(X, e_c)$, the treatment effect is defined as $\Delta = Y_t - Y_c$, where the subscripts t and c stands for the treatment and control group respectively. In contrast, the structural approach seeks to identify g_t and g_c under the assumption that they are of known form, often derived assuming that agents maximize utility. If the functions are correctly specified, the structural approach can not only address the policy evaluation question on the current population but also yield results directly comparable across studies and easily extrapolated across environments. However, it relies crucially on the validity of the functional form assumptions.

The treatment effect approach answers the policy evaluation question under weaker assumptions as it does not need to fully identify g_t and g_c. However, the treatment effect estimate is in principle not comparable across studies under different environments and cannot be extrapolated to new populations because the dependence between X and (e_t, e_c) can be completely different in the new population. As Heckman (2001) quotes Knight (1921), "The existence of a problem in knowledge depends on the future being different from the past, while the possibility of a solution of the problem depends on the future being like the past (p. 313)." The accumulating empirical literature based on the treatment effect approach also shows that the response to the treatment varies across individuals and is likely correlated with observable variables and unobservable variables (see Heckman and Robb (1985, 1986)). This finding manifests the difficulty of linking the treatment effect estimates to structural economic parameters and of extrapolating them from one environment to another.

Heckman and Vytlacil (1999, 2000, 2001, 2002) attempt to address this problem by introducing the marginal treatment effect, which is the mean effect of a program for those at the margin of participation in it for given values of observables and conditioning on the unobservable in the program participation equation. Using the marginal treatment effect conditional on X and (e_t, e_c), the treatment effect can be constructed for different populations.

In this thesis, I focus on investigating how economic agents' behaviour changes in response to various treatments and on understanding the relationship between the treatment effect estimates and the structural parameters. I study three policy changes in Canada. While the treatment effect approach requires no assumptions on how the treatment takes effect, I attempt to combine it with implications from economic theories so as to obtain estimates with economic interpretations, and hence to use them to bound structural parameters. In chapters one and two, the effect of compulsory volunteer policy in Ontario on the volunteer behaviour of youth is examined from theoretical and empirical perspectives. While chapter one provides a theoretical framework, chapter two estimates the effect using the Youth in Transition Survey (YITS). Chapter three investigates the effect of the 1988 tax reform on volunteer behaviour and estimates its cross-price elasticity with money donations using the General Social Survey (GSS). In chapter four, I turn to the effect of public healthcare insurance policy on both utilization and service provider behaviour using the National Population Health Survey (NPHS) and the Canada Census data.

This thesis attempts to contribute to the literature in the following ways. First, in each study, I attempt to decompose the compound treatment effect into its components based on economic models and identify them separately. Second, I investigate and discuss the conditions for each case under which the treatment effect estimate has a structural parameter interpretation. Third, I attempt to bound structural parameters based on treatment effect estimates.

In the first essay, the goal is to provide a theoretical framework to examine effects of the compulsory volunteer policy on individuals' volunteer behaviour. In the model, individuals choose volunteer work to produce a public good. As one of the major interests is to investigate the policy effect on subsequent volunteering behaviour, I develop an overlapping generations model where each individual lives two periods and is mandated to volunteer in the first period. Social capital, which serves as a multiplier in the public good production function, is also included in the model since advocates of compulsory volunteer policies consider it an important pathway for the policy effect.

The model shows that the introduction of the compulsory volunteer policy for the young cohort increases their total volunteer hours, the social capital level, and hence public good provision. However, under the condition that the public good is a normal good, the policy effect on volunteering after the compulsory period depends upon a public good demand elasticity and the social capital accumulation elasticity. If the price elasticity is not large enough, the compulsory volunteer policy may decrease subsequent volunteering. While these conclusions are not directly testable since both the public good demand elasticity and the social capital accumulation elasticity are unlikely to be observed, the model reveals distinct effects that the policy could impose on volunteer behaviour, which suggest that an empirical examination should estimate these effects separately.

The second essay is an extension of the first, with the goal of empirically estimating the effects of the compulsory volunteer policy. The theoretical model suggests that the policy affects volunteer behaviour through various mechanisms and results in three different effects, i.e. the instant direct effect, the long-run direct effect, and the indirect effect, which are separately estimated using the treatment effect approach. As for the long-run direct effect, while there is a psychology literature finding compulsory volunteer policies increase the stated future intention to volunteer, the literature on its actual effect on volunteer behaviour is rare. It is also the case for the indirect effect. This chapter contributes to the literature by providing estimates of these effects. A nonparametric inference technique developed by Conley and Taber (2011) is employed to address the potential over-rejection problem in the estimation.

Using a difference-in-differences approach, I find that: 1) the compulsory volunteer policy increased volunteer participation during high school; 2) those affected by the policy likely volunteered less than they otherwise would have after high school completion; 3) young people in Ontario who were not directly affected by the policy volunteered less after its introduction.

The third essay evaluates the effect of taxation policies on volunteer behaviour, with the aim of uncovering the relationship between donations of time and money by exploiting a natural experiment brought about by the 1988 tax reform in Canada. While theoretical studies tend to suggest that donations of time and money are likely to be substitutes, empirical studies based on cross-sectional variation usually find negative cross-price effects, and hence indicate that they are complements. I set up a theoretical model with individuals differing in wages, preferences, and marginal productivities of volunteer hours to highlight the importance of controlling for the heterogeneity in the empirical investigation. The main contribution to the literature is to provide estimates of the effect of the price of monetary donations to time donations. This study demonstrates the empirical investigation of an economic model using the treatment effect approach when a structural approach is infeasible, and an attempt to justify a structural parameter interpretation for the treatment effect estimates.

This study shows that individuals increase their volunteer behaviour on both the extensive and intensive margins as the tax price of charitable donations increases. The cross-price elasticity of daily volunteer participation is estimated to be 1.385 while that of annual volunteer participation is estimated to be 0.441. Hence our empirical results suggest that donations of time and money are substitutes rather than complements. Hence while tax deductions or tax credits may increase monetary donations, our estimates suggest that individuals will also decrease their volunteer time in response.

The final essay, which is based on joint research with Professor Arthur Sweetman, studies the effect of a public health insurance policy, which is like to have substantial impacts on the behaviour of both patients and service providers. In Canada, provincial governments are the dominant health insurance providers so that different provinces have historically taken alternative approaches to the inclusion of chiropractic services in the public healthcare payment system. Cuts over the past two decades have seen them partly or completely delisted in some provinces. This essay aims at examining the effect of the delisting. As the first study that comprehensively investigates both patients and service providers, the main contribution is to document the effects of delisting on service utilization and on the labour market outcomes of service providers. Moreover, it allows us to bound structural parameters, e.g. the price elasticity of chiropractic services. A secondary contribution is that, as per Cameron, Gelbach and Miller (2008), we bootstrap the t-statistics to obtain P-values corrected for the over-rejection problem in the presence of clustering.

Combined with the result on provider labour supply and patient utilization, we observe an increase of approximately 2.5% in chiropractor working time, but about 23% fewer visits at the new equilibrium. In addition, the roughly 23% decrease in utilization translated into a 15% income decrease. Taking the changes on both the chiropractor and the patient sides into account, we estimate the price elasticity of chiropractic services to be between 0.35 and 0.96. The results imply that a typical chiropractor spends about 33% more time per visit – although this time may involve promotion/marketing and administrative work in addition to direct patient care. While it is impossible to directly test these hypotheses based on current data, the evidence seems to support that the service provided by chiropractors changed following delisting.

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CHAPTER 1
PREFERENCES, SOCIAL CAPITAL, AND COMPULSORY VOLUNTEERING

1.1 INTRODUCTION

Volunteer behaviour is interesting to both researchers and policymakers as one of the major channels of voluntary public good provision and as a potential way to create social capital. As per Putnam (1993, 1995), social capital is the combination of networks, norms, and social trust that facilitate coordination and cooperation for mutual benefit. Recently, there have been proposals e.g. by U.S. President Obama and then U.K. Prime Minister Gordon Brown that students be mandated to provide a certain number of "volunteer" hours, either as a requirement for graduation or to qualify for financial support.

Empirical evidence provides mixed results on the post-school effect of such "compulsory volunteering" school policies. While the majority of studies, e.g. Metz and Youniss (2005), Henderson et al. (2007), find evidence that such policies have positive effects on the *attitude* toward volunteering, Stukas et al. (1999) find the opposite effect on *intentions* regarding future volunteering by a set of students who would have been unlikely to volunteer without the compulsory policy. Marks and Jones (2004) and Yang (2012) find evidence of such policies reducing *actual* post-school volunteering.

This study provides a theoretical framework to examine effects of such a policy on long-run individual volunteering. If volunteering were modeled as a private good, the result would hinge on the intertemporal substitutability or complementarity of volunteering in the school and post-school periods. However proponents of such a policy would regard it as essential that the goods produced by volunteering are public goods and that volunteering builds social capital. In addition to assuming that individuals volunteer to produce a public good as per Duncan (1999), we set up an overlapping generations model in order to incorporate these features. For clarity, there is no intertemporal substitution in the utility function, which is defined as a function of private and public good consumption each period. Social capital serves as a multiplier in the public good production function and exists either at the individual level, e.g. skills, knowledge, or nonprofit entrepreneurial ability (Bilodeau and Slivinski, 1996) or at the community level, e.g. social networks, norms and charitable institutes. (Apinunmahakul and Devlin, 2008, find evidence that social networks promote donations of time and money.). Unlike traditional forms of capital, it is not depleted by use, but rather by non-use. Hence we assume that social capital depreciates and is renewed as a byproduct of total volunteer hours in each period.

The model shows that the introduction of the compulsory volunteer policy for the young cohort increases their total volunteer hours, the social capital level, and hence public good provision. However, its effect on volunteering after the compulsory period is determined by two effects in the opposite direction: 1) individuals want to volunteer more because their volunteer hours become more productive; 2) they want to volunteer less because the shadow value of the public good decreases. The overall effect depends crucially upon a public good demand elasticity.

1.2 THE MODEL

Each individual lives for two periods, i.e. the young and old periods (subscript $t = 1, 2$), so that there are two generations in each period (subscript $\tau > 0$). There are n individuals in each generation. Utilities are determined by the consumptions of a private good, c, and a public good, P, in the two periods. The utility is assumed separable both between the private good and the public good and across periods so that the life time utility of individual i is given by:

$$U_i = [u(c_{i1}) + \alpha_i g(P_1)] + \beta[u(c_{i2}) + \alpha_i g(P_2)] \tag{1.1}$$

where a greater α_i $(0 < \alpha_i < 1)$ indicates a greater preference for the public good. Each individual has one unit of time endowment per period, which can be divided between paid work and volunteer work.[1] Because there is no saving in the model, all income from the paid work is used to purchase the private good. The budget constraints for an individual are:

$$v_{it} + x_{it} = 1 \quad \forall\, t = 1, 2$$

$$c_{it} = w_t * x_{it} \quad \forall\, t = 1, 2$$

where v_{it} is volunteer time, x_{it} is time on the paid work, and w_t is the wage rate. Individuals volunteer to produce the public good, which is determined by the total volunteer time contributions by all individuals, $V_\tau,$ and the social capital level, S_τ.

$$P_\tau = S_\tau * f\left(\sum_i v_{i\tau}\right) = S_\tau * f(V_\tau)$$

Social capital is a function of total volunteer contributions and depreciates at a constant rate, δ:

$$S_{\tau+1} = (1 - \delta) * S_\tau + h(V_\tau), \quad (0 < \delta < 1)$$

Finally, the compulsory volunteer policy is introduced in the young period as setting:

$$v_{i1} \geq \underline{v}$$

where $\underline{v}$ is the minimum compulsory volunteer time. We also assume diminishing marginal utility, i.e. $u_c > 0$, $u_{cc} < 0$, and $g' > 0$, $g'' < 0$, and diminishing marginal returns, i.e. $f' > 0$, $f'' < 0$ and $h' > 0$, $h'' < 0$.

[1] For simplicity, we do not model schooling itself, although we think of the young period as school and the old period as post-school. The key feature of the model is that volunteering has an opportunity cost.

Each individual chooses her volunteer time in both periods taking others' volunteer hours as given. Taking derivatives with respect to v_{i1} and v_{i2}, we have the first order conditions defining the optimal volunteer time for individual i:

$$u_{ci1}w_1 = \alpha_i(g_1' S_1 f_1' + \beta g_2' f_2 h_1') \tag{1.2}$$

$$u_{ci2}w_2 = \alpha_i g_2' S_2 f_2' \tag{1.3}$$

The LHS of both equations are the marginal cost of an hour of volunteer work, which is the wage rate multiplied by the marginal utility of the private good. The RHS gives the marginal benefit. For both the young and the old periods this includes the marginal utility of the public good produced by the additional volunteer hour. In the young period equation (1.2), there is a second term because an additional volunteer hour when young increases social capital and hence public good production in the second period. The second term is the discounted marginal utility of this additional public good.

Since individuals are distinct only in their preference for public goods, those with stronger preferences for public goods would volunteer more hours. The monotonic relationship between the optimal volunteer hours v_{it}^* and α_i implies that there exist threshold values α_1^*, α_2^* such that $v_{i1}^* = \underline{v}\,, v_{i2}^* = 0$. For individuals whose $\alpha_i < \alpha_1^*$, they volunteer $v_{i1} = \underline{v}$ and $v_{i2} = 0$ because of the compulsory volunteer policy and the nonnegative constraint. For those who have $\alpha_i > \alpha_t^*$, they will volunteer v_{it}^*. Assuming there are m_1 individuals who have $\alpha_i < \alpha_1^*$ in the each generation, the total volunteer hours in time τ are:

$$V_\tau^* = V_{1\tau}^* + V_{2\tau}^* = \left(m_1\underline{v} + \sum_{\alpha_i > \alpha_1^*} v_{i1}^*\right) + \sum_{\alpha_i > \alpha_2^*} v_{i2}^* \tag{1.4}$$

Eq. (1.2) to (1.4) define the equilibrium in each time given a certain level of social capital. In the long run, the model reaches the steady state, where the social capital depreciation equals the generation of new social capital:

$$\delta S^* = h(V^*) = h(V_\tau^*) \quad \forall\, \tau > 0 \tag{1.5}$$

1.3 COMPARATIVE STATIC ANALYSIS

Eq. (1.2) to (1.5) define the long-run equilibrium. We focus on the introduction of a policy that mandates volunteer work in the young period and its effect on individuals' voluntary time contributions and total public good provision in the steady state. The following two equations describe how the voluntary time contributions changes for those who have $\alpha_i > \alpha_t^* \quad \forall\, t = 1, 2$:

$$dv_{i1} = \frac{\alpha_i \omega_{i1}[a + \frac{1}{\delta}b + \beta(e + b + \frac{1}{\delta}(1-\delta)kh')]w_2^2}{(w_1^2 + B_1)(w_2^2 + B_2) - B_1 B_2} m_1 d\underline{v} \tag{1.6}$$

$$dv_{i2} = \frac{\alpha_i \omega_{i2}\beta(a + \frac{1}{\delta}b)w_1^2}{(w_1^2 + B_1)(w_2^2 + B_2) - B_1 B_2} m_1 d\underline{v} \tag{1.7}$$

where $a_t = S_t^2 g_t'' f_t'^2 + S_t g_t' f_t'' \;\; \forall\, t = 1, 2$ ($a_1 = a_2 = a$ at the steady state), $e = g_2'' f_2^2 h_1'^2 + g_2' f_2 h_1''$, $b = S_2 g_2'' f_2' f_2 h_1' + g_2' f_2' h_1'$, $k = g_2'' f_2^2 h_1'$, $B_1 = -\sum_{\alpha_i > \alpha_1^*} \alpha_i \omega_{i1}[a + \frac{1}{\delta}b + \beta(e + b + \frac{1}{\delta}(1-\delta)kh')]$, $B_2 = -\sum_{\alpha_i > \alpha_2^*} \alpha_i \omega_{i2}[a + \frac{1}{\delta}b]$, and $\omega_{it} = |1/u_{ccit}| \;\forall\, t = 1, 2$. For simplicity, we assume that the policy does not change the number of volunteers. Mathematically, when α_t^* changes to $\alpha_t^{*\prime}$ in response to a change in $\underline{v}$, we assume $\alpha_1 < \cdots < \alpha_{m_1} < \alpha_t^*$, $\alpha_t^{*\prime} < \alpha_{m_1+1} \ldots < \alpha_n$.[2]

At the steady state, there are the young and old generations in each period. The introduction of a compulsory volunteer policy would change the total volunteer hours of both generations.

$$dV_1 = \frac{w_1^2(w_2^2 + B_2)}{(w_1^2 + B_1)(w_2^2 + B_2) - B_1 B_2} m_1 d\underline{v} \tag{1.8}$$

$$dV_2 = \frac{-w_1^2 B_2}{(w_1^2 + B_1)(w_2^2 + B_2) - B_1 B_2} m_1 d\underline{v} \tag{1.9}$$

[2] An alternative approach is to assume α_i is distributed continuously on the support between zero and one, which does not change the result.

$$dV = dV_1 + dV_2 = \frac{w_1^2 w_2^2}{(w_1^2+B_1)(w_2^2+B_2)-B_1B_2} m_1 d\underline{v} \tag{1.10}$$

Eq. (1.6) and (1.7) provides the effect of a compulsory volunteer policy at the young period ($d\underline{v}$) on the voluntary time contribution of individual i on the young and old periods (dv_{i1}, dv_{i2}). Eq. (1.8) to (1.10) are the effect on the total volunteer time (dV), and the volunteer time provided by the young generation (dV_1) and the old generation (dV_2). On the individual level, the policy forces less private good consumption in the young period, which will affect an individual's optimal choices in both periods. On the aggregate level, it affects public good provision and the social capital stock.

Proposition 1. If $\varepsilon \stackrel{\text{def}}{=} \left|\frac{dP/P}{dg'(P)/g'(P)}\right| < 1$, then the introduction of a compulsory volunteer policy on the young generation will increase the total volunteer time (V) and the volunteer time provided by the young generation (V_1), but decrease that by the old generation (V_2). It will also decrease the voluntary time contribution in both periods (dv_{i1}, dv_{i2}).

Proof. See Appendix.

The numerator of ε is the percentage change in the amount of the public good, and the denominator is the percentage change in the marginal utility, or the shadow price, of the public good. Hence this could be considered as the price elasticity of the public good. Proposition 1 indicates that if the public good is price inelastic, the policy decreases the voluntary time contribution even though the total volunteer time and the social capital level are increased.

Proposition 2. In the case[3] when $(w_1^2 + B_1)(w_2^2 + B_2) - B_1B_2 > 0$, $\frac{dV_2}{d\underline{v}} > 0$ and $\frac{dv_{i2}}{d\underline{v}} > 0$ if and only if $\varepsilon > 1 + \frac{1}{\eta\phi - 1}$, where $\eta \stackrel{\text{def}}{=} \frac{dh/h}{dV/V}$>0, $\phi \stackrel{\text{def}}{=} \frac{-g'f'V}{Sg''f'^2+g'f''}$>0, $\eta\phi - 1 > 0$.

[3] Eq. (3.10) shows that the sign of dV is determined by the denominator, $(w_1^2 + B_1)(w_2^2 + B_2) - B_1B_2$. As $\underline{v}$ increases, it increases virtual income, i.e. private income plus the resources donated by all other individuals, for

Proof. See Appendix.

Proposition 2 indicates that the compulsory volunteer policy has a positive effect on the amount of volunteering when old only when the price elasticity of the public good exceeds a threshold, which is determined by the social capital accumulation elasticity, η. In the case where η goes to infinity, the threshold converges to one.

1.4 CONCLUSION AND DISCUSSION

The model shows how individuals' preferences and social capital accumulation interact and determine the policy effects on volunteer behaviour in equilibrium. The price elasticity of the public good reflects the change in the marginal utility of the public good as the amount of it produced is changed. In the inelastic case, or $\varepsilon < 1$, a one percent increase in the amount of the public good will lead to decrease in its marginal utility by more than one percent. In the elastic case, the absolute percentage change in the marginal utility will be less than that in the amount of the public good. The other key factor, the social capital accumulation elasticity (η), is determined by the social capital production function, which indicates the increment in social capital as total volunteer volume increases.

The model suggests that a compulsory volunteer policy can stimulate the total public good provision and hence social capital accumulation as long as the public good is a normal good. However, it may not necessarily increase post-school volunteering. Proposition 1 indicates that the policy actually decreases post-school volunteering if the public good is inelastic. Proposition 2 provides the sufficient and necessary condition for a positive post-school policy effect. As shown, the price elasticity of the public good has to be larger than a threshold

those who have $\alpha_i > \alpha_t^* \quad \forall\, t = 1, 2$. They would like to consume more public good ($\frac{dP}{d\underline{v}} > 0$ and $\frac{dV}{d\underline{v}} > 0$) as long as it is a normal good. Therefore, a sufficient condition for $(w_1^2 + B_1)(w_2^2 + B_2) - B_1 B_2 > 0$ is that the public good is a normal good.

magnitude, which is a decreasing function of the social capital accumulation elasticity. As the social capital accumulation elasticity goes to infinity, the threshold converges to one. The next chapter studies empirically a compulsory volunteer policy in Ontario, Canada and does not find any positive effect on post-school volunteer behaviour.

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APPENDIX:

The utility function of individual i:

$$U_i = [u(c_{i1}) + \alpha_i g(P_1)] + \beta[u(c_{i2}) + \alpha_i g(P_2)]$$

The budget constraints for an individual are:

$$v_{it} + x_{it} = 1 \qquad \forall\, t = 1, 2$$
$$c_{it} = w_t * x_{it} \qquad \forall\, t = 1, 2$$

The public good in period τ:

$$P_\tau = S_\tau * f\left(\sum_i v_{i\tau}\right) = S_\tau * f(V_\tau)$$

Social capital accumulation:

$$S_{\tau+1} = (1-\delta) * S_\tau + h(V_\tau) \qquad (0 < \delta < 1)$$

A compulsory volunteer policy:

$$v_{i1} \geq \underline{v}$$

FOC:

$$v_{i1}^*: \quad u_{ci1} w_1 = \alpha_i (S_1 g_1' f_1' + \beta g_2' f_2 h_1') \qquad (1.a1)$$
$$v_{i2}^*: \quad u_{ci2} w_2 = \alpha_i S_2 g_2' f_2' \qquad (1.a2)$$

Equilibrium:

$$v_{i1} = v_{i1}^* \qquad \forall\, \alpha_1^* \leq \alpha_i \leq 1;$$
$$v_{i1} = \underline{v} \qquad \forall\, 0 \leq \alpha_i < \alpha_1^*$$
$$v_{i2} = v_{i2}^* \qquad \forall\, \alpha_2^* \leq \alpha_i \leq 1;$$
$$v_{i2} = 0 \qquad \forall\, 0 \leq \alpha_i < \alpha_2^*$$
$$V = V_1 + V_2 = \Sigma_i v_{i1} + \Sigma_i v_{i2}$$
$$\delta S = h(V) \qquad (1.a3)$$
$$P = Sf(V) \qquad (1.a4)$$

where:

$$\alpha_1^* = \frac{w_1 u_c(w_1(1-\underline{v}))}{S_1 g_1' f_1' + \beta g_2' f_2 h_1'}$$
$$\alpha_2^* = \frac{{w_2}^2 u_c}{S_2 g_2' f_2'}$$

Assume there are m_t individuals who belong to $0 \leq \alpha_i < \alpha_t^*$, $\forall\, t = 1, 2$:

$$V = V_1 + V_2 = \left(m_1 \underline{v}_1 + \sum_{\alpha_i > \alpha_1^*} v_{i1}^*\right) + \sum_{\alpha_i > \alpha_2^*} v_{i2}^* \qquad (1.a5)$$

Comparative static analysis:

At any period, there are two generations. We refer to the total volunteer hours of the objective generation with V_1, V_2, and the total volunteer hours of the other generation with V_o in period one and V_y in period two. Thus, from equation (1) to (4), we have:

$$u_{ci1}w_1 = \alpha_i * S_1 * g'\left(S_1 f(V_o+V_1)\right) * f'(V_o+V_1) + \alpha_i * \beta * g'\left(S_2 f\left(V_y+V_2\right)\right) * h'(V_o+V_1) * f(V_y+V_2) \quad (1.a6)$$

$$u_{ci2}w_2 = \alpha_i * \beta * S_2 * g'\left(S_2 f\left(V_y+V_2\right)\right) * f'\left(V_y+V_2\right) \quad (1.a7)$$

Linearization:

(1.a6) $\Rightarrow$
$$-u_{cci1}{w_1}^2 dv_{i1} = \alpha_i\left(S_1 g_1'' f_1' f_1 + g_1' f_1' + \beta(1-\delta) g_2'' {f_2}^2 h_1'\right) dS_1 + \alpha_i\left[{S_1}^2 g_1'' {f_1'}^2 + S_1 g_1' f_1'' + \beta\left(g_2'' {f_2}^2 {h_1'}^2 + g_2' f_2 h_1''\right)\right](dV_o + dV_1) + \alpha_i \beta h_1'(g_2' f_2' + S_2 g_2'' f_2' f_2)(dV_y + dV_2) \quad (1.a9)$$

(1.a7) $\Rightarrow$
$$-u_{cci2}{w_2}^2 dv_{i2} = \alpha_i \beta(1-\delta)(g_2' f_2' + S_2 g_2'' f_2' f_2) dS_1 + \alpha_i \beta h_1'(g_2' f_2' + S_2 g_2'' f_2' f_2)(dV_o + dV_1) + \alpha_i \beta\left({S_2}^2 g_2'' {f_2'}^2 + g_2' f_2''\right)(dV_y + dV_2) \quad (1.a10)$$

Define $\omega_{it} = |1/u_{ccit}|\ \forall\ t = 1,2$, $a_t = S_t^2 g_t'' f_t'^2 + S_t g_t' f_t''\ \ \forall\ t = 1,2$, $b_t = S_2 g_2'' f_2' f_2 + g_2' f_2'$ $\forall\ t = 1,2$, $b = S_2 g_2'' f_2' f_2 h_1' + g_2' f_2' h_1'$, $e = g_2'' f_2^2 h_1'^2 + g_2' f_2 h_1''$, and $k = g_2'' {f_2}^2 h_1'$

(1.a9) $\Rightarrow$
$${w_1}^2 dv_{i1} = \alpha_i \omega_{i1}(b_1 + \beta(1-\delta)k) dS_1 + \alpha_i \omega_{i1}(a_1 + \beta e)(dV_o + dV_1) + \alpha_i \omega_{i1} \beta b(dV_y + dV_2) \quad (1.a11)$$

(1.a10) $\Rightarrow$
$${w_2}^2 dv_{i2} = \alpha_i \omega_{i2} \beta(1-\delta) b_2 dS_1 + \alpha_i \omega_{i2} \beta b(dV_o + dV_1) + \alpha_i \omega_{i2} \beta a_2(dV_y + dV_2) \quad (1.a12)$$

Sum up (11) for $\alpha_i \geq \alpha_1^*$ and (12) for $\alpha_i \geq \alpha_2^*$

(1.a11) $\Rightarrow$
$${w_1}^2 \sum_{\alpha_i \geq \alpha_1^*} dv_{i1} = \sum_{\alpha_i \geq \alpha_1^*} \alpha_i \omega_{i1}(b_1 + \beta(1-\delta)k)\, dS_1 + \sum_{\alpha_i \geq \alpha_1^*} \alpha_i \omega_{i1}(a_1 + \beta e)\,(dV_o + dV_1) + \sum_{\alpha_i \geq \alpha_1^*} \alpha_i \omega_{i1} \beta b\,(dV_y + dV_2)$$

(1.a12) $\Rightarrow$

$$w_2{}^2 \sum_{\alpha_i \geq \alpha_2^*} dv_{i2} = \sum_{\alpha_i \geq \alpha_2^*} \alpha_i \omega_{i2} \beta (1-\delta) b_2 \, dS_1 + \sum_{\alpha_i \geq \alpha_2^*} \alpha_i \omega_{i2} \beta b \, (dV_o + dV_1) + \sum_{\alpha_i \geq \alpha_2^*} \alpha_i \omega_{i2} \beta a_2 \, (dV_y + dV_2)$$

Recall (1.a5):

$$dV_1 = m_1 d\underline{v}_1 + \textstyle\sum_{\alpha_i \geq \alpha_1^*} dv_{i1} \Rightarrow \textstyle\sum_{\alpha_i \geq \alpha_1^*} dv_{i1} = dV_1 - m_1 d\underline{v}$$

$$dV_2 = \textstyle\sum_{\alpha_i \geq \alpha_2^*} dv_{i2} \Rightarrow \textstyle\sum_{\alpha_i \geq \alpha_2^*} dv_{i2} = dV_2$$

$\Rightarrow$

$$w_1{}^2 (dV_1 - m_1 d\underline{v}) = \sum_{\alpha_i \geq \alpha_1^*} \alpha_i \omega_{i1} (b_1 + \beta(1-\delta)k) \, dS_1 + \sum_{\alpha_i \geq \alpha_1^*} \alpha_i \omega_{i1} (a_1 + \beta e) \, (dV_o + dV_1) + \sum_{\alpha_i \geq \alpha_1^*} \alpha_i \omega_{i1} \beta b \, (dV_y + dV_2)$$

$\Rightarrow$

$$w_2{}^2 (dV_2) = \sum_{\alpha_i \geq \alpha_2^*} \alpha_i \omega_{i2} \beta (1-\delta) b_2 \, dS_1 + \sum_{\alpha_i \geq \alpha_2^*} \alpha_i \omega_{i2} \beta b \, (dV_o + dV_1) + \sum_{\alpha_i \geq \alpha_2^*} \alpha_i \omega_{i2} \beta a_2 \, (dV_y + dV_2)$$

At steady state, $V_1 = V_y$, $V_2 = V_o$, and $S_1 = S_2 = S = h(V_1 + V_2)/\delta$:

$$[w_1{}^2 - \sum_{\alpha_i \geq \alpha_1^*} \alpha_i \omega_{i1} (a_1 + \beta e) - \sum_{\alpha_i \geq \alpha_1^*} \alpha_i \omega_{i1} \beta b] dV_1 - \left[\sum_{\alpha_i \geq \alpha_1^*} \alpha_i \omega_{i1} (a_1 + \beta e) + \sum_{\alpha_i \geq \alpha_1^*} \alpha_i \omega_{i1} \beta b \right] dV_2 - \sum_{\alpha_i \geq \alpha_1^*} \alpha_i \omega_{i1} (b_1 + \beta(1-\delta)k) \, dS = w_1{}^2 m_1 d\underline{v} \tag{1.a13}$$

$$-\left(\sum_{\alpha_i \geq \alpha_2^*} \alpha_i \omega_{i2} \beta b + \sum_{\alpha_i \geq \alpha_2^*} \alpha_i \omega_{i2} \beta a_2 \right) dV_1 + \left(w_2{}^2 - \sum_{\alpha_i \geq \alpha_2^*} \alpha_i \omega_{i2} \beta b - \sum_{\alpha_i \geq \alpha_2^*} \alpha_i \omega_{i2} \beta a_2 \right) dV_2 - \sum_{\alpha_i \geq \alpha_2^*} \alpha_i \omega_{i2} \beta (1-\delta) b_2 \, dS = 0 \tag{1.a14}$$

$h'dV_1 + h'dV_2 - \delta dS = 0$ (1.a15)

Since $V_1 + V_2 = V^*$ does not change over time at steady state, $a_1 = a_2 = a$ and $b_1 = b_2 = b/h'$. Set $B_1 = -\sum_{\alpha_i > \alpha_1^*} \alpha_i \omega_{i1}[a + \beta e + \beta b + \frac{1}{\delta} b + \frac{1}{\delta}(1-\delta)\beta k h']$, $B_2 = -\sum_{\alpha_i > \alpha_2^*} \alpha_i \omega_{i2}[a + \frac{1}{\delta} b]$, we have:

$\Rightarrow$ $$({w_1}^2 + B_1)dV_1 + B_1 dV_2 = {w_1}^2 m_1 d\underline{v}_1 \quad (1.a16)$$

$\Rightarrow$ $$B_2 dV_1 + ({w_2}^2 + B_2)dV_2 = 0 \quad (1.a17)$$

Solve the equations:

$\Rightarrow$ $$dV_1 = \frac{w_1^2(w_2^2 + B_2)}{(w_1^2 + B_1)(w_2^2 + B_2) - B_1 B_2} m_1 d\underline{v} \quad (1.a18)$$

$\Rightarrow$ $$dV_2 = \frac{-w_1^2 B_2}{(w_1^2 + B_1)(w_2^2 + B_2) - B_1 B_2} m_1 d\underline{v} \quad (1.a19)$$

$\Rightarrow$ $$dV_1 + dV_2 = \frac{w_1^2 w_2^2}{(w_1^2 + B_1)(w_2^2 + B_2) - B_1 B_2} m_1 d\underline{v} \quad (1.a20)$$

For those who have $\alpha_i > \alpha_t^*$ in both periods ($t = 1, 2$), we have:

(1.a11)
$\Rightarrow$ $$dv_{i1} = \frac{\alpha_i \omega_{i1}[a + \frac{1}{\delta} b + \beta(e + b + \frac{1}{\delta}(1-\delta)kh')]w_2^2}{(w_1^2 + B_1)(w_2^2 + B_2) - B_1 B_2} m_1 d\underline{v} \quad (1.a21)$$

(1.a12)
$\Rightarrow$ $$dv_{i2} = \frac{\alpha_i \omega_{i2} \beta(a + \frac{1}{\delta} b) w_1^2}{(w_1^2 + B_1)(w_2^2 + B_2) - B_1 B_2} m_1 d\underline{v} \quad (1.a22)$$

Proof of Proposition 1:

$\varepsilon \stackrel{\text{def}}{=} \left|\frac{dP/P}{dg'(P)/g'(P)}\right| < 1 \Rightarrow \left|\frac{dP_2/P_2}{dg'(P_2)/g'(P_2)}\right| < 1 \Rightarrow \frac{-dP_2/P_2}{dg'(P_2)/g'(P_2)} < 1 \Rightarrow \frac{-g_2'}{S_2 g_2'' f_2} < 1$

$\Rightarrow \frac{-g_2' f_2' h_1'}{S_2 g_2'' f_2 f_2' h_1'} < 1 \Rightarrow S_2 g_2'' f_2' f_2 h_1' + g_2' f_2' h_1' < 0 \Rightarrow b < 0$

Since $a < 0$, $e < 0$, $b < 0 \Rightarrow B_1 > 0$, $B_2 > 0$, and $(w_1^2 + B_1)(w_2^2 + B_2) - B_1 B_2 > 0$

By Eq. (1.6) to (1.10), $\Rightarrow \frac{dv_{i1}}{d\underline{v}} < 0$, $\frac{dv_{i2}}{d\underline{v}} < 0$, $\frac{dV_1}{d\underline{v}} > 0$, $\frac{dV_1}{d\underline{v}} < 0$, and $\frac{dV}{d\underline{v}} > 0$.

Proof of Proposition 2:

Given $(w_1^2 + B_1)(w_2^2 + B_2) - B_1 B_2 > 0$,

$B_2 < 0 \Leftrightarrow a + \frac{1}{\delta} b > 0 \Leftrightarrow b > -\delta a \Leftrightarrow \left|\frac{dP/P}{dg'(P)/g'(P)}\right| \left(\frac{-\delta a}{g' f' h'} - 1\right) < -1$

$\Leftrightarrow \varepsilon \left(\frac{-\delta(S^2 g'' f'^2 + S g' f'')}{g' f' h'} - 1\right) < -1 \Leftrightarrow \varepsilon \left(\frac{-\delta(S^2 g'' f'^2 + S g' f'') - g' f' h'}{g' f' h'}\right) < -1$

$\Leftrightarrow \varepsilon \left(\frac{\delta(S^2 g'' f'^2 + S g' f'') + g' f' h'}{g' f' h'}\right) > 1 \Leftrightarrow \varepsilon \left(\frac{S g'' f'^2 h + g' f'' h + g' f' h'}{g' f' h'}\right) > 1$

Since $\varepsilon > 0$, we have $\frac{S g'' f'^2 h + g' f'' h + g' f' h'}{g' f' h'} > 0$

$\Leftrightarrow \varepsilon > \frac{g' f' h'}{S g'' f'^2 h + g' f'' h + g' f' h'} = 1 - \frac{S g'' f'^2 h + g' f'' h}{S g'' f'^2 h + g' f'' h + g' f' h'} = 1 + \frac{1}{\frac{h'}{h}\left(\frac{-g' f'}{S g'' f'^2 + g' f''}\right) - 1}$

$\Leftrightarrow \varepsilon > 1 + \frac{1}{\frac{h' V}{h}\left(\frac{-g' f'}{V(S g'' f'^2 + g' f'')}\right) - 1} = 1 + \frac{1}{\eta \phi - 1}$, where $\eta \equiv \frac{dh/h}{dV/V} > 0$, $\phi \equiv \frac{-g' f'}{V(S g'' f'^2 + g' f'')} > 0$

CHAPTER 2
DOES COMPULSORY VOLUNTEERING AFFECT SUBSEQUENT BEHAVIOUR? EVIDENCE FROM A QUASI EXPERIMENT IN CANADA

2.1 INTRODUCTION

Volunteer behaviour is interesting to both researchers and policymakers, not only as one of the major channels of public good provision but also, perhaps more importantly, as a way to create social capital. Compulsory volunteering policies are sometimes seen as a method to encourage pro-social behaviour, especially in schools and universities. For example, both U.S. President Obama and former U.K. Prime Minister Gordon Brown proposed that high school students "volunteer", either as a requirement for graduation or in exchange for financial support.[4] Proponents expect such programs to benefit not only communities with extra goods and services, but also may increase future social involvement by participants, an effect that has been found for true volunteer programs. (See e.g. Frumkin et al. (2009), who find those who volunteered for AmerCorps subsequently had a higher level of civic engagement.) However, there are those who think that "compulsory volunteering" is an oxymoron, which potentially undermines the inherent

[4] "Obama's Prepared Remarks July 2, 2008" July 2, 2008. Web. 1 Feb. 2012. <http://www.thedenverchannel.com/politics/16770474/detail.html>; "Gordon Brown's plan for army of teen volunteers" 12 Apr. 2009. Web. 1 Feb. 2012. <http://www.telegraph.co.uk/news/politics/5143911/Gordon-Browns-plan-for- army-of-teen-volunteers.html>

altruistic nature of volunteering, devalues the experience of all people involved, and overruns the capacity of the nonprofit sector to provide well-structured volunteer programs.

This paper attempts to provide evidence for this debate by empirically investigating the effect of a provincial compulsory volunteering policy introduced in Ontario, Canada. While it is not usual to force an individual to, or not to, consume a private good unless an externality is involved (e.g. forbidding cigarettes in public places) compulsory volunteer policy may also be justified for encouraging the provision of public goods. Volunteer work can substitute for paid human resources in the public sector. Moreover, as per Andreoni (2006), even if contributions are used to provide private goods for charity recipients, their private consumption becomes a public good as it is a concern of the contributors as well. However, compulsory volunteer policies are unlikely an optimal policy instrument if the only goal is to provide public goods. For example, the compulsory volunteer policy in Ontario that mandates a fixed number of community service hours as a requirement for high school graduation can be seen as akin to a specific tax on high school certificates, which is unlikely to be an optimal tax.

However, the compulsory volunteer policy may still be appealing if it can encourage long-run pro-social behaviour, e.g. individuals' subsequent volunteering. Firstly, the policy may have a direct effect on the preference for volunteering through habit formation. Secondly, it can also indirectly boost volunteer activities by stimulating social capital accumulation. Moreover, it may help build non-cognitive skills for students, which have been found to have as much return as cognitive skills in the human capital literature. In this paper, we attempt to provide evidence on individuals' direct behavioural response to the policy, both short-run and long-run, and also on the indirect policy effect through social capital accumulation. We focus on volunteer participation as it is a very important index of pro-social behaviour and one that is most likely to be affected by the policy of interest. Additionally, it is also an indicator of motivation and self-esteem, which are important components of non-cognitive skills. So it may shed some light on the policy effect on non-cognitive skill formation.

Our empirical results suggest that the policy increased the volunteer participation rate during high school. However, we do not find evidence that the compulsory volunteer policy affected individuals' volunteer behaviour beyond high school. Moreover, the investigation on the indirect effect shows evidence of crowd-out, which exceeds the social capital accumulation effect and imposes a net negative effect on non-compulsory volunteering. In summary, while the policy is effective in increasing the volunteer participation rate in high school, policy makers should not neglect its negative impacts on both other "voluntary volunteers" and future volunteering by those subject to the policy.

The remainder of this paper is organized as follows. Section 2.2 provides a literature review. Section 2.3 describes the compulsory volunteer policy in Ontario. Section 2.4 discusses theories and policy issues. In Sections 2.5 and 2.6, we describe our data sources and outline our estimation methods. The empirical results are presented in Section 2.7. Section 2.8 provides a robustness check on the attrition. Finally, Section 2.9 offers a conclusion.

2.2 LITERATURE REVIEW

Volunteering is an important topic in public and labour economics, e.g. Freeman (1997), Andreoni (2006), and Yoruk (2008), and compulsory volunteering has been frequently examined in the psychology literature. Recent studies have attempted to establish whether there is a causal relationship between previous volunteer activity and the future intention to volunteer. Metz and Youniss (2003, 2005) exploit a high school level quasi-experiment in a middle class suburban town in Massachusetts where the school board required all students who enrolled after 2001 to perform 40-hours of community service as a prerequisite for high school graduation. They follow three cohorts of students -those who enrolled in 2000, 2001, and 2002- at three points in time: the beginning of the 11th grade, the end of the 11th grade, and the end of the 12th grade. They divide both of the treatment and comparison groups by students' inclination to service. While for the comparison group (2000 cohort) the more-inclined group was defined as having volunteered in at least two of the three pre-policy years, during grades 10 through 12, for the treatment group (2001 and 2002 cohorts) it is defined as students who completed the 40-hour requirement before the end of 11th grade. By comparing these groups, they find that required volunteering significantly increases the future volunteer intention of the less-inclined group, but does not affect that of the more-inclined group. They find the same pattern on intention to vote and to join a civic organization, and on the interest in discussing political issues.

Henderson et al. (2007) explore the same policy change that is the focus of this paper. They survey 1768 first-year students regarding their perceptions and attitudes, the nature and amount of previous and current volunteering, and other measures of civic and political engagement. By comparing the after to the before group, they find that 1) the mandatory policy significantly increased the probability of volunteering in the last year of high school; 2) previous volunteer experience has a positive and significant effect on the attitude toward volunteering; 3) after controlling for previous volunteer experience, the mandated policy indicator has an estimated negative but statistically insignificant effect on volunteering attitude; 4) the effects of volunteer work on other measures of civic engagement are generally insignificant.

Taylor and Pancer (2007) define a measure of the quality of community service experience and find it is positively correlated with future volunteering intentions. Their study's conclusion are in line with those from an earlier one by Stukas, Snyder, and Clary (1999), who investigate a group of college students who were required to volunteer in order to graduate. They find stronger perceptions of external control eliminate the positive relation between prior volunteer experience and future intentions to volunteer. They also find that students who initially felt unlikely to volunteer had lower intentions after being required, rather than given an opportunity, to serve. Those who initially felt more likely to volunteer were relatively unaffected by the mandated service.

These findings suggest that the compulsory volunteer policy may increase individuals' future intention to volunteer as long as compulsory volunteer programs are well implemented and provide positive experiences for volunteers. However, it is unclear whether the intentions translate into actual volunteer participation.

Using a panel of college students in the National Education Longitudinal Study of 1988, Marks and Jones (2004) define three dependent variables: 1) dropped-service (1 if volunteered in high school but not after, 0 otherwise); 2) began-service (1 if not volunteered in high school but did after, 0 otherwise); 3) sustained-service (1 if volunteered both in and after high school, 0 otherwise). Using logistic models, they find that students who were required to volunteer in Grade 12 were more likely to drop service after high school compared with those who were not subject to compulsory volunteer policies. However, it is not clear from their findings whether those required to volunteer had a higher subsequent propensity to volunteer than they would have otherwise.

A study with some similarity to ours is Helms (2006), who investigates the effect of the service requirement for public high school graduation in Maryland. She estimates that the policy increases the volunteer participation by around 7.3% in Grade 8 but has zero or negative effects in Grade 12. One difference between the Maryland service requirement policy and the Ontario policy studied here, is that the Maryland requirement is largely implemented as part of the school curriculum, while the mandated volunteer activity in Ontario is explicitly defined as extracurricular work and in large implemented through the interaction between schools, families, and local charitable organizations. Nevertheless, her findings on Grade-eight students are similar to our estimate for the direct effect during high school. Moreover, as most of the service requirement is fulfilled by the time students complete the tenth grade, her findings on Grade-12 students are perhaps somewhat comparable to our estimate for the direct effect after high school. In addition, we are able to use our data to follow those affected by the policy for several years after high school and hence estimate longer-run effects, as well as to study individuals not directly affected by the policy whose volunteering may potentially have been crowded out by mandated volunteering.

2.3 THE COMPULSORY VOLUNTEER POLICY IN ONTARIO

The Canadian province of Ontario's Education Ministry started to mandate community service as a condition for high school graduation in 1999. All students who began high school in 1999 or later in Ontario must complete 40 hours of community service in a suitable community placement before graduation. According to the *Ontario Secondary School, Grade 9 to 12, Program and Diploma Requirements* (1999), the service should benefit communities, but its primary purpose is to contribute to students' development. It may take place in a variety of settings, including businesses, not-for-profit organizations, public sector institutions (including hospitals), and informal settings but must not be for pay or for academic credit, and should be completed outside of normal instructional hours. It is the students' responsibility to maintain and submit to their principals records of their service hours signed by the organization or person supervising the activities.

Even though the policy change was initiated at the provincial level, it was largely funded and implemented by school boards and demanded cooperation by students' families as well as local communities. Brown et. al. (2007) examined the program and found: 1) except for a general guideline, the Ministry of Education left implementation to school boards; 2) placements for not-for-profit agencies are preferred but those at for-profit organizations or involving informal helping are also accepted, especially in rural schools which usually have fewer nearby not-for-profit organizations; 3) in most schools the primary responsibility for finding a community service placement is left to the student and his/her family. The program provides considerable flexibility to school boards and students, which helps to minimize the perception of external control.

An issue that should be mentioned is that the Ontario academic credit (OAC) year, or Grade 13, was eliminated along with the introduction of the compulsory volunteer policy in 1999. Before the policy change, Grade 13 was not required to receive an Ontario Secondary School Diploma (OSSD). Students could graduate high school after Grade 12 and many students did take this option. However, this option was largely for students who did not intend to directly enter universities since Canadian universities, and indeed most universities in North America, required OAC for admission of Ontario students.[5] Therefore, with the OAC year, it was often seen that high school students either graduated after Grade 12 and started to work or went to community college, or graduated after Grade 13 and went to university. After the elimination of Grade 13, all high school students graduated after Grade 12. The OAC year was replaced with an extra ten days of schooling in each high school grade. In practice, the elimination of Grade 13 also generated a "double-cohort", in which the last Grade 13 cohort and the first Grade 12 cohort graduated in the same year. In the empirical model, there is an attempt to control for the confounding of the elimination of Grade 13; see Section 2.5.3.

2.4 THEORIES AND POLICY ISSUES

As volunteers produce public goods, the policy not only affects individuals who are subject to it but also imposes an externality on the volunteer choice of all individuals. While the policy may increase the volunteer participation of high school students, it is also worthwhile to examine the

[5] To enter university, students were required to complete 30 high school credits including six at the OAC level. One could complete an OAC course before the OAC year and a student who had completed 30 credits with six OACs would be permitted to graduate. This practice was called fast-tracking. However, it was not common due to the heavy workload.

policy effect on volunteer behaviour after those students leave high school as the compulsory volunteer policy is aimed at improving students' development. On one hand, the policy might engender a positive effect on the volunteer choice in subsequent periods through habit formation and/or information effects. This was envisioned by those drafting the policy. On the other hand, to mandate an individual to "volunteer" work that she would not normally choose may decrease her future volunteer hours e.g. through inter-temporal substitution. Therefore, the direct effect on mandated individuals after the effective period depends on which effect is dominant. Other effects are also possible. When local organizations are flooded with student volunteers the quality of the volunteer experience may deteriorate, discouraging future volunteering. Over time, as organizations adjust to the new policy, the importance of this issue may reduce. Additionally, there may be peer effects that could be either positive (from associating with enthusiastic volunteers), or negative (from associating with disgruntled students being forced to volunteer). Overall, while the direct effect of the compulsory policy is expected to positively affect the volunteer participation during high school, its impact after high school is ambiguous.

Besides the direct effects on individuals who have been mandated to volunteer, the policy may also have indirect effects through crowd-out and/or social capital accumulation. For those who do not have the mandate and those who have fulfilled it, their non-compulsory volunteering may be crowded out by the "compulsory" volunteer hours akin to what the literature has documented regarding government grants affecting charitable donations, e.g. Andreoni and Payne (2003). The policy could also encourage the social capital accumulation, which may have a positive effect on volunteering.[6] As per Putman (1993, 1995), social capital is the combination

[6] However, Yang (2012) explores a theoretical model in which this social capital accumulation increases productivity in the production of the public good. Depending on the elasticity of demand for the public good, a compulsory volunteer policy may increase production of the public good but actually decrease post-school volunteering.

of networks, norms, and social trust that facilitate coordination and cooperation for mutual benefit. The policy can help build up the social network in communities, e.g. relationships between individuals and charities. Yoruk (2008) finds that personal solicitations have considerable effect on the propensity to volunteer. Moreover, the policy might bring about scale economies in the production of public goods as it increases the total supply of volunteer hours, e.g. charities may recruit/train specialists to train and organize volunteer human resources. Given these many direct and indirect effects of opposite signs, the net effect is an empirical question.

2.5 DATA

Canada's longitudinal and nationally-representative Youth in Transition Survey (YITS) is employed. It consists of two cohorts collected separately. Cohort A, comprising 15-year-olds in 1999, was attached to the OECD Programme for International Student Assessment (PISA 2000). Cohort B, which covers 18-to-20-year-olds in 1999, was selected using the sampling frame of the Labour Force Survey. In total, almost 30,000 youth aged 15 (cohort A), and more than 22,000 youth aged 18 to 20 (cohort B) from the ten provinces participated in the first cycle of the YITS in 2000. Follow-up interviews with the YITS participants took place in 2002, 2004, 2006, and 2008. At the time of the 2008 interview, the two cohorts were aged 23, and 26 to 28.

As the sampling structure of cohorts A and B differ, sampling weights are employed to align both cohorts to the population. Both weighted and unweighted results are reported for comparison. The cumulative attrition is about 50% in the YITS. A robustness check using only individuals who had remained in the sample for at least four cycles, that is who were observed from 2000 to 2006 or 2008, is conducted to examine its influence.

2.6 IDENTIFICATION STRATEGY AND EMPIRICAL ISSUES

Since the compulsory volunteer policy change is assumed exogenous to individuals' volunteer behaviour, we implement two sets of difference-in-differences approaches using youth in other provinces as a comparison group to estimate the direct and indirect effects. In Ontario, the treatment province, there are two groups affected by different components of the policy effect after it was introduced in 1999. While students who enrolled in high school before 1999, i.e. the before group, would only experience the indirect effect, students who enrolled after 1999, i.e. the after group, would be affected by both the direct and indirect effects. Since identification of the direct effect lies on comparing the after group to the before group, we make two functional form assumptions to ensure that each effect is identifiable. First, the direct and indirect effects are assumed to be additive and separable. Second, the indirect effect is assumed to be the same for these two Ontario groups.

2.6.1 The direct effect

Conceptualizing the policy change in Ontario as a quasi-experiment with the Ontario high school students as the treatment group and those in other provinces as the comparison group, we use the difference-in-differences approach to identify the direct effect. We define those who were enrolled grade 9 (first year of high school in Canada) before 1999 as the before group and those who were enrolled after as the after group. The volunteer participation of the before and after groups is in the same period and hence are affected by the indirect effect. Their difference should present the direct impact of the policy. We estimate the following model to compare the volunteer outcome of the treatment-comparison groups and before-after groups:

$$v_{ipt} = \alpha_0 + \beta_0 Direct_{ip} + \delta_0 HSyear_i + \mu_0 HSprov_p + [\gamma_0 X_{it}] + \varepsilon_{ipt} \tag{2.1}$$

where v_{ipt} is the volunteer indicator for individual i in province p at period t; $Direct_{ip}$ is the direct effect indicator that equals one for Ontario students who enrolled in high school after

1999, and zero otherwise; $HSyear_i$ is the before-after group indicator; and $HSprov_p$ is a set of province indicators controlling for the high school province fixed effect. X_{it} is a vector of controls in brackets to indicate that X may be omitted and that the elements of X vary in specifications when included. ε_{ipt} is the error term, which may be arbitrarily correlated within provinces, but is treated as independent across provinces. The issue of the clustered error is discussed in Section 2.6.4. As the dependent variable is an indicator, the model is estimated using both linear probability and logit models.[7]

The key assumption for the difference-in-differences approach to be valid is the "common trend" assumption that the difference between the before-after groups of comparison provinces is the same as that of the treatment province without the treatment, i.e. the compulsory volunteer policy. The YITS sampled only those who were 15 years old by the end of 1999 so that the before group has the same birth year as the after group but is a grade more senior. Perhaps because school admission age is to some degree chosen by families rather than administratively assigned, the before and after groups are somewhat different in the data. However, the "common trend" assumption holds as long as the difference is the same across provinces.

Table 2.1 presents some descriptive statistics for the four groups. While the sample size of these groups varies significantly, Ontario is very similar to other provinces except for having fewer Canadian citizens, more visible minorities, and a more urban population. Most importantly, we can see that the difference between the before and the after group is similar over the Ontario and non-Ontario groups in most variables. This makes it likely that the after group in

[7] Ai and Norton (2003) argue that the usual marginal effect of an interaction term in a nonlinear model requires an adjustment to be interpreted as an estimate of the actual effect. However, Puhani (2008) shows the conclusions of Ai and Norton (2003) are irrelevant for the estimation of the treatment effect in a nonlinear difference-in-differences model because the cross difference of the conditional expectation of the potential outcome without treatment in a nonlinear model, unlike in a linear model, is not zero. In any case, we report the estimates of LPM for comparison. The results of the LPM are similar to the logit model.

non-Ontario provinces provides a "good" counterfactual case of the after group in Ontario without treatment. In Section 2.6.3, we will further discuss the grade selection issue caused by the elimination of Grade 13.

We are interested in both the short- and long-term direct effect. The longitudinal nature of the YITS allows us to estimate a profile of the policy effect over time. Members of cohort A have been interviewed biennially for five cycles, among which the first two cycles were during high school (age 15 and 17). By repeating the difference-in-differences regression on these cycles, we can plot the estimated policy effect on each age stage from their adolescence to early adulthood. It is of particular significance to compare the effects over time for the volunteer choices in different periods made by the same individuals.

Cohorts A and B are pooled in the regressions to increase the sample size. Also, pooling makes it possible to control for the time fixed effect. When we estimate the policy effect at age 19, for example, the difference-in-differences regression might capture certain time specific effects since all cohort A observations of age-19s are observed in 2003. Pooling cohort B introduces observations of age-19s in 1999.

2.6.2. THE INDIRECT EFFECT

Another set of difference-in-differences models are employed to identity the indirect effect. While we still compare Ontario to other provinces, the volunteer participation change of those who enrolled in high school before 1999 (the before group) before and after the introduction of the policy provides the indirect effect. Since students who enrolled in Ontario high school after 1999 were affected by the direct effect, we exclude all those who enrolled in high school after 1999 (the after group) in the indirect effect regressions. We estimate the following model:

$$v_{ipt} = \alpha_1 + \beta_1 Indirect_{pt} + \delta_1 Year_t + \mu_1 Prov_p + [\gamma_1 X_{it}] + e_{ipt} \qquad (2.2)$$

where v_{ipt} is the volunteer flag for individual i in province p at period t; $Indirect_{pt}$ is the indirect effect indicator that equals one for students in Ontario after 1999, and zero otherwise; $Year_i$ and $Prov_p$ are year and province fixed effects respectively and X_{it} is a vector of optional controls. The error term, e_{ipt} is assumed be correlated within provinces but independent across provinces.

Since the reference year of the first cycle of YITS is 1999, we have only one period available before the treatment. Unfortunately, the policy took effect on September, 1999 so that the before group actually had only eight months before the policy was officially introduced. However, the indirect effect we intend to identify probably takes time to have an observable effect on individuals' volunteer behaviour. The mandated students had to find suitable volunteer placements first unless they had voluntarily volunteered. Charities would need time to change their recruiting strategy, especially as the government did not provide any extra funding. Therefore we can reasonably expect some months to pass before the policy had real impacts on those who were not directly subject to it. Even though there might be some instant indirect effects, they would only influence the value of the volunteer participation indicator for those who did some volunteer work after September but had not done any volunteer work from January to August, likely a small proportion. Therefore, the observations from 1999 most probably provide an acceptable measure for the volunteer behaviour before the treatment in studying the indirect effect. However, we need to interpret the empirical results with caution since they may underestimate the volunteer participation before the policy.

We separately employ the difference-in-differences model both on the pooled data, which includes observations of age-17-to-23s in cohort A and B, and on two age groups, i.e. age-19-to-20s and age-20-to-21s. While the regression using the pooled data provides an overall average effect, the age group regression allows heterogeneous effects for different age groups and provides us a chance to find out how the indirect effect works and where it focuses.

2. 6.3. THE "DOUBLE COHORT" EFFECT

As the compulsory volunteer policy was introduced, Grade 13 was simultaneously eliminated in Ontario in 1999. This may put some stress on the "common trend" assumption for the difference-in-differences approach. Firstly, the elimination of Grade 13 was likely to affect the transition status from high school and to the early adulthood, including the probability of high school graduation, of post-secondary school participation, and of labour force participation, and hence had impacts on the volunteer participation. Secondly, according to the literature about the civic return to education, the decrease in schooling years per se may impose a negative effect on the volunteer behaviour. Lastly, parents and students were likely to make adjustments in terms of grade selection in order to try to avoid the double cohort year. Given grade selections are costly, this more likely occurred for families that had a high expectation of the child's education outcome.

Two approaches are employed in an attempt to disentangle our key results from these confounders. Firstly, we control for a set of covariates regarding each individual's transition status. Three status indicators are included in regressions to control for whether a respondent was attending high school, in her high school graduating year, and enrolling in post-secondary education. Secondly, current schooling years are added to control for the one-less-high-school-year effect.[8] Additionally, a "double-cohort" indicator variable is added to control for the cohort fixed effect. A "private high school" indicator is also included.

[8] In an alternative specification where the highest diploma obtained instead of actual schooling years is used as the control for education level, the estimates are generally unchanged.

Secondly, we conduct analysis on subgroups that are less likely subject to the grade selection issue. This is based on an education expectation variable that is available only for cohort A, who were asked a question in the first cycle at age 15 about their education expectations, i.e. "What is the highest level of education you think you would like to get?". Based on this question, we define three education expectation indicators, namely "university-intender", "community-college-intender", and "high-school-only-intender". Table 2.2 shows that the percentage of university-intenders in the before group in Ontario is significantly higher than that of the comparison group. This is consistent with the prior that students with high education expectations have more incentives to switch to the early cohort to acquire marginal advantages. We then separately investigate the policy effect for these groups of university-intenders, college-intenders, and high-school-only-intenders.

Moreover, the sub-group analyses also help mitigate the confounding of the effect of one less schooling year on the volunteer behaviour. Because the optional Grade 13 was not as valuable for non-university-intenders as for university-intenders, fewer of the former took Grade 13 when it was available. Table 2.2 presents the percentage of students who took 13 years of secondary school in Ontario, which shows the percentage decreased from 64.8% to 6.2% for university-intenders, and from 26.7% to 4.9% for non-university-intenders. Therefore, we expect to observe that the elimination of Grade 13 imposed somewhat smaller effects on non-university-intenders.

2.6.4. DIFFERENCE-IN-DIFFERENCES WITH CLUSTERED SAMPLE

In this study, we try to estimate the effect of a policy that occurs only once in one jurisdiction using difference-in-differences approaches. Recent literature has found some practical problems with this approach. Bertrand, Duflo and Mullainathan (2004) find over-rejection of the null hypothesis to be close to ubiquitous in difference-in-differences specifications with jurisdiction-level policy changes.[9] Donald and Lang (2007) point out that the degrees of freedom used in inference are not determined by the number of individual observations in the dataset, but are closer to the number of clusters potentially subject to the policy change.

Following the most common approach to addressing this problem, we adjust the standard errors for clustering and conduct inference using t-tests where the degrees of freedom are the number of clusters minus one.[10] However, this strategy encounters finite sample problems when the number of clusters is small, as is the case in our context.

As there are only ten cluster provinces and one single policy change in this study, we compute the confidence interval of the policy effect estimates as per Conley and Taber (2011) (referred to as CT hereafter), in addition to the inference based on clustered standard errors and asymptotic assumptions.[11] Under the assumption that the before-after difference of the treatment group without treatment follows the same distribution as that of the control group, CT construct the confidence interval of the treatment effect from the information from the control groups using permutation test techniques. Intuitively, if the null hypothesis is true, e.g. the treatment effect is zero, the before-after difference of the treatment group should be one random draw from the distribution of that exhibited by the control group. We employ the population-weighted linear probability model as it is comparable to the logit model results based on individual level data. In addition to being an alternative inference tool, CT show that their confidence interval can also

[9] Bertrand, Duflo, and Mullainathan (2004) find in the case of difference-in-differences that if 1) the dependent variable is positively serial correlated; 2) it is based on long time series; and 3) the treatment variable varies very little within a group over time, more accurate inferences are obtained if clustering of the variance-covariance is on the group level rather than on the group-period level.

[10] The standard error is clustered at the province level and the degrees of freedom is adjusted to G-1 (the number of clusters) in obtaining critical values.

[11] Cameron, Gelbach and Miller (2008) propose bootstrap-based approaches to improve inference in a similar context. However, their approach does not work when there is only one policy change.

serve as a consistent interval estimator for the policy effect, as they also show that the difference-in-differences point estimator is unbiased but inconsistent when there exist clustered error terms and the number of treatment groups is small.

There are two practical issues. First, given it is more complex to employ sampling weights with the CT approach and the weighted and unweighted results are generally similar, we compute the CT confidence intervals for unweighted models only. Second, as we have one single policy change and nine comparison groups, the estimated CDF is a step function and the probability increments are limited to 1/10. Under this context, the smallest confidence interval that can be identified for a two-tailed test is 80% (0.80=1-2*1/10). While this is lower than conventional significance levels, it provides a robustness check. One-tailed hypothesis tests at a 10% significance level are also feasible.

2.7 Results

2.7.1 The direct effect

The direct effect examines how the policy affects individuals who are subject to the policy mandate during high school and after. As discussed in Section 2.6.1, its identification lies on the comparison of the before and after groups across provinces. Figure 2.1 presents the annual volunteer participation rate for the four groups. The two lines for the comparison group (dotted lines) show a downward trend as age increased, and more importantly, a stable difference between the before and after group over time. The after group had a volunteer participation rate of around 10% less than the before group. In Ontario, while the before group was quite similar to that of the comparison group, the volunteer participation rate of the after group was relatively high at age 15 and age 17, dropped sharply at age 19, and converged back to the common trend after age 21. Taking the difference between the after and before groups, Figure 2.2 highlights the pattern. The difference for the comparison group, the dotted line in Figure 2.2, is almost a horizontal line. However, in Ontario, the difference is algebraically much higher at age 15 and 17 when the policy was binding. At age 19, when the policy was no longer binding as high school was completed, the gap in the volunteer participation rate between the after and before groups sharply plunged below that of the comparison group. For age 21 and 23, the differences restored to the same level as the comparison group.

The figures seemingly show that the compulsory volunteer policy had some impact on individuals' volunteer participation and that the effect varied over time. As discussed in Section 2.6.1, we then employ difference-in-differences models to estimate the direct policy effect. Table 2.3 presents the results of three specifications using the pooled sample, which estimate the average direct effect on the annual volunteer participation from adolescence to early adulthood. Three types of models are employed: logit models with sampling weights, and linear probability models (LPM) with and without sampling weights. The 80% CT confidence interval is computed for the unweighted LPMs. Column (1) of Table 2.3 presents the estimate of the basic specification, which is equation (1) without X_{it}. The specification of column (2) includes demographic controls including gender, age, a citizenship indicator, and an urban indicator. On top of the specification of column (2), transition status controls discussed in Section 2.6.3 are added in the specification reported in column (3).

Table 2.3 presents the direct effect on the volunteer participation during the adolescence to early adulthood. The weighted models, both logits and LPMs, provide similar estimates. Under the assumption of a homogeneous treatment effect, these estimates are of average treatment effects. They indicate that the policy did not increase volunteer participation, taking into account both the immediate compliance during high school and the effect on subsequent volunteer behaviour after completion.

The last panel of Table 2.3 presents the direct effect estimates using unweighted LPMs. They are larger than the weighted estimates. As per Magee et al. (1998), the estimate with sampling weights is more desirable given the population coefficient is of interest. Even though the weighted and the unweighted models are slightly different, the coefficients are still statistically insignificant except for the full controlled model in column (3), which is significant at 10%. The CT 80% confidence intervals are reported for the unweighted LPMs. They are much wider than that based on asymptotic clustered standard errors, and are centered above any of the point estimates.

We next investigate the policy effect on different stages by employing the difference-in-differences model with full controls on five age groups. All control variables, including demographics and transition status variables are added. Given the age structure of the YITS, the age-15 regression uses only the observations from the first cycle (1999) of cohort A. The age-17-18 regression pools age-17s and age-18s together so that the treated group in 2001 (from the age-17s in cohort A cycle 2) is compared with two non-treated groups, the age-17s in cohort A cycle 2 and the age-18s in cohort B cycle 1. While these two regressions estimate the policy effect during high school, the age-19, age-21, and age-23 regressions estimate the policy effects after. Observations from cohort A and B of the relevant ages are used.

Table 2.4 presents the pattern over time of the direct effect. The estimates in the first two columns show that the policy imposed a statistically significant positive effect on volunteer participation during high school: 3.19% at age 15, and 5.99% at age 17-to-18. As the total mandate is only 40 hours and a student can finish it at any year during high school, it seems reasonable that the policy increased volunteer participation in all high school years but by a modest magnitude. The unweighted estimate tells the same story except the magnitudes are larger. The CT confidence intervals do not contradict these results.

Columns (3) to (5) of Table 2.4 present the direct effect after high school. While the estimates in the age-21 and age-23 regression are statistically insignificant, the age-19 regression shows that students who had been mandated to volunteer in high school were 5.19% less likely to participate in volunteer activities at age 19, which is statistically significant at the 1% level. This conclusion relies on the validity of the asymptotic assumptions. But even based on the conservative 80% CT confidence interval, we can reject the hypothesis that there is a positive direct effect at age 19 at the 10% level.

The results in Table 2.4 indicate that the compulsory volunteer policy effectively increased volunteer participation in high school but decreased it right after high school. While the positive direct effect during high school is consistent with our expectation and robust to different inferential approaches, the negative effect found in the age-19 regression deserves further investigation. Hence we estimate the age-19 regression for university-intenders, college-intenders, and high-school-only-intenders separately. As discussed in Section 2.6.3, the sub-group analysis mitigates the impacts of the grade selection issue and may disentangle the direct effect from confounders. The results are reported in Table 2.5.

Regressions in Table 2.5 are based on observations from cohort A since the education expectation variable is only available for that group. Comparing column (1) of Table 2.5 with column (3) of Table 2.4, the point estimates of the cohort-A-only regression are bigger in absolute value but the CT confidence interval provides very similar results.

Columns (2) to (5) of Table 2.5 present the result of the age-19 regression for four education expectation groups. (The non-university-intender includes the college-intender, the high-school-only-intender, and other groups, e.g. apprenticeship or trade certificates.) They show that the negative direct effect is concentrated on non-university-intender groups. While the estimated direct effect on university-intenders is statistically insignificant, it is estimated around -10% for all non-university-intender groups. Both weighted and unweighted models provide similar estimates. For the non-university-intender and college-intender group, the CT 80% confidence bounds are [-17.04%, -2.66%] and [-12.34%, -2.89%] respectively. The CT confidence interval provides a very wide range for the high-school-only-intender group as the number of observations decreases in each province-period cell, e.g. there are only 21 high-school-only-intenders in the after group of Ontario.

As discussed in Section 2.6.3, students with lower education expectations suffered less impact from the elimination of Grade 13 and the grade selection issue. With no intention to claim that they provide an average treatment effect, we expect that the sub-group analyses on non-university intenders to yield an estimate somewhat closer to the causal effect of the compulsory volunteer policy after high school. Table 2.5 shows that the estimated direct effect of the compulsory volunteer policy on volunteer participation after high school completion is estimated as negative for non-university-intenders.

The results regarding the direct effect show that students who were subject to the compulsory volunteer policy were more likely to volunteer during high school, became less likely to volunteer immediately after completion, especially for non-university-intenders, and showed no difference two years after high school completion. After taking the short-run and the long-run effects into account, the total policy effect on volunteer participation is estimated to be around zero. The estimates indicate that, while the policy is effective in encouraging volunteer participation in high school, it is unlikely to have any positive effect on the volunteer choice after completion and may not increase civic engagement in the long run.

2.7.2 THE INDIRECT EFFECT

The compulsory volunteer policy may also impose an indirect impact on volunteer behaviour through the crowd-out effect. As discussed in Section 2.6.2, we estimate equation (2) on the pooled data and two age groups, which are presented in Table 2.6. Columns (1) to (3) present the results using the pooled sample, i.e. observations from age 17 to 23, in three specifications. Columns (4) and (5) provide the estimates for age-19-to-20s and age-20-to-21s. Cohorts A and B are employed excluding students who were enrolled in high school after 1999. The basic model does not include any covariates. Demographics controls and transition status controls are gradually added in the next two specifications.

Columns (1) to (3) of Table 2.6 estimate that the introduction of the compulsory volunteer policy decreases the volunteer participation rate of those who are not subject to the policy by 6.45%. The weighted and unweighted results are similar. The CT confidence interval indicates that the negative effect ranges from -9.25% to -4.56%. The regression using the pooled data provides the average indirect effect by comparing the volunteer participation of the age-18-to-20s of cohort B in 1999 to that of cohort A in 2001, 2003, and 2005, when they are aged 17, 19, and 21. As discussed in Section 2.4, the indirect effect consists of the crowd-out effect and the social capital accumulation effect. While the former likely imposes an instant negative impact, the latter takes time to have an influence. Columns (4) and (5) present the age-19-to-20 and age-20-to-21 regressions focusing on the long-run indirect policy effect. In the age-19-to-20 regression, the age-19-to-20s in 1999, who are from cohort B, are compared with age-19s, who are from cohort A, in 2003. The main after group of the age-20-to-21 regression becomes the age-21s in 2005. Columns (4) and (5) estimate an effect that is also negative but smaller in absolute value. The results indicate that the volunteer participation rate of typical age-20s decreased by 4.78 percentage points four years after the introduction of the compulsory volunteer policy, and by 3.40 percentage points six years after it. It shows that the negative indirect effect decreased over time. The CT confidence interval of the age-20-to-21 regression covers zero, which also suggest a weaker effect in the somewhat longer run.

2.8 Robustness check: Sample attrition

The cumulative attrition rate of the YITS is around 50%, which, especially in light of Abraham, Helms, and Presser (2009), raises the concern that the observed pattern over time may be affected by the sample attrition. We conduct a robustness check using only individuals who had remained in the sample for at least four cycles, i.e. who were observed from 2000 to 2006 or 2008, to gain some insight into the impact of the sample attrition. Individuals who were observed in four cycles but left in the last cycle are included in order to avoid losing too many observations from the treated group. Table 2.7 presents estimates of the direct effect and the indirect effect of pooled sample and over different age groups. All regressions are fully controlled, and only weighted logit models are employed. The results are similar with those based on the full sample.

2.9 Conclusion

This paper attempts to investigate the interaction between individuals' volunteer participation from adolescents to early adulthood and a “compulsory volunteerism” policy. We study a provincial compulsory volunteer policy for high school students in Ontario, Canada. As it is anticipated that the policy would increase the volunteer participation during high school, it is of particular interest to test the following two hypotheses: (1) the policy had a positive effect on the subsequent volunteering behaviour; (2) the policy had a net positive environmental effect (i.e. indirect effect) on non-compulsory volunteering. These hypotheses define key components of the evaluation of compulsory volunteer polices.

Using the YITS, we find that the policy increased volunteer participation in all observed years during high school. The estimated direct effect is about 6% around ages 17 to 18. For volunteer participation after high school completion, the results show that those affected by the policy volunteered about 5% less than they otherwise would have after high school completion, and that this effect is mostly concentrated on students who did not intend to go to university at age 15. These conclusions are also supported by nonparametric confidence intervals proposed by Conley and Taber (2011), which are employed because the policy treatment occurred in a single province with potentially provincially-clustered errors.

We also find that those who were not directly affected by the policy volunteered less after the introduction of the policy, again using standard methods and the Conley and Taber approach. The total indirect effect is estimated to be about -6.45% in the subsequent eight years after the introduction of the policy with some evidence of a diminishing effect.

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Table 2.1:Means and stand errors of selected variables of four groups (cohort A and B)

	Ontario		non-Ontario	
	Started HS before 1999	Started HS after 1999	Started HS before 1999	Started HS after 1999
Male	0.504	0.573	0.504	0.589
	(0.0026)	(0.0167)	(0.0014)	(0.0035)
Visible minority	0.169	0.136	0.102	0.083
	(0.0019)	(0.0116)	(0.0008)	(0.0019)
Citizen	0.982	0.959	0.991	0.988
	(0.0007)	(0.0067)	(0.0003)	(0.0008)
Lived in urban area	0.874	0.741	0.787	0.688
	(0.0017)	(0.0148)	(0.0011)	(0.0033)
Attended private HS	0.046	0.088	0.100	0.074
	(0.0011)	(0.0095)	(0.0008)	(0.0018)
University-intenders*	0.668	0.432	0.639	0.439
	(0.0074)	(0.0328)	(0.0034)	(0.0069)
HS diploma in 2007	0.959	0.741	0.945	0.688
	(0.0029)	(0.0432)	(0.0017)	(0.0093)
PSE diploma in 2007	0.655	0.262	0.632	0.226
	(0.0071)	(0.0434)	(0.0037)	(0.0084)
Never married in 2007	0.617	0.684	0.518	0.621
	(0.0072)	(0.0458)	(0.0038)	(0.0098)
Child(ren) in 2007	0.015	0.043	0.015	0.086
	(0.0018)	(0.0199)	(0.0009)	(0.0057)
Volunteer in 2007	0.355	0.280	0.341	0.249
	(0.0071)	(0.0443)	(0.0036)	(0.0087)
Number of observations	37,485	882	131,114	20,168

Standard errors are reported in parentheses. Sampling weights are employed. *from cohort A only.

Table 2.2: Education Attainment for age-19s in Ontario (cohort A only)

	All	University -intender	Non-university -intender	College -intender	High-school-only-intender
Started HS before 1999					
High school graduate	0.950	0.985	0.885	0.919	0.747
Took 13 year Secondary Education	0.518	0.648	0.267	0.289	0.166
Number of individuals	2,596	1,751	845	668	177
Started HS after 1999					
High school graduate	0.850	0.911	0.812	0.893	0.686
Took 13 year Secondary Education	0.054	0.062	0.049	0.078	0.004
Number of individuals	113	50	62	41	21

Sampling weights are employed.

Table 2.3: Direct effect for pooled sample (cohort A and B)

	Basic	(1) + Demographics	(2) + Transition Status
	(1)	(2)	(3)
Logit model with sampling weight			
Direct effect	0.0070	-0.0017	-0.0068
	(0.0121)	(0.0105)	(0.0123)
pseudo R^2	0.018	0.032	0.053
Linear probability model with sampling weights			
Direct effect	0.0062	-0.0047	-0.0150
	(0.0109)	(0.0097)	(0.0123)
R^2	0.023	0.042	0.069
Linear probability model without sampling weights			
Direct effect	0.0229	0.0108	0.0273*
	(0.0140)	(0.0113)	(0.0132)
Conley-Taber 80% confidence interval	[-0.0000, 0.0734]	[0.0000, 0.0438]	[0.0095, 0.0684]
R^2	0.041	0.072	0.096
Number of provinces	10	10	10
Number of obs.	179,285	179,285	179,285

The marginal effect of logit model is reported with robust standard errors clustered at province level in parentheses. *p < 0.1, **p <0 .05, ***p < 0.01. See Appendix for estimates of other covariates of logits model with sampling weights.

Table 2.4: Direct effect for five age groups (cohort A and B)

	Age 15	Age 17-18	Age 19	Age 21	Age 23
	(1)	(2)	(3)	(4)	(5)
Logit model with sampling weight					
Direct effect	0.0319***	0.0599***	-0.0519***	-0.0359	-0.0265
	(0.00482)	(0.0174)	(0.00697)	(0.0300)	(0.0518)
pseudo R^2	0.035	0.055	0.052	0.043	0.052
Linear probability model with sampling weights					
Direct effect	0.0326***	0.0508**	-0.0533***	-0.0421	-0.0369
	(0.0047)	(0.0171)	(0.0082)	(0.0297)	(0.0465)
R^2	0.046	0.073	0.068	0.055	0.064
Linear probability model without sampling weights					
Direct effect	0.0809***	0.0716***	-0.0490*	0.0221	0.0099
	(0.0120)	(0.0200)	(0.0250)	(0.0368)	(0.0216)
Conley-Taber 80% confidence interval	[0.0442, 0.1002]	[0.0017, 0.1614]	[-0.0872, 0.0000]	[-0.0078, 0.1067]	[-0.0021, 0.0704]
R^2	0.041	0.060	0.056	0.063	0.059
Number of provinces	10	10	10	10	10
Number of individuals	24,807	33,307	29,197	24,094	19,051

The marginal effect of logit model is reported with robust standard errors clustered at province level in parentheses. *p < 0.1, **p <0 .05, ***p < 0.01. See Appendix for estimates of other covariates of logit models with sampling weights.

Table 2.5: Direct effect for age-19s (cohort A only)

	All	Univ-intender	Non-univ-intender	College-intender	HS-only-intender
	(1)	(2)	(3)	(4)	(5)
Logit model with sampling weight					
Direct effect	-0.0791***	-0.0113	-0.100***	-0.0873***	-0.101***
	(0.0105)	(0.0156)	(0.0068)	(0.0120)	(0.0139)
pseudo R^2	0.043	0.035	0.032	0.035	0.031
Linear probability model with sampling weights					
Direct effect	-0.0785***	-0.0103	-0.109***	-0.0935***	-0.116***
	(0.0117)	(0.0154)	(0.0083)	(0.0125)	(0.0181)
R^2	0.056	0.047	0.040	0.045	0.036
Linear probability model without sampling weights					
Direct effect	-0.0520**	0.0411	-0.113***	-0.0927**	-0.102**
	(0.0207)	(0.0282)	(0.0258)	(0.0300)	(0.0351)
Conley-Taber 80% confidence interval	[-0.0958, -0.0000]	[-0.0124, 0.0697]	[-0.1704, -0.0266]	[-0.1234, -0.0289]	[-0.1948, 0.0236]
R^2	0.051	0.044	0.037	0.039	0.031
Number of provinces	10	10	10	10	10
Number of individuals	21,530	13,667	7,863	4,842	2,654

The marginal effect of logit model is reported with robust standard errors clustered at province level in parentheses. *p < 0.1, **p <0 .05, ***p < 0.01. See Appendix for estimates of other covariates of logit models with sampling weights.

Table 2.6: Indirect effect for pooled sample (cohort A and B)

	Basic	(1) + Demographics	(2) + Transition Status		
			All	Age 19-20	Age 20-21
	(1)	(2)	(3)	(4)	(5)
Logit model with sampling weight					
Indirect effect	-0.0573***	-0.0654***	-0.0645***	-0.0478***	-0.0340***
	(0.0116)	(0.0104)	(0.0117)	(0.0108)	(0.00838)
pseudo R^2	0.010	0.031	0.051	0.042	0.035
Linear probability model with sampling weights					
Indirect effect	-0.0613***	-0.0690***	-0.0678***	-0.0500***	-0.0355***
	(0.0116)	(0.0104)	(0.0111)	(0.0109)	(0.0085)
R^2	0.014	0.041	0.066	0.054	0.044
Linear probability model without sampling weights					
Indirect effect	-0.0817***	-0.0606***	-0.0659***	-0.0484***	-0.0425***
	(0.0081)	(0.0096)	(0.0089)	(0.0141)	(0.0099)
Conley-Taber 80% confidence interval	[-0.0953, -0.0456]	[-0.0772, -0.0140]	[-0.0925, -0.0456]	[-0.0847, -0.0216]	[-0.0790, 0.0049]
R^2	0.022	0.058	0.084	0.050	0.053
Number of provinces	10	10	10	10	10
Number of obs.	114,594	114,594	114,594	38,074	33,714

The marginal effect of logit model is reported with robust standard errors clustered at province level in parentheses. *p < 0.1, **p <0 .05, ***p < 0.01. See Appendix for estimates of other covariates of logit models with sampling weights

Table 2.7: Robustness check: attrition (cohort A and B)

	Pooled	Age 15	Age 17-18	Age 19	Age 21	Age 23
	(1)	(2)	(3)	(4)	(5)	(6)
Direct effect	-0.0116	0.0269***	0.0469***	-0.0481***	-0.0348	-0.0190
	(0.0152)	(0.0068)	(0.0155)	(0.0172)	(0.0261)	(0.0490)
pseudo R^2	0.056	0.040	0.053	0.054	0.043	0.052
Number of obs.	142,623	16,674	22,545	22,432	22,126	18,317

	Pooled	Age 19-20	Age 20-21
	(1)	(3)	(4)
Indirect effect	-0.0918***	-0.0442***	-0.0332***
	(0.0095)	(0.0097)	(0.0100)
pseudo R^2	0.052	0.043	0.038
Number of obs.	56,588	30,182	29,878

The marginal effect of logit model is reported with robust standard errors clustered at province level in parentheses.
*p < 0.1, **p <0 .05, ***p < 0.01.

Figure 2.1: Annual volunteer participation rate of the four groups

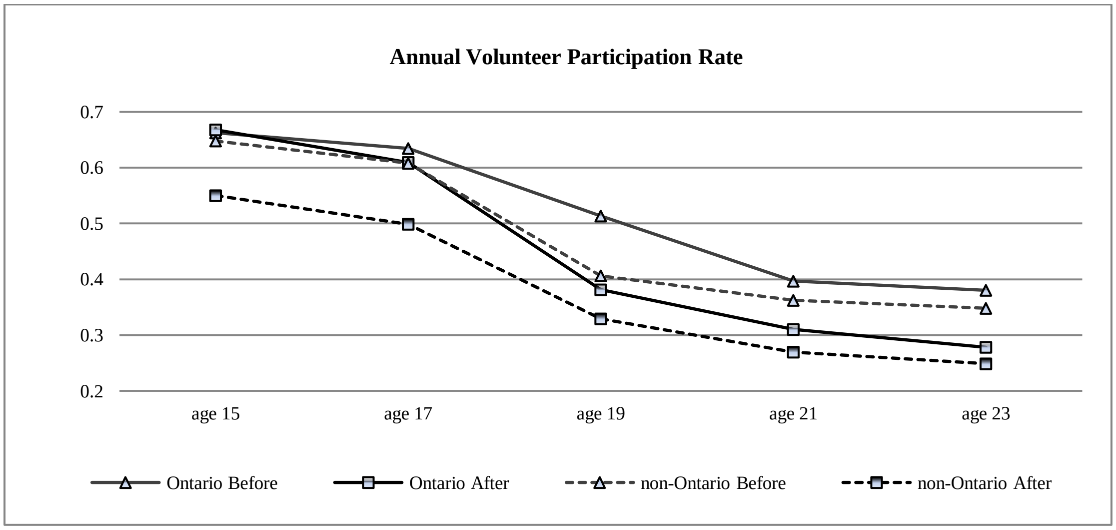
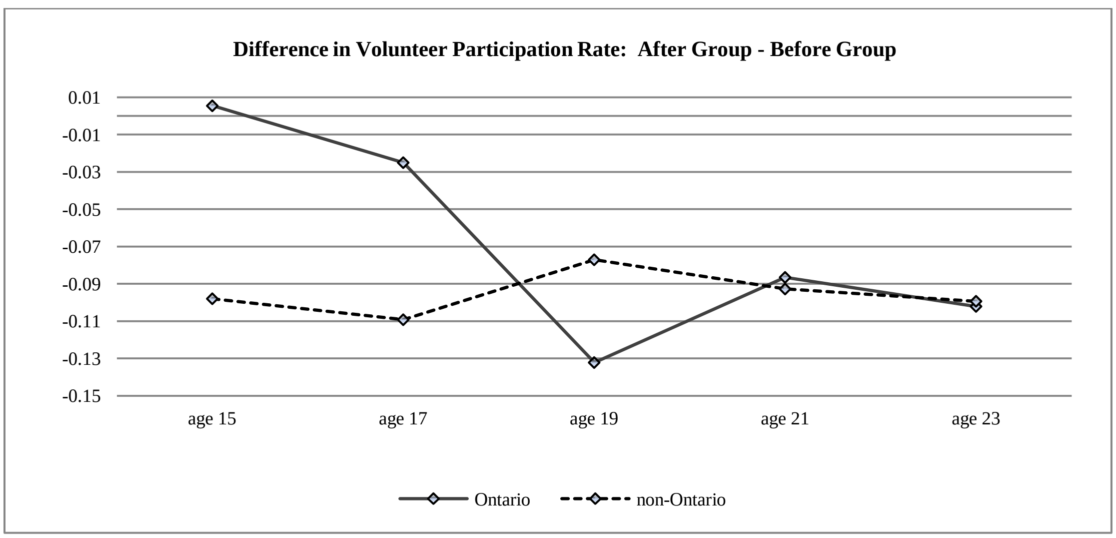


Figure 2.2: Difference between the after group and before group over time



APPENDIX:

Table 2.A1: Logit model with sampling weight for direct effect for pooled model

	Basic	(1) + Demographic	(2) + controls
	(1)	(2)	(3)
Direct effect	0.0070	-0.0017	-0.0068
	(0.0121)	(0.0105)	(0.0123)
Treatment flag	0.0921	0.0865	0.0648
	(0.0654)	(0.0717)	(0.0577)
After flag	-0.0084	-0.0501***	0.0126*
	(0.0127)	(0.0102)	(0.0074)
Male		-0.0907***	-0.0764***
		(0.0149)	(0.0173)
Citizen		0.0410**	0.0507**
		(0.0202)	(0.0213)
Age		-0.0117	-0.0193**
		(0.0080)	(0.0097)
Urban		-0.0140**	-0.0401***
		(0.0059)	(0.0069)
Cohort B		-0.0590*	-0.0196
		(0.0311)	(0.0313)
Married		-0.0106	0.0100
		(0.0099)	(0.0125)
Child flag		-0.1450***	-0.0952***
		(0.0251)	(0.0257)
Schooling			0.0277***
			(0.0038)
Private HS			0.0704***
			(0.0197)
HS student			0.1850***
			(0.0176)
PSE student			0.0700***
			(0.0088)
Graduating year			0.0529***
			(0.0075)
Double-cohort			0.0038
			(0.0050)
pseudo R^2	0.018	0.032	0.053
N	179,285	179,285	179,285

The marginal effect of logit model is reported with robust standard errors clustered at province level in parentheses.
*p < 0.1, **p <0 .05, ***p < 0.01.

Table 2.A2: Logit model with sampling weight for direct effect for five age groups

	Age 15	Age 17-18	Age 19	Age 21	Age 23
	(1)	(2)	(3)	(4)	(5)
Direct effect	0.0319***	0.0599***	-0.0519***	-0.0359	-0.0265
	(0.0048)	(0.0174)	(0.0070)	(0.0300)	(0.0518)
Treatment flag	0.0130	0.0149	0.1370	0.0300	0.0783
	(0.1310)	(0.1610)	(0.0911)	(0.0823)	(0.0554)
After flag	-0.0234***	0.0185	0.0190	0.0152*	0.0129
	(0.0055)	(0.0170)	(0.0245)	(0.0081)	(0.0192)
Male	-0.1380***	-0.1380***	-0.0634***	-0.0571***	-0.0688***
	(0.0111)	(0.0120)	(0.0150)	(0.0188)	(0.0179)
Citizen	0.0243***	0.1000**	0.1450***	0.1880***	-0.2460*
	(0.0065)	(0.0498)	(0.0161)	(0.0195)	(0.1390)
Urban	-0.0112	-0.0596***	-0.0265	-0.0363**	-0.0245**
	(0.0093)	(0.0226)	(0.0174)	(0.0181)	(0.0105)
Schooling	-0.0131***	0.0667***	0.0363	0.0261***	0.0331***
	(0.0012)	(0.0196)	(0.0246)	(0.0089)	(0.0067)
Private HS	0.1060***	0.0815**	0.0835***	0.0763***	0.0581***
	(0.0274)	(0.0362)	(0.0182)	(0.0175)	(0.0160)
HS student		0.0476	0.0025	0.0340	0.2490***
		(0.0298)	(0.0159)	(0.0565)	(0.0461)
PSE student		0.0380	0.1130***	0.117***	0.0796***
		(0.0383)	(0.0140)	(0.0062)	(0.0124)
Graduating year	-0.0248	-0.0203	0.1290***	0.0196	-0.0166
	(0.0217)	(0.0404)	(0.0272)	(0.0293)	(0.0529)
Double-cohort	-0.1700***	-0.0939***	-0.0262**	0.0246*	-0.0078
	(0.0024)	(0.0178)	(0.0116)	(0.0136)	(0.0135)
pseudo R^2	0.035	0.055	0.052	0.043	0.052
N	24,807	33,307	29,197	24,094	19,051

The marginal effect of logit model is reported with robust standard errors clustered at province level in parentheses.
*p < 0.1, **p <0 .05, ***p < 0.01.

Table 2.A3: Logit model with sampling weight for direct effect for five age groups

	All (1)	Univ-intender (2)	Non-univ-intender (3)	College-intender (4)	HS-only-intender (5)
Direct effect	-0.0791***	-0.0113	-0.100***	-0.0873***	-0.1010***
	(0.0105)	(0.0156)	(0.0068)	(0.0120)	(0.0139)
Treatment flag	0.2040**	0.0200	0.2530	0.6630***	0.997***
	(0.0859)	(0.0991)	(0.1790)	(0.0878)	(0.0006)
After flag	0.0445***	0.0727***	0.0225*	0.0013	0.0275
	(0.0073)	(0.0079)	(0.0119)	(0.0140)	(0.0316)
Male	-0.0458***	-0.0354***	-0.0512***	-0.0436***	-0.0506*
	(0.0066)	(0.0065)	(0.0133)	(0.0054)	(0.0289)
Citizen	0.0349	0.0303	0.0700	-0.105	
	(0.0542)	(0.0487)	(0.0630)	(0.216)	
Married	-0.0193	-0.0042	-0.0274	0.0159	-0.1070***
	(0.0210)	(0.0254)	(0.0285)	(0.0470)	(0.0226)
Child flag	-0.0542*	-0.0972**	-0.0185	-0.0346	0.0592
	(0.0298)	(0.0487)	(0.0211)	(0.0342)	(0.0416)
Urban	-0.0394**	-0.0460*	-0.0479***	-0.0742***	-0.0056
	(0.0157)	(0.0254)	(0.0081)	(0.0129)	(0.0210)
Schooling	0.0428***	0.0481***	0.0291***	0.0251***	0.0297
	(0.0124)	(0.0131)	(0.0097)	(0.0065)	(0.0212)
Private HS	0.0484*	0.0359	0.0739***	0.0751***	0.0791***
	(0.0257)	(0.0323)	(0.0247)	(0.0219)	(0.0285)
HS student	0.0124	0.0093	0.0174	0.0022	0.0483
	(0.0315)	(0.0280)	(0.0421)	(0.0366)	(0.0553)
PSE student	0.1240***	0.1160***	0.0909***	0.0952***	0.0578***
	(0.0112)	(0.0163)	(0.0076)	(0.0106)	(0.0205)
Graduating year	0.0851***	0.0974***	0.0552**	0.0450**	0.0536*
	(0.0286)	(0.0275)	(0.0242)	(0.0213)	(0.0326)
Double-cohort	-0.0493***	-0.0164	-0.115***	-0.298***	-0.0755***
	(0.0176)	(0.0183)	(0.0197)	(0.0633)	(0.0242)
pseudo R^2	0.043	0.035	0.032	0.035	0.031
N	21,530	13,667	7,863	4,842	2,654

The marginal effect of logit model is reported with robust standard errors clustered at province level in parentheses. *p < 0.1, **p <0 .05, ***p < 0.01.

Table 2.A4: Logit model with sampling weight for indirect effect

		(1) +	(2) + Transition Status		
	Basic	Demographic	All	Age 19-20	Age 20-21
	(1)	(2)	(3)	(4)	(5)
Indirect effect	-0.0573***	-0.0653***	-0.0645***	-0.0478***	-0.0340***
	(0.0116)	(0.0104)	(0.0117)	(0.0108)	(0.0084)
Treatment	0.0313***	0.0402***	0.0231**	0.0065	-0.0228***
	(0.0085)	(0.0075)	(0.0096)	(0.0072)	(0.0064)
After1999	-0.0793***	0.0166	0.0776**	0.0002	-0.0033
	(0.0132)	(0.0111)	(0.0384)	(0.0054)	(0.0083)
Male		-0.0940***	-0.0732***	-0.0618***	-0.0578***
		(0.0164)	(0.0171)	(0.0165)	(0.0193)
Citizen		0.0854***	0.0842***	0.1080***	0.0991***
		(0.0196)	(0.0241)	(0.0213)	(0.0152)
Urban		-0.0183***	-0.0397***	-0.0356***	-0.0439***
		(0.0064)	(0.0074)	(0.0103)	(0.0121)
Cohort B		-0.0215***	0.0221	0.0426***	0.0124
		(0.0056)	(0.0179)	(0.0107)	(0.0096)
Married		-0.0448***	-0.0118*	-0.0142	-0.0006
		(0.0057)	(0.0062)	(0.0099)	(0.0161)
Child flag		-0.1480***	-0.0834***	-0.0971***	-0.0972***
		(0.0221)	(0.0200)	(0.0106)	(0.0136)
Schooling			0.0393***	0.0380**	0.0321***
			(0.0079)	(0.0179)	(0.0103)
Private HS			0.0632***	0.0516**	0.0474**
			(0.0217)	(0.0217)	(0.0207)
HS student			0.1310***	0.0081	0.0565
			(0.0084)	(0.0136)	(0.0413)
PSE student			0.0762***	0.0995***	0.0996***
			(0.0088)	(0.0121)	(0.0100)
Graduating year			0.0588***	0.1090***	0.0307***
			(0.0069)	(0.0245)	(0.0080)
Double-cohort			0.0140	0.0343*	0.0256*
			(0.0099)	(0.0182)	(0.0142)
pseudo R^2	0.010	0.030	0.051	0.042	0.035
N	114,594	114,594	114,594	38,074	33,714

The marginal effect of logit model is reported with robust standard errors clustered at province level in parentheses.
*p < 0.1, **p <0 .05, ***p < 0.01.

CHAPTER 3
ARE CONTRIBUTIONS OF TIME AND MONEY SUBSTITUTES OR COMPLEMENTS?

3.1 INTRODUCTION

Volunteer activity is work provided without being motivated by financial gain that generates social output. For Canada in 2004, 11.8 million individuals volunteered almost two billion hours to charities and other nonprofit organizations, which is equivalent to one million full-time jobs, around 5% of all jobs in the economy. The total value of volunteer hours, even by a modest calculation, exceeds the $8.9 billion of monetary donations estimated by the survey for the same period.

This paper aims to study the relationship between contributions of time and money first in a theoretical model and then by exploiting quasi-experiments induced by a series of tax policy changes in Canada, especially the 1988 tax reform. In the reform, the tax assistance to charitable donations was converted from a tax deduction to a non-refundable tax credit, which introduced significant change in the tax price of charitable donations.[12] The advantages of exploiting the tax price variation introduced by the tax reforms are twofold. Firstly, it is exogenous to individuals'

[12] The tax price of charitable donation is defined as the after tax cost of a one-dollar donation for a donor.

unobserved heterogeneity such as in the preference for public goods and in productivity in the labour market and volunteer activities. Second, it is independent of other tax incentives, e.g. the marginal tax rate, so as to avoid the confounding of other time use decisions, e.g. paid work hours. We empirically explore how individuals adjust their volunteer behaviour in response to changes in the tax price of charitable donations. While previous studies have evaluated how tax reforms influence private contributions, no study has done it for the conversion of a tax deduction system to a tax credit system. The evidence in this paper supports the hypothesis that donations of time and money are substitutes rather than complements. It is in contrast to most previous empirical evidence, which generally points to a complementary relationship.

We further develop a "warm-glow" type model in attempt to reconcile these seemingly contradictory empirical findings. In the model, individuals care about the total contribution they make for public goods through gifts of time and money. Heterogeneity in wages, preferences, and marginal productivities of volunteer hours[13] is included in the model. The model is tested by exploiting a series of tax policy changes in Canada, especially the effect of the tax price of monetary donations on volunteer behaviour. The empirical results show that individuals increased their donations of time on both the extensive and intensive margins as the tax price of monetary donations increased, which indicates donations of time and money were gross substitutes rather than complements. The cross-price elasticity of the annual volunteer participation is estimated to be 0.44. As most current policies that encourage voluntary public good provision are aimed at monetary donations, e.g. the tax credit on charitable donations, one implication is the effectiveness of the tax preference policy on charitable behaviour may have

[13] Fellner et al. (2011) find that individuals with high marginal product contribute more than those with low marginal product when contributions can be linked to the type of the donor.

been overstated, as the increase in monetary contribution may have been offset by a reduction in donations of time.

The remainder of this paper is organized as follows. Section 3.2 reviews the literature. Section 3.3 presents the theoretical model and its implications. The empirical issues and the identification strategy are summarized in Section 3.4. Section 3.5 describes the tax policy changes in Canada. In Section 3.6 and 3.7, we describe our data sources and outline the tax price simulation method. The empirical results are presented in Section 3.8. Section 3.9 checks the robustness of the results. Finally, Section 3.10 offers a brief conclusion and discussion.

3.2 LITERATURE REVIEW

Research on voluntary contributions of money and time dates back to the 1980s. Early studies, e.g. Menchik and Weisbrod (1988), Brown and Lankford (1992) and Vaillancourt (1994), usually assume that both volunteer hours and charitable donations are private goods which can generate utility. Volunteer hours are considered to be a leisure-like good so that individuals equate the marginal utility from the last volunteer hour with the marginal gain from work or leisure. Under this assumption, the choice of donations of time and money is the same as choosing between two private goods and their relationship is revealed by the sign of the cross-price effect. Defining the prices of donations of time and money as the net wage rate and the tax price, respectively, these studies, except Vaillancourt (1994), explicitly take the interaction of donations of money and time into account and jointly estimate both choices. Menchik and Weisbrod (1988) find that volunteer hours are negatively correlated with the tax price of monetary donations, which suggests donations of time and money are complements.

Brown and Lankford (1992) further assume that the labour force participation, or the work hour decision, is determined prior to the decision on how many hours to volunteer, which makes the opportunity cost of a volunteer hour, or the shadow price of time budget constraint, deviate from the wage rate because individuals cannot substitute volunteer hours with work hours. Under this assumption, they control for total available hours instead of the net wage rate and find volunteer activity is negatively correlated with the tax price of monetary donations. They conclude that donations of time and money are complements based on this evidence.

Using the Current Population Survey 1989 (CPS), Freeman (1997) finds that while individuals with higher hourly earnings are more likely to increase donations of both time and money, the increment in monetary donations is larger than that in volunteer hours. It suggests individuals might substitute volunteer hours with monetary donations as the relative value of time increases. As well, he suggests that it might be inadequate to consider donations of time as a private good since "volunteering is a 'conscience good or activity'—something that people feel morally obligated to do when asked, but which they would just as soon let someone else do."(S140)

Duncan (1999) drops the private good assumption and adopts a pure public good model where donors give money and time as inputs to produce a public good. In his model, a donor cares only about the public good so, at equilibrium, she contributes an arbitrary combination of volunteer hours and donation dollars as long as the total value of the contribution, which is defined as donation dollars plus the product of volunteer hours and their marginal product, is at the optimal level. Duncan finds evidence that households decrease monetary contributions when local government expenditures increase, which is consistent with the public good model. However, unlike private good models, pure public good models such as Duncan's yield the strong hypothesis that donations of time and money are perfectly substitutable, which has not been supported by existing empirical studies.

Apinunmahakul et al. (2008) use Canadian data and define the net wage and tax price as the prices of the donation of time and money. They find the effects of the net wage and the tax price on volunteering hours are not well estimated but do estimate that the net wage negatively affects monetary donations, from which they conclude that donations of time and money are complements. Apinunmahakul and Devlin (2008) find that donations of both time and money are affected by a donor's social networks. Feldman (2010) uses a biennial national survey in the United States. Employing a model similar to that in Apinunmahakul et al. (2008), she finds that the volunteer dummy is negatively correlated with the tax price of charitable donations, which implies a negative cross price effect. After imposing the further assumption that utility over donations of time and money is quadratic, the estimated coefficients of the structural model imply that the marginal utility of monetary donations decreases as volunteer hours increase. She concludes donations of time and money are substitutes and that the estimated negative cross-price effect is observed because of behavioural effects such as an increase in the material incentive decreasing the intrinsic motivation to contribute, or donors being more likely asked to volunteer.

Overall, while the consumption model treats donations of time and money as two private goods and hence provides no prediction on their relationship, the public good model takes them as substitutable inputs in the production of public goods, which implies that they should be substitutes for a contributor.[14] Most empirical evidence has pointed to a complementary relationship between donations of time and money, which seems to favour a consumption model over a public good model.

[14] This argument also applies to the social-image models, as in Benabou and Tirole (2005), where individuals make donations of time and money to improve their social image rather than for their utility from the public good per se.

3.3 A THEORETICAL MODEL ON VOLUNTEER BEHAVIOUR

The studies cited above assume individuals obtain utility either from volunteer hours per se, from the public good or the value of the contribution made to produce the public good. All these motivations are incorporated into our model. Firstly, we assume volunteer activity is a special type of leisure so that it provides utility in the same manner as watching a movie. However, it simultaneously differs from regular leisure activity in that it provides special satisfaction. As in the Canadian Survey of Giving, Volunteering, and Participating (CSGVP) in 2004, many volunteers reported that they were motivated by using skills and experience (77%), or exploring their own strength (49%). Only a small percentage volunteered in the sector in which they had their paid jobs, which support the idea that volunteer activities are taken as a break from regular paid work.

Secondly, we also assume an individual volunteers in order to produce a public good so that she cares about the total dollar value of her contributions which is the sum of her monetary donations and the number of her volunteer hours multiplied by the productivity of the volunteer work. It is a natural extension of the "warm glow" model of monetary donations (Andreoni et al. 1996, Andreoni 1989, 1990). However, we replace the public good with the charitable good out of two considerations. The first comes from previous studies on monetary donations. Andreoni (1988) finds that in a pure public good model, the fraction of individuals who make monetary donations diminishes to zero as the size of the economy grows. Ribar and Wilhelm (2002) find that, theoretically, impure altruistic preference leads to either zero or complete crowd-out in a large economy. But their empirical evidence supports a zero crowd-out effect, which, they conclude, suggests that the altruistic motivation of monetary donations might be weak at the margin. Second, since our focus is on the relationship between donations of time and money, assuming an individual cares about the total value of her contribution generates the same prediction in this dimension as the pure public good model in Duncan (1999).[15] By combining these motivations, we can explicitly take into account both the charitable dimension and the time-consuming dimension of volunteer activities.

Assuming additive separability between the utility of the private good, the utility of charitable good, and the utility of the non-working hours, we assume the individual maximizes:

$$U_i = X + \delta_i F(C) + [K(l+v) + mL(l) + nN(v)] \qquad (3.1)$$

subject to budget constraints: $C = d + \alpha_i v$; $l + v + w = T$; $X + (1 - t_1)d = (1 - t_2)r_i w$, where X is the private good, C, is the charitable good defined as the sum of money donations, d, and the product of volunteer hours, v, and the productivity of the volunteer activity, α_i, that individual i chooses to do, r_i is the wage rate of her paid work, w is paid hours, l is leisure, t_1 is the tax benefit rate for charitable donations, and t_2 is the constant marginal tax rate on earnings.

Individuals are assumed heterogeneous in three dimensions, the preference for the charitable good, δ_i, the productivity of volunteer hours, α_i, and the wage rate, r_i. As in Andreoni (1996), an individual's marginal product of volunteer hours is assumed to be less than her wage rate for paid work, or $\alpha_i \leq r_i$. Finally, we assume individuals do not change the type of their volunteer work, i.e. fixed α_i, in response to tax price changes.

3.3.1 A COMPARATIVE STATIC ANALYSIS UNDER THE TAX DEDUCTION SCENARIO:

In most developed economies, charitable donations are deducted from taxable income so that the cost of one extra dollar of donations, in terms of consumption, is one minus the marginal tax rate (MRT). Setting the tax price $p = (1 - t)$, where $t = t_1 = t_2$, the monetary budget constraint

[15] See footnote 5. Our model is also consistent with the social-image model if the social image or reputation is defined as a positive monotonic function of the charitable good.

becomes: $X + pd = pr_i w$. Maximizing Eq. (3.1) over the monetary donation, d, volunteer hours, v, and working hours, w, we have the first order conditions:

$$\delta_i F' \tag{3.2}$$

$$\delta_i \alpha_i F' + nN' = mL' \tag{3.3}$$

$$pr_i = K' + mL' \tag{3.4}$$

Eq. (3.2) shows that individuals make monetary contributions until the marginal utility of the charitable good equals the tax price of charitable donations. In Eq. (3.3), the marginal utility of an extra volunteer hour has two components, 1) the marginal utility of the charitable good that is produced by volunteer activity, or $\delta_i \alpha_i F'$; and 2) the marginal utility directly acquired from the activity per se, or nN'. Eq. (3.4) reveals that the tax price, p, affects not only the monetary donation but also working hours. As it increases, individuals want to substitute monetary donations with volunteer hours. However, it simultaneously provides an incentive to allocate more time to paid work instead of volunteer and/or leisure. The aggregate effect of an increase in p is ambiguous with the two confounding effects in opposite directions.

The charitable good, volunteer work, and leisure are assumed to be "goods" and subject to diminishing marginal utility, i.e. $\frac{\partial U_i}{\partial C} = F' > 0$ and $\frac{\partial^2 U_i}{\partial C^2} = F'' < 0$; $\frac{\partial U_i}{\partial v} = \delta_i \alpha_i F' + K' + nN' > 0$ and $\frac{\partial^2 U_i}{\partial v^2} = \delta_i \alpha_i F'' + K'' + nN'' < 0$; $\frac{\partial U_i}{\partial l} = K' + mL' > 0$ and $\frac{\partial^2 U_i}{\partial l^2} = K'' + mL'' < 0$. It is worth mentioning that the relationship of volunteer hours and leisure is determined by $\frac{\partial^2 U_i}{\partial l \partial v} = K''$. If $K'' > 0$, volunteer hours and leisure are complements as more leisure increases the marginal utility of volunteer hours (and vice versa). Moreover, we assume that $N'' < 0$ and $L'' < 0$, which implies that when the total time spent on volunteer work and leisure is fixed (and hence working time is fixed), the more time spent on one activity the less marginal utility is derived from it.

Linearizing Eq. (3.2) to (3.4), we have:

$$\frac{dw}{dp} = \frac{-(r_i - \alpha_i)mL'' - r_i nN''}{A} \tag{3.5}$$

$$\frac{dv}{dp} = \frac{(r_i - \alpha_i)(K'' + mL'') - r_i K''}{A} \tag{3.6}$$

$$\frac{dl}{dp} = -\left(\frac{dw}{dp} + \frac{dv}{dp}\right) = \frac{\alpha_i(K'' + nN'') + (r_i - \alpha_i)nN''}{A} \tag{3.7}$$

where $A = (mL'' + K'')(nN'' + K'') - K''^2$. In Eq. (3.5), the sign of the change in working hours is determined by the denominator, A, since the numerator is positive given $N'' < 0$ and $L'' < 0$. We first assume that A is positive. This is because the effect on working hours of an increase in the tax price in Eq. (3.5) should have the same sign as the effect on working hours of a reduction in the marginal tax rate t, as $p = (1 - t)$. A reduction in the marginal tax rate t is equivalent to an increase in the after-tax wage and empirical studies on labour supply typically show that the substitution effect is larger than the income effect as wage increases result in individuals working slightly more, which would imply a positive A in this model.The case in which the income effect dominates ($A < 0$) is discussed later.

With $A > 0$, the effect of the tax price on volunteer hours is determined by the numerator in Eq. (3.6). If the productivity of volunteer work is zero, $\alpha_i = 0$, the model degenerates to a consumption model and the numerator to $r_i mL'' < 0$. Individuals will allocate less time to volunteer activity as the tax price increases, $\frac{dv}{dp} < 0$, because it also provides an incentive to paid work. In the case where $r_i \geq \alpha_i > 0$, this still holds given a sufficient condition that $K'' > 0$, or that volunteer work and leisure are complements. So an individual will decrease both volunteer hours and leisure as she decides to work more. The following inequality provides the sufficient and necessary condition for $dv/dp < 0$ when $A > 0$:

$$\left|\frac{mL''}{\alpha_i}\right| > \left|\frac{K''+mL''}{r_i}\right| \qquad (3.8)$$

As the tax price increases, and hence an individual increases her working hours, she would decrease her volunteering instead of her leisure given the condition. Eq. (3.8) also shows that the actual sign of dv/dp for an individual is determined not only by her preferences for volunteer time and leisure but also by the productivity of her volunteer work and her paid work. In the case where $A < 0$, an individual will work less as the tax price increases. Under some conditions including leisure and volunteer work being substitutes, it may be that $dv/dp < 0$.

3.3.2 A COMPARATIVE STATIC ANALYSIS UNDER THE TAX CREDIT SCENARIO:

In a credit system, charitable donations are deducted from the tax payable at a certain rate so that the tax price no longer coincides with the marginal tax rate. Donors in a credit system have a tax price of charitable donations that is independent of the level of earned income. Setting the tax price of charitable donations as one minus the tax credit rate, or $p = (1 - t_1)$, we maximize Eq. (3.1) subject to: $X + pd = (1 - t_2)r_i w$. Unlike the previous model, the tax price of charitable donations, p, is no longer determined by the marginal tax rate, $1 - t_2$. The comparative analysis provides the following equations:

$$\left.\frac{dv}{dp}\right|_c = \frac{-\alpha_i(K''+mL'')}{A} \qquad (3.9)$$

$$\left.\frac{dd}{dp}\right|_c = \frac{1}{\delta_i F''} + \frac{\alpha_i^2(K''+mL'')}{A} \qquad (3.10)$$

$$\left.\frac{dl}{dp}\right|_c = -\left(\frac{dw}{dp} + \frac{dv}{dp}\right) = \frac{\alpha_i K''}{A} \qquad (3.11)$$

where the subscript c indicates they are under a credit system. In the denominator, A takes the same form as in Eq. (3.5). It is positive if the substitution effect exceeds the income effect. However, in the credit case, the change in the tax credit rate does not affect the net wage so that the income effect should be trivial. Therefore $A > 0$.

Eq. (3.9) shows that an individual chooses to volunteer more when the tax price of charitable donations increases because $K'' + mL'' < 0$. This positive cross-price effect is consistent with the model assumption that donations of time and money are substitutes. It also suggests that the more productive is an individual's volunteer work, the higher is the cross-price effect. Eq. (3.10) provides the own-price effect on monetary donations. The first part of its RHS, $1/\delta_i F''$, shows that an individual will decrease her total contribution as the tax price increases. The more the individual prefers the charitable good (higher δ_i), the smaller is this effect. The second part shows donations of time and money are perfectly substitutable at the rate of the productivity of volunteer work. Eq. (3.11) suggests a simple time allocation rule in a tax credit system: as the tax price increases, an individual will consume more leisure if leisure and volunteer hours are complements (K'').

3.3.3 SUMMARY OF COMPARATIVE STATIC RESULTS

Our model assumes that the individual cares about her total contribution to public good production so that donations of time and money are substitutes. Nevertheless, our comparative static results suggest that an increase in the after-tax price for monetary donation may reduce time donation under the tax deduction scenario (subsection 3.3.1) but not under a tax credit scenario (subsection 3.3.2), because under the former a higher tax price of donations is the same as a reduction in tax rates. Lower tax rates can change working hours and hence change the amount of time available for leisure and volunteering. Depending upon the degree of substitutability/complementarity between leisure and volunteering, volunteering hours can fall. In any case, because the results are different under tax deduction and tax credit regimes, distinct identification strategies must be employed. In both cases, there is the additional issue of heterogeneity across individuals with different preferences for public good contributions and different productivity in public and private good production.

3.4 IDENTIFICATION STRATEGY

Most empirical studies have used observations under tax deduction systems, e.g. U.S data, and defined the prices of volunteer time as the net wage and that of monetary donations as one minus the marginal tax rate. Negative cross-price effects between donations of time and money are often found using cross-sectional survey data, which have been used to support the conclusion that they are complements. However, our theoretical model suggests these approaches are problematic for two reasons.

Firstly, when the identification depends on cross-sectional variation, it is vulnerable to problems arising from unobserved heterogeneity. As presented in the model, the volunteer behaviour is affected by the preference for the charitable good, δ_i, and the marginal product of the volunteer activities, α_i, which are unobservable. The tax price variation in a cross-section comes largely from the variation in the marginal tax rate. Because the income tax is progressive in most countries, the tax price is usually negatively associated with the wage and hence income under a deduction system. The negative correlation found between the tax price and the volunteer behaviour might simply result from a positive association of the preference for charitable goods with income. Even when the preference for charitable goods may have been taken into account by estimating equations for monetary donation and volunteering jointly, the problem of differential productivity remains. Individuals with high earned incomes might choose to volunteer more simply because they are more productive. In this case, a negative correlation between the donation tax price and volunteering may be falsely interpreted as a negative cross-price effect.

Secondly, while the working time is usually assumed fixed and excluded, it potentially confounds the relation of interest. As discussed in Section 3.3, both the price of monetary donations and the net wage are determined by the marginal tax rate under a tax deduction system. The estimated coefficient of the price of monetary donations may be confounded by the effect that individuals work more and volunteer less in response to a higher net wage. As shown in Eq. (3.8), this confounding may also interact with idiosyncratic effects such as the preference for leisure, the productivity of volunteer work and paid work.

To mitigate these problems, we conduct a reduced form test of the model using the variation in the tax price over time introduced by a series of tax reforms in Canada. The advantage is twofold. First, in the 1988 tax reform, Canada switched the tax benefit for charitable donations from being a deduction to a non-refundable credit, which dissolved the relationship between the tax price of charitable donations and the MRT. Secondly, these policy changes brought about variations that are plausibly exogenous to individuals' choices and to individuals' unobserved heterogeneity. Hence it is possible to disentangle the causal effect between the tax price and volunteer behaviour from the confounding effects.

Exploiting the tax policy changes, we employ two models to estimate the impact of the tax price on volunteer measures using individual level repeated cross-sectional data. The first model, or the pooled model, identifies the cross-price effect using both the cross-sectional and over-time variation in the tax price by estimating the equation:

$$v_{it} = a_0 + a_1 p_{it} + X_{it}\beta + \varepsilon_{it}$$

where v_{it} is the dependent variable that is defined as either a volunteer indicator or the amount of volunteer time by individual i in period t; p_{it} is the tax price of charitable donations; X_{it} is a vector of socio-economic controls; and ε_{it} is the error term. a_1 is the key coefficient that stands for the cross-price effect of monetary donations to time donations.

The second model, or the fixed effect model, uses the over-time tax price change only to identify the cross-price effect by estimating the equation:

$$v_{it} = b_0 + b_1(p_{it} - p_{it_0}) + b_2 p_{it_0} + X_{it}\beta + e_{it}$$

where $p_{it_0}$is the counterfactual tax price that individual i would face in the reference year before the tax reform. For observations for the reference year, it equals p_{it}; e_{it} is the error term. b_1 is the key coefficient for the cross-price effect. It estimates how the over-time tax price change caused by tax reforms affects volunteer measures conditional on the tax price before the reforms.

The volunteer behaviour and the tax price of charitable donations are investigated on the individual level rather than the household level in this study. This approach is adopted because 1) volunteer behaviour is observed only on the individual level, 2) the Canadian personal income tax system is in general based on individual income rather than on household income, and 3) the spouse's income is not observed in the available data. However, an individual's volunteer behaviour might be dominated by other household members, e.g. the spouse, or determined by a bargaining process within the household. Andreoni et al. (2003) and Yoruk (2010) have found that the charitable donations made by husband-deciding couples are different from those by either the wife-deciding or mutual-bargaining couples. We conduct a robustness check on a sub-sample consisting of only single-member households (neither married nor in the common-law relationship) in Section 3.9.

3.5 TAX POLICY CHANGES IN CANADA

Two major tax policy changes are exploited to acquire exogenous changes in the tax price of charitable donations. The first is the 1988 tax reform, where the tax benefit of charitable donations was switched to a tax credit. The second occurred in 2000 and 2001, when the base of provincial income tax was switched from the basic federal tax to the federal taxable income.

3.5.1 THE 1988 TAX REFORM

The tax reform in 1988 is known as a tax flattening in Canada and included significant changes in the progressive schedule of income tax rates. While the statutory tax rates were restructured substantially, the definition of the taxable income was also redefined due to the conversion of numerous tax deductions to tax credits e.g. personal exemptions and payroll tax deductions. As a consequence, the statutory tax rates for different incomes are not directly comparable pre- and post-reform. Maslove (1989) studies the marginal tax rates in 1984 and 1988 and finds that 1) over 95% of the families in the bottom five income deciles experienced increases in their marginal tax rates and 5% of these families faced increases of about 20 percentage points or more;[16] 2) More than 50% of the families in the top income decile experienced declines in their marginal tax rates.

While the reform had a significant impact on the marginal tax rate schedule, the change in the tax price of charitable donations was even more substantial. In the tax deduction scenario before the reform, charitable donations could be used to reduce taxable incomes so that the tax price of a one-dollar charitable donation was one minus the MRT for a taxable donor and one for a non-taxable donor. After the conversion to the tax credit, there are three cases. First, for non-taxable donors, because the tax credit is non-refundable, the tax price is one. Second, for a

[16] Besides the change in the taxable income definition, he concludes that the abolition of the standard employment expense deduction also played an important role for the increase in the marginal tax rate for those at the low incomes.

taxable donor who claims donations with a total annual amount less than or equal to $250,[17] the tax price is one minus the statutory tax rate of the lowest income bracket. Third, for a taxable donor who donates more than a total of $250 in a year, the tax price of the last dollar donation is one minus the statutory tax rate of the highest income bracket. All provinces except Quebec automatically adopted provincial credit systems similar to the new federal credit system in 1988. Quebec collects its own provincial tax and stuck to a provincial deduction for charitable donations until 1993 so that Quebec residents had both a federal tax credit and a provincial tax deduction between 1988 and 1993. As a rough approximation, the provincial tax takes about 50% of the federal tax in Canada. Consider an example of an individual outside of Quebec whose taxable income falls into the highest bracket both before and after the reform, the first-dollar tax price was $0.49 = $1 - 34\% * 1.5$ pre-reform, and changed to around $0.75 = $1 - 17\% * 1.5$ post-reform, where 17% is the tax credit rate for the first dollar of donations.

3.5.2 The switch from tax-on-tax to tax-on-income

The switch from the tax-on-tax to the tax-on-income in 2000 affected the tax price of charitable donations through the provincial tax credits. Under the tax-on-tax method, the provincial personal income tax was calculated as a certain percentage of the basic federal tax. In 1998, the provincial percentage was, for example, 42.8% in Ontario, which implies the first-dollar tax price was $0.757. After the switch to tax-on-income, provinces and territories are able to establish income tax brackets and rates independently of the federal income tax brackets and rates. As a result, the formula of the first-dollar tax price became $taxprice = 1 - 17\% -$

[17] The threshold was decreased to $200 in 1994. Spouses can combine their donations to avoid facing the threshold twice.

$provincial\_credit\_rate$. For example in 2001, the tax price increased by 1.5% to $0.768 in Ontario.

3.6 DATA

The General Social Survey (GSS) is used, which is a national representative survey conducted annually on various topics regarding the living conditions and well-being of Canadians. For all cycles used in this study, the population aged 15 and older has been sampled. The annual sample size was approximately 10,000 persons until 1998 and increased in 1999 to around 25,000. Sampling weights are available and have been employed in both descriptive analysis and regressions to make the data representative of the population.

Three volunteer behaviour measures are used in the empirical study: (1) minutes of volunteer work the respondent did in the day previous to the interview, (2) the daily volunteer indicator derived from the first measure, (3) the annual volunteer indicator for whether a respondent did any volunteer work in the last 12 months.[18] The specific questions vary slightly across cycles. The first three rows of Table 3.1 present the average of these volunteer measures in different cycles. In 1998 and 2005 both sets of measures are observed. The first row shows that the daily volunteer measure increased on the intensive margin (from 130 minutes to 170 minutes for those who did volunteer from 1998 to 2005) but the second row shows there as a decrease on the extensive margin (the daily volunteer participation rate changed from 1.8% to 1.5%). Taking both margins into account, average volunteer time increased by 9% from 1998 to

[18] There are other datasets available with volunteer measures around 2000, e.g. the National Survey of Giving, Volunteering, and Participating (NSGVP) in 1997 and 2000, and the Canadian Survey of Giving, Volunteering, and Participating (CSGVP) in 2004 and 2007. However, we use GSS instead of these datasets because 1) GSS has more before-policy-change periods, which can be used to control the trend; and 2) GSS has both daily and annual volunteer measures in 1998 and 2005 for comparison purposes.

2005, which is consistent with the change measured by the annual volunteer participation rate in the third row.

The tax price of charitable donations is the key independent variable and will be discussed in the next section. In addition, a set of socio-economic variables is controlled for in regressions. The net weekly wage is included to control for the opportunity cost of leisure time. It is derived as: $net\ wage = (1 - marginal\ tax\ rate) * \ personal\ income\ /\ working\ weeks$. Because it is based on working weeks, this measure likely underestimates the cost for those who work less hours in a working week, e.g. part-time workers. We also add household income to control for the income and/or wealth effect. It is derived from the 9- or 12-category household income variable in the GSS by using the middle value of each income category (zero for the lowest category and 1.3 times the lower bound for the highest category) and adjusting to 1992 dollars with the consumer price index. Compared to the average household income series reported by Statistics Canada,[19] they appear to be underestimated but demonstrate the same pattern of changes over time, e.g. the recession in the early 1990s and the recovery afterward. Along with those of the other variables, the averages of the derived household income are reported in Table 3.1.

Independent variables also include age, age squared, two indicators for younger than 25 and older than 65, a high school graduate indicator, a bachelor degree indicator, a male indicator, marital status, an immigrant indicator, a child(ren) indicator, an urban residence indicator, and province and/or census metropolitan area (CMA).[20] Religious affiliation and attendance variables (four religion indicators and four attendance indicators) are also incorporated in some

[19] Statistics Canada, Income in Canada, Category No. 75-202.
[20] A census metropolitan area (CMA) is a grouping of census subdivisions comprising a urban area, which must register an urban core population of at least 100,000 at the previous census.

specifications because churches are considered as charitable organizations; religion and church attendance are likely to have an association with an individual's volunteer choices. The daily volunteer measures regressions also include interview day and month indicators.

3.7 TAX PRICE SIMULATION

Because the personal income tax system in Canada is generally individual-based, we compute the tax price at the individual level without taking into account the individual's spousal information. Theoretically, we want to measure the actual monetary cost of the last dollar of charitable donations, or the last-dollar tax price, when is relevant to the volunteer decision. However, we use the first-dollar tax price instead for two reasons. Firstly, the last-dollar tax price is also determined by the annual amount of monetary donations an individual chooses to make so that it is endogenous. A large amount of donations may push the donor's income taxable into a lower tax bracket under a tax deduction system. Under the two-tier credit system in Canada, donors would have a higher credit rate as the total annual donation exceeds the threshold. Secondly, the actual amount of monetary donations is not observed. Hence, following Clotfelter (1985), we use the first-dollar tax price in this study.

The first-dollar tax price is computed in two steps. In the first step, the total tax and the after-tax income are simulated for each individual under two scenarios, i.e. one when each individual makes a zero charitable donation and the other when each individual makes a \$100 charitable donation. Then in the second step, we compare each individual's after-tax income under these two scenarios and calculate the tax price as:

$$taxprice = (\$100 - \Delta after\_tax\_income)/\$100$$

where the numerator gives the decrease in the income available for other purposes after tax income after the $100 donation is made. The Canadian Tax and Credit Simulator (CTaCS)[21] is used to simulate the after-tax income in the first step. Seven variables, i.e. the amount of donations, tax year, province of residence, gender, age, marital status, and personal earned income, plus four variables concerning the age of the individual's dependent children, are used as inputs to simulate the total tax payable. Since the simulation takes into consideration differences in basic tax rates across tax brackets, in provincial tax rates, and in personal exemptions across provinces, the computed tax prices have substantial variation in all cycles.

Following the same procedure, we also simulate the counterfactual tax price in 1986 and 1998, which are the reference years before the 1988 tax reform and the 2000 tax reform respectively. Because the two-tier tax credit for charitable donations provides a higher credit rate for the monetary donations exceeding $250 ($200 after 1994), we also simulate the tax prices assuming each individual makes annual contributions of $260, $500, and $1,000. Regressions using these alternative tax prices are estimated as a robustness check and the results are not sensitive to the tax prices under these alternative assumptions.

We also simulate the MRT by computing the incremental tax if the individual had an extra $100 income. The simulated MRTs may be underestimated for individuals with very low incomes, who usually have high effective marginal tax rates because of social assistance taxbacks, which are not included in our tax simulation. However, this is unlikely to influence the analysis because 1) in the deduction scenario, most of those individuals had zero taxable income so that their tax prices should be one no matter their marginal tax rate; 2) in the credit scenario, the tax price is no longer a function of the marginal tax rate.

[21] The CTaCS is a package that simulates the Canadian personal income tax and transfer system. It was developed by Kevin Milligan at the University of British Columbia.

3.8 RESULTS

3.8.1 THE TAX PRICE AND TAX REFORMS

Figure 3.1 shows how the tax prices and the marginal tax rates changed for individuals with different personal incomes in the 1988 tax reform. While the tax price for the very low income individuals who did not have tax liabilities remained at one before and after the reform, it was flattened for those with positive taxable incomes as it is no longer determined by the marginal tax rate. For most people with incomes higher than $30,000, the tax price effectively increased. Figure 3.2 provides the tax price over personal incomes for the four most populous provinces before and after the 1998 reform. The tax price varied both across provinces and over time, though it was relatively small in size.

We decompose the tax price variation into the cross-sectional and over-time components in Table 3.2. For the 1988 tax reform, by comparing columns (1) and (2), it can be seen that the cross-sectional variation decreased after the reform as also seen in Figure 3.1. Moreover, the correlation coefficient with the MRT variation shows the correlation between the tax price and the marginal tax decreased substantially as a result of the reform. Columns (3) and (4) show that, for the 1988 reform, the over-time variation is sizable and, more importantly, only 10% of it can be explained by the variation in the MRT as shown by the correlation coefficient.

The second panel of Table 3.2 presents the variation around the 2000 tax reform. The correlation between the tax price and the MRT increased in cross-sections after the reform. The over-time variation is small comparing to that in the cross-sections.

3.8.2 THE TAX PRICE AND VOLUNTEER BEHAVIOUR

Our expectation is that those individuals who had the tax price of monetary donations increase in the 1988 tax reform (in practice, largely individuals with high incomes), would decrease monetary donations and make more time donations because donations of time and money are posited to be substitutes. Individuals who had a lower tax price (largely individuals with lower incomes) would increase monetary donations and decrease time donations. Using the GSS, we plot the average daily volunteer participation rate and the average tax price of monetary donations over the average real after-tax income for ten income deciles for both the pre- (1986) and post-reform (1992) periods in Figure 3.3.

Panel B shows that the average tax price was higher in 1992 than it was in 1986 for the groups with higher after-tax incomes and lower in 1992 than in 1986 for most groups with lower after-tax incomes. Along with Panel B, Panel A shows that higher income groups who had the biggest increase in the tax price increased their volunteer participation and that lower income groups who had a reduction in their tax price reduced their volunteer participation. This is consistent with our model. While the correspondence is imperfect, particularly for those with middle incomes, the graph foreshadows the findings later in this session from the regression analysis which allows for the control of other influences.

While the focus of this study is volunteer behaviour, we include a graphical check of whether the change in monetary donations is consistent with our model. Unfortunately, the GSS cycles around the 1988 tax reform do not have measures of monetary donations. The only available measures in public-use data are from the aggregate tax data. Figure 3.4 provides the average ratio of the amount of donations to after-tax income for 1986 and 1992 using the same horizontal real after-tax income axis as Figure 3.3.[22] While Figure 3.4 shows that the donation-to-income ratio increased for almost all the groups, the relative increase is larger for lower income groups who experienced a reduction in their tax price and smaller for higher income groups whose tax price increased. Hence taking into account the limitations of the aggregate monetary donation data available and that these graphs cannot control for confounding influences, Figures 3.3 and 3.4 appear not to be too greatly at variance with the proposition that donations of time and money are substitutes.

Two sets of regressions are implemented on the volunteer behaviour. The first set uses as its dependent variables daily volunteer activity measures, i.e. the daily volunteer minute and the daily volunteer indicator. The key identification is based on the 1988 tax reform. Tables 3.3 and 3.4 provide the results of this set of regressions. The other set is on the annual volunteer indicator. The tax price variation comes from the change in the 2000 reform. These estimates are reported in Table 3.5.

Table 3.3 presents the estimates of the daily volunteer participation regression using a Probit model[23], where columns (1) to (3) is based on the pooled model and columns (4) to (6) are based on the fixed effect model. Besides the tax price variables, the coefficients of the net weekly wage ($netwage$) and the logged household income ($loghhinc$) are also reported. The results provide the impact on the volunteer choice at the extensive margin. Comparing the coefficient of the tax price in column (1), which is based on a cross-section under a deduction system, to column (2), which is also based on a cross-section but under a credit system, the

[22] Figure 4 is based on Table 2 in "Tax Statistics on Individuals", published by Revenue Canada in Ottawa, 1988, and 1994, which provides the number of tax filers, the amount of total income, the amount of after-tax income, and the amount of charitable donations for 21 income categories. In order to make Figure 4 comparable to Figure 3, we first re-group the 21 income categories into ten income deciles, then calculate the average after-tax income and the ratio of the amount of donations to after-tax income for each decile.

[23] Estimates using linear probability model are very similar.

estimated cross-price effect is much more significant in a credit system. Pooling all four years together (1986, 1992, 1998, and 2005), the estimate shows a ten percentage point increase in the tax price increases the probability that an individual does some volunteer work in a given day by 0.16 percent, which is equivalent to a cross-price elasticity of 0.956.

Columns (4) to (6) report the estimates of the fixed effect model, which focus on how the tax price change caused by the tax reform affects volunteer behaviour conditional on the counterfactual tax price before the reform (1986). Compared to the tax price coefficient in column (3), column (4) provides a larger and statistically significant effect, which is equivalent to a cross-price elasticity of 1.385. In columns (5) and (6), religion controls and CMA fixed are added to the model and the result remains almost unchanged.

In Table 3.3, the effect of the net weekly wage on the daily volunteer participation is statistically insignificant but of the right sign except in column (2). The effect of household incomes is statistically significant when multiple cycles are used. The estimates show a 10% increase in the household income would lead to about a 0.2% increase in the probability of daily volunteer participation.

The tax price effect on the intensive margin is estimated using a Tobit model. Table 3.4 presents the marginal effect on the expected daily volunteer minutes for volunteers, or $E(y|y > 0)$. The pattern in Table 3.4 is the same as in Table 3.3. Taking the estimate in column (4) for example, a ten percentage point increase in the tax price increases the average daily volunteer minutes of a volunteer by about three minutes, which is about 0.143 in terms of elasticity. Comparing the first two panels in Table 3.6, we can see that the cross-price elasticity estimated on the extensive margin is about ten times more than that on the intensive margin. However, positive and statistically significant cross-price effects are found between donations of time and the tax price on both margins.

Table 3.5 presents the estimates of the annual volunteer participation regression using a Probit model, with columns (1) to (4) based on the pooled model and columns (5) to (6) on the fixed effect model. As the over-time variation around the 2000 reform is small compared to the cross-sectional one, the fixed effect model in columns (5) and (6) does not provide an accurate estimate of the tax price effect. While the estimate is negative, it is very small and statistically insignificant. However, the pooled model gives estimates consistent with those of the daily volunteer regressions. The estimate in column (3) shows that the annual volunteer participation increases by 1.6% as the tax price increases by ten percentage points. In terms of elasticity, it is 0.441, which is about one half of the estimate from the daily volunteer indicator regression using the pooled model (column (3) of Table 3.3).

The estimates from both set of regressions indicate that the tax price of monetary donations has a positive effect on individuals' time donations. Individuals appear to choose to donate more time to make their contributions as the tax price increases.

In most developed countries, governments provide tax benefits for monetary donations in order to encourage private contributions to public goods. The efficacy and efficiency of these policies depend crucially on how individuals adjust their contributions in response to these tax incentives. Previous studies on the effect of these policies usually focused on the impact on monetary donations. Based on U.S. data, while Randolph (1995) estimates the permanent price elasticity of monetary donations as 0.51, Auten, Sieg, and Clotfelter (2002) find it is between 0.79 and 1.26. The results using Canadian data are mixed, with 0.15 in Glenday, Gupta, Pawlak (1986) and 1.07 in Kitchen (1990, 1992). If the relationship between donations of time and money are complementary as previous studies have concluded, tax incentives on monetary donations would encourage volunteer hours as a bonus. However, if as we conclude, donations of time and money are actually substitutes, the increase in monetary donations stimulated by tax incentives has to be offset by the decrease in donations of time.

3.9 Robustness checks

As mentioned in Section 3.4, both the volunteer behaviour and the tax price of charitable donation may be influenced by the spouse. In order to investigate how this issue affects the empirical results, we re-estimate all the empirical models using only single individual households. The sub-sample is around 40% of the total sample. The resulting estimates are consistent with the those using the whole sample.

Another concern is the first-dollar price used in the study as it overestimates the tax price for those who make large monetary donations. As mentioned as a robustness check, we have simulated tax prices with annual donations of \$260, \$500, and \$1000. Estimates based on these alternative tax prices are consistent as well. All these results are available upon request.

3.10 Conclusion and discussions

This paper analyzes the relationship between donations of time and money from both theoretical and empirical perspectives. We develop a model where individuals choose monetary donations, volunteer hours, and working hours simultaneously. As one of our contributions, our model permits volunteers to differ in their preference for the charitable good and in their marginal product across individuals. The model shows that this heterogeneity affects individuals' charitable behaviour. A correlation of volunteer behaviour with the tax price of a charitable donation in a cross-section may just reflect the association of the labour market ability with either the preference for the charitable good or the productivity of volunteer hours.

In order to empirically investigate the effect of the tax price of charitable donations on volunteer behaviour, we use data from Canada where the tax benefit for monetary donations has become a tax credit rather than a tax deduction. This avoids the coincidence of the tax prices of donations and the marginal tax rate and provides tax price variation that is exogenous to individual-specific effects. Exploiting a series of tax policy changes in Canada, we find that individuals increase their volunteer behaviour on both the extensive and intensive margins as the tax price of charitable donations increases. The cross-price elasticity of daily volunteer participation is estimated to be 1.385 while that of the annual volunteer participation is estimated to be 0.441. Our empirical results suggest that donations of time and money are substitutes rather than complements. Hence while tax deductions or tax credits increase monetary donations, individuals also decrease their volunteer time in response.

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Table 3.1: Means of selected General Social Survey variables for selected survey years[1]

	1986	1992	1998	2005
Daily volunteer indicator	0.016	0.011	0.018	0.015
Daily volunteer minutes[2]	209.6	144.9	130	170.6
Annual volunteer indicator	N.A.	N.A.	0.339	0.357
Male	0.490	0.489	0.492	0.493
Age	40.9	43.1	43.4	44.1
High school diploma	0.609	0.694	0.722	0.799
PSE diploma	0.284	0.369	0.384	0.474
Immigrant	0.183	0.187	0.198	0.194
Marriage	0.633	0.63	0.615	0.615
Child(ren)	0.401	0.413	0.393	0.366
Household income[3]	44.181	44.407	50.683	52.774
Urban	0.775	0.775	0.795	0.805
After-tax weekly wage[3]	0.235	0.253	0.237	0.301
Household size	2.924	3.028	2.961	2.901
No. of Observation[4]	16,390[5]	9,815	10,749	19,597

[1] Sampling weights are employed. [2] Average volunteer minutes for those who volunteered. [3] Adjusted to 2002 dollars using the consumer price index and reported in $1000. [4] Unweighted number of observations. [5] 9946 observations from the core component are used to calculate the means for the daily volunteer indicator and daily volunteer minutes.

Table 3.2: Tax price variation decomposition

	cross-sectional variation		over-time variation	
	Δtp_i before[1]	Δtp_i after[2]	$\Delta tp_{it} = p_{it} - p_{it_0}$[3]	$\Delta tp_{it}/p_{it_0}$
	(1)	(2)	(3)	(4)
1988 Reform				
mean	-0.003	0.0086	0.0547	0.0795
SD	0.1677	0.1109	0.0920	0.3216
(P10, P90)	(-0.1906, 0.1882)	(-0.0818, 0.1912)	(-0.0056, 0.1683)	(-0.0071, 0.2708)
ρ(Δtp, ΔMRT)[4]	-0.5091	-0.2619	-0.0998	N.A.
2000 Reform				
mean	0.0068	0.0150	0.0206	0.0275
SD	0.1197	0.1287	0.0810	0.1107
(P10, P90)	(-0.0897, 0.1807)	(-0.0762, 0.1931)	(0.0000, 0.0479)	(0.0000, 0.0646)
ρ(Δtp, ΔMRT)	-0.1966	-0.2848	-0.2267	N.A.

[1] Including year 1986 only for the 1988 reform; year 1989, 1990, and 1998 for the 2000 reform. [2] Including year 1992, 1998, and 2005 for the 1988 reform; year 2000, 2003, and 2005 for the 2000 reform. [3] The tax price before the reform (tp_{before}) is the tax price in 1986 for the 1988 reform and the tax price in 1998 for the 2000 reform. [4] The correlation coefficient between the variation in the tax price with the corresponding variation in the marginal tax rate for individuals who had positive taxable incomes.

Table 3.3: Probit estimates of daily volunteer indicator

	1986 only	1998 only	1986-2005	1986-2005	(4) + religion controls	(4) + CMA fixed effect
	(1)	(2)	(3)	(4)	(5)	(6)
p_{it}	0.011	0.040**	0.016*			
	(0.019)	(0.018)	(0.008)			
Δtp_{it}				0.025**	0.024**	0.025**
				(0.011)	(0.011)	(0.012)
p_{it_0}				0.016*	0.015*	0.016*
				(0.009)	(0.009)	(0.009)
$netwage$	-0.020	0.001	-0.004	-0.005	-0.005	-0.005
	(0.013)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
$loghhinc$	0.001	0.001	0.002**	0.002*	0.002*	0.002*
	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Pseudo R^2	0.059	0.078	0.054	0.055	0.058	0.077
N	6,711	6,814	33,753	33,753	33,753	33,753

The estimated marginal effects are reported with robust standard errors in parentheses. *p < 0.1, **p <0 .05, ***p < 0.01. Estimates in columns (1) and (2) are based on the 1986 and 1998 GSS respectively. Columns (3) to (6) are all based on the GSS of 1986, 1992, 1998, and 2005. See Appendix for estimates of other covariates.

Table 3.4: Tobit estimates of daily volunteer minutes

	1986 only	1998 only	1986-2005	1986-2005	(4) + religion controls	(4) + CMA fixed effect
	(1)	(2)	(3)	(4)	(5)	(6)
p_{it}	10.445	37.444**	17.437*			
	(31.177)	(17.555)	(10.222)			
Δtp_{it}				28.537**	28.064**	25.777**
				(13.525)	(13.435)	(12.820)
p_{it_0}				16.910	16.381	14.800
				(10.376)	(10.360)	(9.747)
$netwage$	-32.853	0.988	-4.308	-5.243	-5.183	-5.076
	(21.402)	(2.917)	(4.348)	(4.684)	(4.620)	(4.485)
$loghhinc$	1.961	0.686	2.934*	2.719*	2.647*	2.352*
	(4.469)	(1.544)	(1.542)	(1.529)	(1.542)	(1.414)
Pseudo R^2	0.025	0.035	0.024	0.024	0.025	0.039
N	6,725	6,814	33,753	33,753	33,753	33,753

The estimated marginal effects for the expected daily volunteer minutes conditional on volunteering, $E(y|y>0)$, are reported with robust standard errors in parentheses. *p < .1, **p < .05, ***p < .01. Estimates in columns (1) and (2) are based on the 1986 and 1998 GSS respectively. Columns (3) to (6) are all based on the GSS of 1986, 1992, 1998, and 2005. See Appendix for estimates of other covariates.

Table 3.5: Probit estimates of annual volunteer indicator

	1998 only	1998-2005	1989-2005	(3) +CMA fixed effect	1989-2005	(5) +CMA fixed effect
	(1)	(2)	(3)	(4)	(5)	(6)
p_{it}	0.227***	0.173***	0.166***	0.167***		
	(0.078)	(0.023)	(0.021)	(0.021)		
Δtp_{it}					-0.011	-0.010
					(0.027)	(0.027)
p_{it_0}					0.233***	0.234***
					(0.025)	(0.025)
$netwage$	0.003	0.011	0.016**	0.018**	0.022***	0.023***
	(0.018)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
$loghhinc$	0.042***	0.043***	0.044***	0.045***	0.046***	0.047***
	(0.012)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Pseudo R^2	0.042	0.044	0.049	0.052	0.049	0.053
N	6,810	52,348	68,610	68,585	68,610	68585

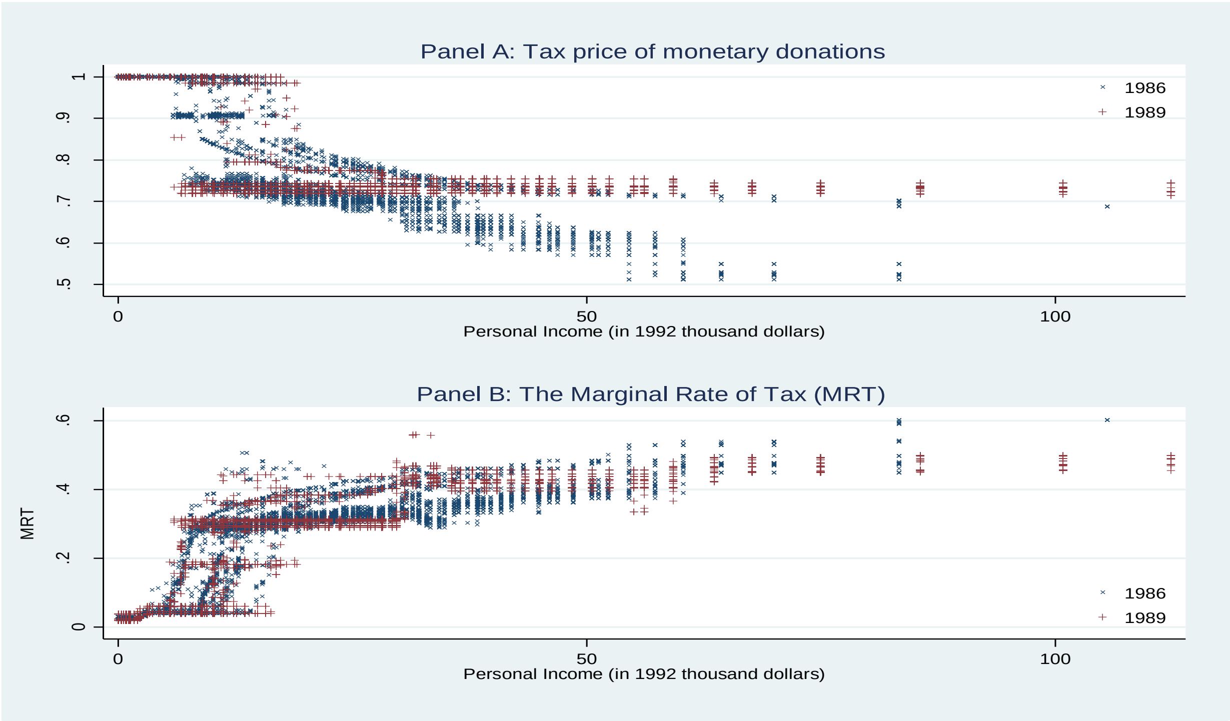
The estimated marginal effects are reported with robust standard errors in parentheses. *p < 0.1, **p <0 .05, ***p < 0.01. Estimates in column (1) are based on the 1998 GSS. Column (2) is based on the GSS of 1998, 2000, 2003, and 2005. Columns (3) to (6) include the GSS of 1989 and 1990 in addition to those used in column (2). See Appendix for estimates of other covariates.

Table 3.6: Cross-price elasticity of donations of time

	(1)	(2)	(3)	(4)	(5)	(6)
Daily volunteer indicator (Table 3)						
p_{it}	0.639	2.247**	0.956*			
Δtp_{it}				1.385**	1.372**	1.342**
Daily volunteer minutes (Table 4)						
p_{it}	0.041	0.244**	0.092*			
Δtp_{it}				0.143**	0.141**	0.134**
Annual volunteer indicator (Table 5)						
p_{it}	0.543***	0.423***	0.441***	0.445***		
Δtp_{it}					-0.030	-0.026

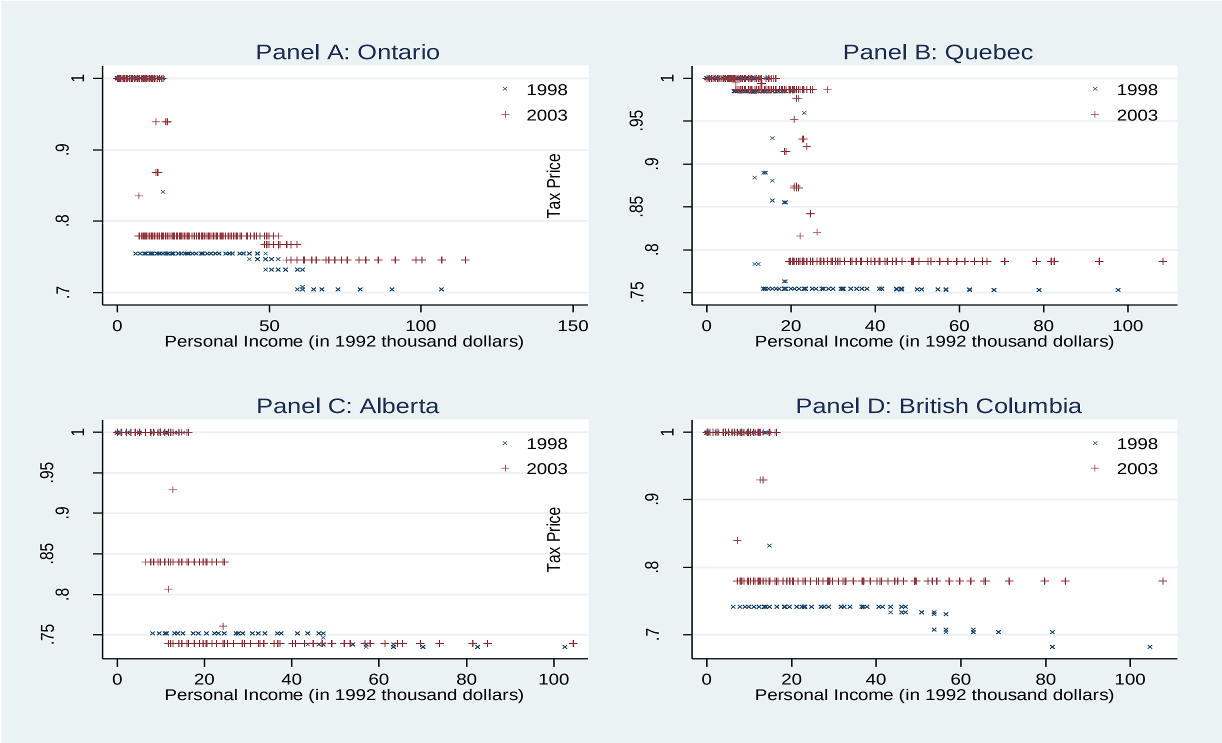
The cross-price elasticity is calculated based on the estimates in the corresponding columns of Table 3.3, Table 3.4, and Table 3.5.

Figure 3.1: The first-dollar tax price of monetary donations and the marginal rate of tax over the personal income: 1986 vs. 1989



* the income is the average of each 50-individual bin for data confidentiality reasons.

Figure 3.2: Tax price and personal income: 1998 vs. 2003



* the income is the average of each 20-individual bin for data confidentiality reasons.

Figure 3.3: Average Tax Price and Average daily volunteer participation: 1986 vs. 1992

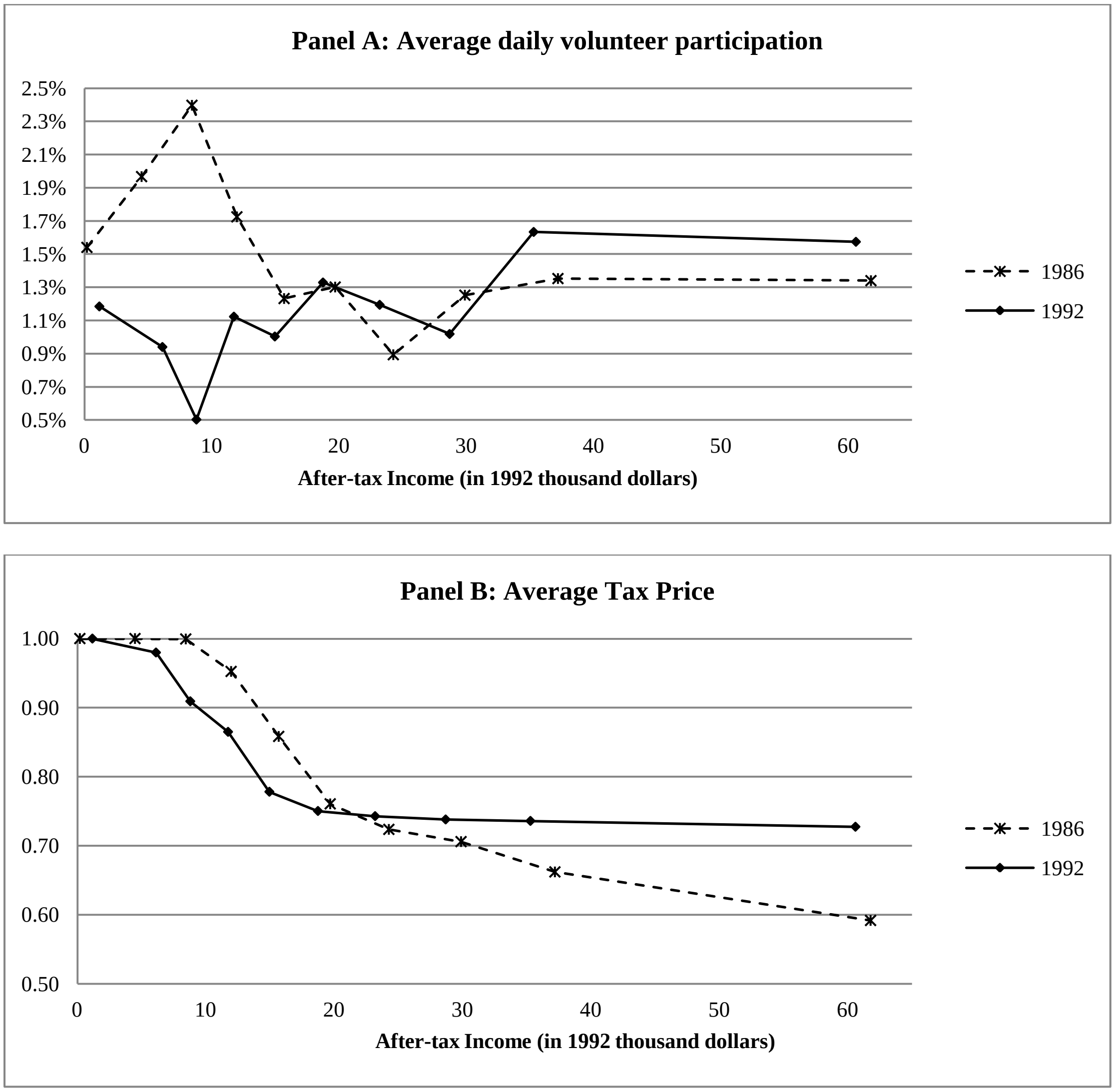
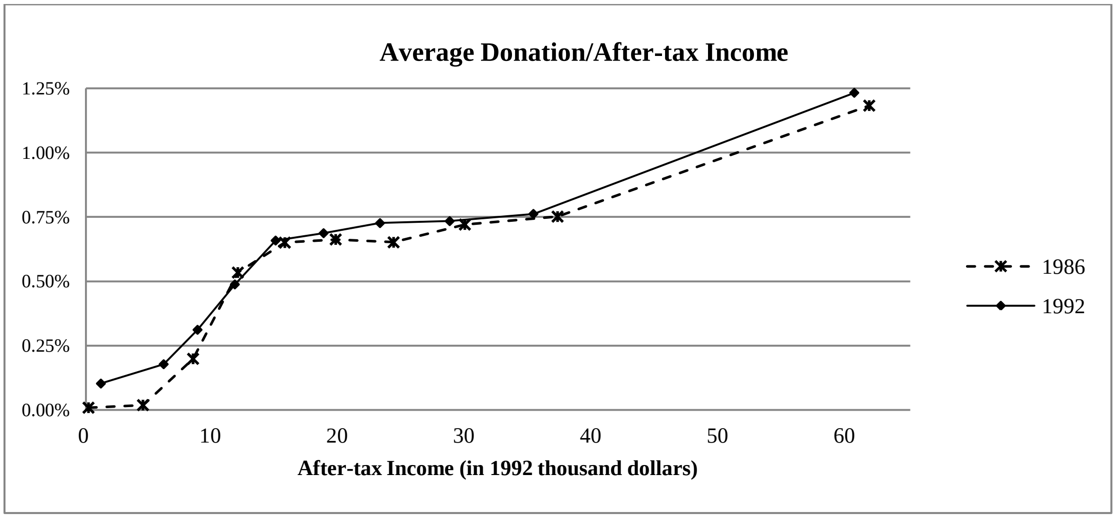


Figure 3.4: Average Donation / Net Income: 1986 vs. 1992



APPENDIX:

Table 3.A1: Probit model of daily volunteer indicator

	1986 only (1)	1998 only (2)	1986-2005 (3)	1986-2005 (4)	(4) + religion controls (5)	(4) + CMA fixed effect (6)
p_{it}	0.300	1.045**	0.444*			
	(0.540)	(0.464)	(0.232)			
Δtp_{it}				0.678**	0.670**	0.657**
				(0.311)	(0.310)	(0.319)
p_{it_0}				0.433*	0.423*	0.412*
				(0.236)	(0.237)	(0.239)
$netwage$	-0.558	0.018	-0.107	-0.127	-0.127	-0.133
	(0.358)	(0.083)	(0.101)	(0.110)	(0.109)	(0.114)
$loghhinc$	0.030	0.020	0.069**	0.064*	0.063*	0.060*
	(0.073)	(0.042)	(0.034)	(0.034)	(0.034)	(0.034)
HS diploma	0.359***	0.192	0.241***	0.242***	0.232***	0.251***
	(0.125)	(0.131)	(0.068)	(0.068)	(0.068)	(0.069)
PSE diploma	0.0352	0.186*	0.115**	0.112**	0.113**	0.115**
	(0.113)	(0.097)	(0.050)	(0.051)	(0.051)	(0.050)
Male	0.001	-0.031	-0.091*	-0.094**	-0.094**	-0.102**
	(0.106)	(0.103)	(0.047)	(0.047)	(0.047)	(0.047)
Age	0.025	0.041**	0.027***	0.027***	0.026***	0.026***
	(0.017)	(0.018)	(0.008)	(0.008)	(0.008)	(0.008)
Age^2	-0.0002	-0.0003	-0.0001*	-0.0001*	-0.0001*	-0.0001*
	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Immigrant	0.047	-0.029	-0.157**	-0.157**	-0.156**	-0.154**
	(0.147)	(0.124)	(0.065)	(0.065)	(0.067)	(0.066)
Married	0.041	0.114	-0.069	-0.064	-0.067	-0.059
	(0.118)	(0.113)	(0.050)	(0.051)	(0.051)	(0.051)
Child(ren)	0.017	-0.015	0.029	0.027	0.027	0.031
	(0.117)	(0.103)	(0.049)	(0.049)	(0.049)	(0.050)
Urban	-0.264**	-0.101	-0.146***	-0.147***	-0.141***	0.601
	(0.124)	(0.094)	(0.052)	(0.052)	(0.052)	(0.585)
pseudo R^2	0.059	0.078	0.054	0.055	0.058	0.077
N	6,711	6,814	33,753	33,753	33,753	33,753

The estimated coefficients are reported with robust standard errors in parentheses. *p < 0.1, **p <0 .05, ***p < 0.01. Estimates in columns (1) and (2) are based on the 1986 and 1998 GSS respectively. Columns (3) to (6) are all based on the GSS of 1986, 1992, 1998, and 2005.

Table 3.A2: Tobit model of daily volunteer minutes

	1986 only (1)	1998 only (2)	1986-2005 (3)	1986-2005 (4)	(4) + religion controls (5)	(4) + CMA fixed effect (6)
p_{it}	104.00	369.9**	172.0*			
	(311.0)	(171.7)	(100.9)			
Δtp_{it}				281.5**	277.3**	267.5**
				(133.6)	(133.0)	(133.3)
p_{it_0}				166.8	161.8	153.6
				(102.5)	(102.5)	(101.3)
$netwage$	-327.2	9.760	-42.48	-51.71	-51.21	-52.68
	(212.9)	(28.82)	(42.88)	(46.21)	(45.65)	(46.56)
$loghhinc$	19.53	6.781	28.93*	26.82*	26.16*	24.41*
	(44.45)	(15.25)	(15.17)	(15.04)	(15.21)	(14.64)
HS diploma	211.8***	69.99	103.5***	104.0***	99.03***	105.6***
	(77.64)	(48.53)	(31.34)	(31.37)	(31.36)	(31.37)
PSE diploma	11.32	55.34	46.11**	44.44**	44.72**	43.81**
	(67.28)	(33.69)	(22.14)	(22.20)	(22.20)	(21.49)
Male	-0.213	-7.869	-39.07*	-40.24*	-40.25*	-42.65**
	(64.85)	(36.93)	(21.20)	(21.30)	(21.41)	(20.66)
Age	14.14	14.46**	11.54***	11.33***	11.05***	10.80***
	(10.37)	(6.31)	(3.62)	(3.64)	(3.64)	(3.63)
Age^2	-0.122	-0.092	-0.062*	-0.060*	-0.060*	-0.055
	(0.106)	(0.063)	(0.035)	(0.036)	(0.036)	(0.036)
Immigrant	38.00	-7.89	-64.90**	-65.16**	-63.26**	-62.30**
	(89.43)	(43.15)	(28.71)	(28.72)	(29.53)	(28.29)
Married	24.85	32.75	-32.94	-30.67	-32.13	-27.46
	(69.04)	(39.49)	(22.30)	(22.41)	(22.49)	(21.86)
Child(ren)	4.84	1.28	10.35	9.48	9.42	10.84
	(71.05)	(36.30)	(21.61)	(21.73)	(21.66)	(21.55)
Urban	-160.1**	-49.23	-67.46***	-67.74***	-65.11***	202.6
	(80.93)	(36.33)	(24.98)	(24.99)	(24.98)	(223.3)
pseudo R^2	0.025	0.035	0.024	0.024	0.025	0.039
N	6,725	6,814	33,753	33,753	33,753	33,753

The estimated coefficients are reported with robust standard errors in parentheses. *p < .1, **p < .05, ***p < .01. Estimates in columns (1) and (2) are based on the 1986 and 1998 GSS respectively. Columns (3) to (6) are all based on the GSS of 1986, 1992, 1998, and 2005.

Table 3.A3: Probit model of annual volunteer minutes

	1998 only (1)	1998-2005 (2)	1989-2005 (3)	(3) + CMA fixed effect (4)	1989-2005 (5)	(5) + CMA fixed effect (6)
p_{it}	0.631***	0.483***	0.479***	0.483***		
	(0.219)	(0.065)	(0.060)	(0.060)		
Δtp_{it}					-0.033	-0.028
					(0.077)	(0.077)
p_{it_0}					0.672***	0.676***
					(0.072)	(0.072)
$netwage$	0.007	0.030	0.047**	0.052**	0.062***	0.067***
	(0.051)	(0.021)	(0.020)	(0.020)	(0.020)	(0.020)
$loghhinc$	0.116***	0.120***	0.128***	0.131***	0.134***	0.137***
	(0.032)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
HS diploma	0.278***	0.235***	0.257***	0.261***	0.264***	0.267***
	(0.060)	(0.024)	(0.020)	(0.020)	(0.020)	(0.020)
PSE diploma	0.211***	0.208***	0.198***	0.200***	0.201***	0.203***
	(0.046)	(0.017)	(0.015)	(0.015)	(0.015)	(0.015)
Male	-0.053	-0.100***	-0.085***	-0.086***	-0.075***	-0.077***
	(0.042)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)
Age	-0.0013	-0.008***	-0.0002	-0.0002	0.0011	0.0012
	(0.0077)	(0.0027)	(0.0024)	(0.0024)	(0.0024)	(0.0024)
$Age^2(/10^4)$	-0.020	0.876***	0.154	0.148	0.041	0.0343
	(0.796)	(0.277)	(0.246)	(0.247)	(0.246)	(0.247)
Immigrant	-0.220***	-0.167***	-0.164***	-0.144***	-0.166***	-0.146***
	(0.062)	(0.022)	(0.019)	(0.020)	(0.019)	(0.020)
Married	-0.020	-0.077***	-0.085***	-0.091***	-0.095***	-0.101***
	(0.048)	(0.017)	(0.016)	(0.016)	(0.016)	(0.016)
Child(ren)	0.077*	0.114***	0.117***	0.117***	0.116***	0.116***
	(0.045)	(0.017)	(0.015)	(0.015)	(0.015)	(0.015)
Urban	-0.243***	-0.220***	-0.199***	-0.144	-0.198***	-0.137
	(0.047)	(0.018)	(0.016)	(0.247)	(0.016)	(0.247)
pseudo R^2	0.042	0.044	0.049	0.052	0.049	0.053
N	6,810	52,348	68,610	68,585	68,610	68,585

The estimated coefficients are reported with robust standard errors in parentheses. *p < 0.1, **p <0 .05, ***p < 0.01. Estimates in column (1) are based on the 1998 GSS. Column (2) is based on the GSS of 1998, 2000, 2003, and 2005. Columns (3) to (6) include the GSS of 1989 and 1990 in addition to those used in column (2).

CHAPTER 4
CHIROPRACTIC PUBLIC SUBSIDY REDUCTIONS IN CANADIAN HEALTHCARE

4.1 INTRODUCTION

Public health insurers and payers play a crucial intermediary role in financing healthcare service provision. The reimbursement schedules they establish provide both incentives and constraints for healthcare providers and patients alike. From a Canadian perspective, Flood, Stabile, and Tuohy (2006) analyze the process by which decisions are made regarding "listing" services for public funding. However, despite changes to benefit schedules being common, as discussed, for example, by the [US] National Association of State Budget Officers (2011) regarding Medicaid cost containment, economic studies focusing on the ramifications of changes in the publically insured basket in developed countries are rare.[24] For governments, acting directly or indirectly, the opportunity cost of adding or removing a health service in the coverage basket is the social welfare benefit of the service(s) that would alternatively be insured, and/or the cost of collecting the taxation or premium funding.

[24] There is, however, an extensive related literature examining payment caps, co-payments and the like for pharmaceuticals; a Cochrane review is by Austvoll-Dahlgren, Aaserud et al. (2008).

Associated with the decision to list a service for public insurance are decisions regarding the magnitude and the form of the subsidy. Schokkaert and Van De Voorde (2011) provide a useful survey of user charges, which is effectively the same decision from the patient perspective. Smith (2005) develops a particularly interesting theoretical analysis addressing the interface between subsidies/user charges. Assuming a fixed public healthcare budget that can be augmented by user charges, the decision to subsidize and the degree of subsidy (i.e., the size of the user charge) is a function of: cost-effectiveness, the price elasticity of demand, the epidemiology of the disease, and equity considerations.

Part of the information base that could be employed in such decision-making is the causal impact of marginal policy changes on patients and providers. In this paper we estimate the causal impact on the labour market outcomes of chiropractors and on patient utilization of a series of Canadian “delisting” policies for chiropractic care that are credibly exogenous to providers and patients. Delisting in this case implies completely removing, or reducing the annual cap on, a fixed public subsidy per visit in a context with "balance billing" whereby public insurance pays the cost up to a maximum and the patient pays the balance. Stabile and Ward (2006) survey a range of delisting, including the partial ones in the early years of the time period we study, but only from the patient side.

Chiropractic is a particularly interesting example on the margins of Medicare. It is one of the most frequently sought complementary/alternative health care services, with most patients seeking it for lower back pain. These policy changes are particularly amenable to study for three reasons. Firstly, there exist clear-cut delisting policies implemented in different provinces at different periods that are credible sources of exogenous variation with respect to patients and providers, and chiropractic services are treated as a single item in the schedule of benefits making the effects on providers and patients relatively easy to observe. In addition, the comparison group includes a province with chiropractic services covered throughout the study period and also provinces where these services were never covered. Secondly, the chiropractic profession is comparatively independent of the mainstream healthcare system, which makes the results less likely to be affected by any simultaneous changes elsewhere. Lastly, chiropractors have not only the attributes of medical specialists, but also attributes of primary care providers, and in all provinces in Canada chiropractors are subject to education requirements, licensing processes, and health professional regulatory environments.

Beyond Canada, changes to elements of the insurance coverage schedule and user charges are also found in US and European mixed-funded health systems. A particularly well-known large-scale effort to prioritize/ration insured Medicaid coverage across a range of services involved the US Oregon Health Plan of the 1990s (Bodenheimer, 1997). McKnight (2007) is an interesting exploration of a policy change that is the converse of that here on some dimensions, but with many underlying commonalities. He looks at the prohibition of "balance billing" for physician services under US Medicare and finds that it is almost a pure income transfer from physicians to patients. In the European context Robinson (2002) discusses the margins of public healthcare plans and analyzes the arguments for user charges. There is a larger research literature looking at user charges in medium and less developed countries as discussed by Schokkaert and Van De Voorde (2011).

More broadly, despite a large research literature looking at differences in healthcare utilization across insurance coverage status (e.g., Institute of Medicine, 2003), there are relatively few studies with credible sources of exogenous variation that allow the magnitude of causal impacts to be estimated for specific components of the benefit schedule. In contrast, some recent studies addressing causal impacts of coverage (access to the entire schedule of benefits) on utilization are by Finkelstein, Tubman et al. (2011), Moreira and Barros (2010), Anderson, Dobkin and Gross (2010), and Card, Dobkin and Maestas (2008).

Since this study effectively looks at changing (a type of) co-payments, an important benchmark against which our patient-side findings can be compared is the well-known RAND Health Insurance Experiment, which was designed to investigate the effect of alternative health insurance plans on the utilization of health services, health expenditure, health status, quality of care, and patient satisfaction (e.g., Manning et al., 1987). Around six thousand persons were assigned to different fee-for-service insurance plans with out-of-pocket payment rates of 0, 25, 50, or 95 percent. Manning et al. (1988) estimate two-step models for both inpatient and outpatient services and find impacts on the intensive margin (i.e. the number of medical contacts), rather than on the extensive margin (i.e. on whether there is any contact at all). Moreover, they find that the price elasticity for medical care is in the range from -0.1 to -0.2.

The contribution of this paper is two-fold. Firstly, it is, to the best of our knowledge, the first paper that investigates the effect of delisting policies from both patients' and providers' perspectives. Secondly, we implement a range of methodological approaches that may be of general interest. In this context we observe jurisdictional-level, not individual-level, policy changes that occur for a relatively small number of jurisdictions. This implies econometric challenges since we need to take into account not only the common problem of macro variables in micro regressions, but the small numbers of treated and untreated jurisdictions imply that the usual approaches that rely on the asymptotic properties of the cluster-robust variance estimators are unreliable.

Using difference-in-differences we find no statistically significant effect of partial delisting on either chiropractors or patients. In contrast, for full delisting, on the chiropractor side statistically and economically significant impacts on incomes and annual weeks of work are observed, but no effect on weekly hours of work is found. The average earned income of chiropractors decreased by 15.2% and they worked around 1 additional week per year. For patients, the full delisting causes a large and statistically significant decrease in the utilization of chiropractic services on both the intensive and extensive margins. Utilization decreased by 23%, which if we apply the average benchmark price change in the province of Ontario implies a price elasticity ranging from -0.35 to -0.96 depending upon the assumptions invoked regarding the service quality. The large elasticity compared to that observed in the RAND Health Insurance Experiment may result from chiropractic being a complementary/alternative form of care, and it makes some sense that services on the margins of Medicare with close substitutes would have higher elasticities.

The remainder of the paper proceeds as follows. In section 2, we introduce the context of chiropractic care in Canada, the delisting policies in the 1990s and 2000s, and relevant policy issues. The data and some empirical issues are discussed in sections 3 and 4 respectively. In section 5, we present our empirical results. Finally, the conclusions are drawn in section 6.

4.2 Institutional context

Chiropractic is well established in the U.S., Canada, Australia and some other jurisdictions and is growing rapidly. The Canadian Institute for Health Information (2011) reports that there were almost 8,000 chiropractors in 2009. This is a comparable number to speech-language pathologists, appreciably more than optometrists, but fewer than pharmacists or physiotherapists. In Canada, provincial Medicare programs are the dominant form of health insurance. According to the federal Canada Health Act, provincial governments are mandated to fund broadly similar "medically necessary" physician and hospital services in return for federal financial contributions. Many medical technologies and services, ranging from pharmaceuticals outside of hospitals to chiropractic services, are, however, beyond the purview of the Act and in these areas there are substantial policy differences across provinces.[25] Like other health insurers, for both technological and budgetary motivations provincial governments in Canada continually make choices regarding funding new services and partially or fully delisting others. Given government fiscal pressures, for public health insurance plans delisting is not only a method for reallocating healthcare resources, but it is an overall budget-control instrument.

At the start of our data period in 1990, five provinces (Quebec, New Brunswick, Nova Scotia, Prince Edward Island, and Newfoundland) did not cover chiropractic, while the other five (British Columbia, Alberta, Saskatchewan, Manitoba, and Ontario) subsidized coverage. As detailed in table 1, chiropractic services started to be de-insured in the middle of the 1990s, which was a period of significant budget cuts as governments reduced their debt and deficits. These changes fall into two categories: partial and full delisting. A partial delisting involves a decrease in the annual insurable maximum that affects intensive users only and introduces a comparatively small demand shock. For full delisting, chiropractic care is removed from the public insurance plan altogether. In all jurisdictions where funding is provided, copayments are required given the limited coverage per visit. Chiropractic services are, therefore, funded by a mixture of users' out-of-pocket payments, private and (where available) public insurance. Prices are set by individual practitioners, although provincial associations commonly establish

[25] Within each province these programs provide universal social insurance and have no risk adjustment or other features commonly found in private insurance markets. See Van de Ven and Schut (2011) for an international discussion of relevant related issues, and Marchildon (2006) for a description of the Canadian healthcare system. In Canada, by law and with relatively minor exceptions, private payers are not permitted for core hospital and physician services. Non-core services may, in contrast, be funded out of pocket or through private health insurance.

recommended fees. Manga (2004) documents that chiropractors in Ontario charged, for example, around $60 for the first visit and $30 for subsequent visits just prior to chiropractic care being delisted in 2004. Of these charges, public health insurance covered respectively $11.75 and $9.65.

When chiropractic was delisted from the public health insurance plan, private insurance and/or out-of-pocket payments necessarily increased among continuing patients, which exerted a negative influence on utilization and equilibrium prices. Related to this, substitution may occur since physiotherapists and massage therapists provide services that can be seen as close to those of chiropractic care. The equilibrium may also be affected by chiropractors spending more time and/or costs on marketing, promotion or administration, and, as suggested by Gordon et al. (2007) based on qualitative work looking at reduction in the subsidy for physiotherapy, service quality could improve and time per patient increase. Moreover, the response to the policy change may differ across patient groups; those with different income levels may have dissimilar price elasticities and/or preferences for quality. Neither prices nor service quality can, however, be observed directly in the data available.

4.3 DATA AND DESCRIPTIVE STATISTICS

Twenty percent samples from Canada’s 1991, 1996, 2001, and 2006 censuses are used to study chiropractors, and the National Population Health Survey (NPHS) is employed for patient side outcomes. Along with basic demographics, detailed occupational and educational (including postsecondary field of study) information is collected in the census, as are data on earnings (inflation adjusted to $2002), weeks worked in the preceding calendar year, and hours worked in the week prior to each census. Individuals aged 25 to 65 who reported working and identify their occupation as "chiropractor" or "doctor of chiropractic", and whose highest level of education is a bachelor's degree or higher are included in the sample.

Table 2 presents the descriptive statistics for chiropractors across the census years. Earnings are defined in two ways. Following one convention they are employment plus positive self-employment earnings, which allows for the natural logarithm of earnings to be taken and removes the effect of large capital investments (taxable losses). Given that the census masterfiles do not have top-coding or other adjustments common in public use files, to deal with measurement error we exclude those with no earnings and a small number of outliers using Chauvenet's criterion (Barnett and Lewis, 1994). Second, the straightforward sum of employment and self-employment earnings is presented and this quantity is employed in quantile regressions without removing negative values, or outliers since this estimation method is less sensitive to them. The sample using the first definition is employed for all analyses except the quantile regression. Average earnings are also presented for four groups of provinces in table 2: those with a complete delisting (which may have also have a partial delisting), those with a partial delisting only, the one province with continuous funding for chiropractic, and those provinces that never funded chiropractic services. Among the other characteristics, key trends are the very substantial increase in the number of practicing chiropractors from 1990 to 2005 and the associated continuous decrease in their annual incomes. However, these trends appear to be common across provinces and are distinguished from any delisting effect via the comparison group.

The NPHS is a biennial longitudinal survey from 1994 to 2008 conducted by Statistics Canada with an initial sample of 17,000 individuals and data available. Socio-demographic information is collected and sample weights are provided to address attrition. Information on the number of chiropractic visits in the last 12 months is collected for every member in the household aged 12 and older and no restrictions are imposed in selecting the sample for analysis.

Table 3 documents chiropractor utilization from 1994 to 2008. Over 10% of the population visits a chiropractor at least once per year and the percentage increases across the decade. The number of visits per capita similarly increases, but the annual number of visits per patient is more stable. Table 4 presents the demographics of chiropractor patients compared to non-patients. Although not shown in the tables, across all 16 years of the data 27.2% of the population see a chiropractor at least once with an average total number of visits of 22.8 among this group and 6.2 visits for the whole population.

4.4 EMPIRICAL STRATEGY

Since the delisting policies constitute sources of variation that are exogenous to practitioners and patients, we implement difference-in-differences approaches to estimate the causal impacts of the policy changes.

4.4.1 CHIROPRACTOR REGRESSIONS

Using census data, chiropractor outcomes are specified as:

$$y_{ipt} = \alpha + \beta_0 partial_{pt} + \beta_1 full_{pt} + \delta Prov_p + \mu Year_t + [\gamma X_{ipt}] + \varepsilon_{ipt} \quad (1)$$

where $Prov$ and $Year$ are fixed effects for province and census year respectively, and X is a set of control variables in brackets to indicate that the elements of X vary across specifications when included. Two indicator variables, *partial* and *full* delisting, are defined according to Table 1 and are set to zero in the periods in each province before each policy change and to one following implementation.[26] For provinces that did not have a policy change, the variables are set to zero in

[26] The partial delisting in Alberta happened late in 1995 and is treated as having happened between 1995 and 2000.

all periods regardless of whether the province insured chiropractic services or not. The delisting indicators, therefore, measure deviations from province fixed effects.

The dependent variable y indexes individual i of province p in census year t. Several dependent variables are employed across specifications, including: first, the logarithm of annual earnings, annual weeks of work, and working hours in the reference week by OLS, and, second, annual earnings by median/quantile regression. Also, in a third approach the data are aggregated within provinces for each year in a two step procedure. Individual characteristics (i.e., X_{ipt}) are partialed out in the first stage using the same approach as in Bertrand, Duflo and Mullainathan (2004), and Cameron, Gelbach and Miller (2008), but without the logarithmic transformation for the dependent variable. In the second stage, a version of equation 1 is estimated by OLS with the i subscript suppressed and without the term brackets.[27] These three methods explore both the robustness of our findings as well as alternative approaches to inference as discussed in section 4.3.

Three specifications cumulatively add control variables as outlined in the notes under the tables. The first is a base model with no controls, which answers an unconditional question often of relevance to policymakers. Subsequent regressions present conditional results. If the treatment in our quasi-experiment is "as good as randomly assigned" there should be very little change in the coefficient on the difference-in-differences variable with the addition of covariates, although precision is likely to increase since the residual variance decreases. Elements of the error term, ε, may be arbitrarily correlated within provinces, but are treated as independent across provinces; inference is discussed in section 4.3. The remaining elements of the equation are coefficients, or vectors of coefficients, to be estimated.

[27] Since earnings are not transformed in the aggregate regressions there is no straightforward relationship between these coefficients and those estimated in the individual-level earnings regressions.

4.4.2 PATIENT REGRESSIONS

Patient-side regressions are estimated by panel data models using the eight waves of the NPHS. Since the unobserved heterogeneity is likely to correlate with the transitory shocks we use fixed, rather than random, effect models. The regressions have a form similar to that in equation 1:

$$y_{ipt} = \beta_0 partial_{pt} + \beta_1 full_{pt} + \mu Year_t + [\gamma X_{ipt}] + u_i + v_{ipt} \quad (2)$$

where u_i is an individual fixed effect (nuisance parameter) that is not estimated since Chamberlain's (1980) conditional likelihood is employed. Unlike specification 1 a province fixed effect is not included in the base specification since it is collinear with the individual fixed effect. However, a provincial fixed effect is added to the demographic variables, and is identified by those who switch provinces across waves. The v_{ipt} term is clustered by province. Two dependent variables are investigated: an indicator for having had at least one visit to a chiropractor, and a count of the number of chiropractic visits in the last 12 months for the whole population. Conditional logit models are specified when the variable is dichotomous, and negative binomial models when a count variable serves as the dependent variable. Similar to the analysis with the census data, three specifications with increasingly large sets of control variables are estimated. Also, a version of the model is estimated by OLS on aggregated data.

4.4.3 IDENTIFICATION AND INFERENCE

Identification in difference-in-differences models rests on the "common trend" assumption as discussed by DiNardo and Lee (2010). Given the broadly similar institutional and contextual background across provinces within Canada, and since a key motivation for delisting was broad-based government fiscal capacity constraints leading to a wide range of cost containment activities in health and other sectors, this assumption seems plausible. Nevertheless, a threat to internal validity is that provincial governments with greater chiropractic utilization growth rates might have a higher probability of delisting services, in which case the measured impact is an underestimate of the "true" causal one. The common trend assumption, which is central to identification in difference-in-differences models seems plausible in this context. While it cannot be tested, the assumption can be supported by a “falsification test”. Unfortunately, this is not feasible with the NPHS data since the first policy change happens between the first and second waves. However, it is feasible to test it in a limited way using census data. We conduct two separate tests; one for each of the partial and full treatment groups of provinces during the pre-delisting period (i.e., 1990 to 1995 after removing Alberta from the sample) and the difference-in-differences estimates based on these placebo policy changes show no statistically significant impacts. A more subtle related modeling issue is that the identifying assumption cannot be simultaneously true with the dependent variable specified in logarithms and levels, and it is not clear which is preferred.[28] Both may, however, be reasonable approximations and as discussed earlier in section 4 we employ both for comparison purposes.

Related to identification, the delisting indicator coefficients measure the average impact of each policy type, and do not reflect any individual change. In the presence of jurisdiction-specific elements of the error term, which seem appropriate, it is not clear how individual impacts could be identified without very strong assumptions. See Imbens and Wooldridge (2009, 6.5.3) for a discussion of identification.

Inference must be approached carefully in difference-in-differences specifications such as those employed here where policy events do not vary within groups, and where intra-group correlations are plausible (that is, the errors are potentially clustered within province and the policy changes are effectively macro variables in microdata regressions as in Moulton, 1990).

[28] This is, of course, a ubiquitous problem in difference-in-differences models.

Bertrand, Duflo and Mullainathan (2004) find over-rejection of the null hypothesis to be close to ubiquitous. A number of papers subsequently develop methodological approaches to this problem for the common case where the number of treatment groups is small and the asymptotically justified solution of clustering the standard errors tends to over-reject the null hypothesis leading to incorrect inference.

Donald and Lang (2007) point out that the degrees of freedom used in inference are not determined by the number of individual observations in the dataset, but are closer to the number of clusters/jurisdictions subject to the policy change. (The quasi-experimental randomization, and hence the degrees of freedom, are at the level of the policy change and not the individual.) They propose a conservative approach, in which the individual data are aggregated to the cluster level and then used to estimate the difference-in-differences model. Since the number of policy changes is small the t-distribution rather than the normal is also used for critical values. In line with this approach, we estimate the aggregate data models discussed earlier in section 4.

Cameron, Gelbach and Miller (2008) propose bootstrap-based approaches to improve inference that appear to be very promising. Comparing a variety of bootstrapping techniques, they argue based on both theory and Monte Carlo evidence for: resampling clusters, using the wild bootstrap, imposing the null hypothesis, and simulating t-statistics to permit asymptotic refinement.[29] They find that, in cases with ten clusters, the wild cluster bootstrap on t-statistics performs well in coping with the over-rejection problem. They also find support for Donald and Lang and observe that a practical approach to addressing the problem is to conduct inference

[29] The t-statistic, instead of the standard error, is bootstrapped because it is asymptotically pivotal, which provides the possibility of asymptotic refinement. We thank Cameron, Gelbach and Miller for making their code available on their websites. A rhetorical downside of this approach is that it bypasses the generation of standard errors, which Angrist and Pichke (2010) argue some economists like to be able to observe.

using t-tests where the degrees of freedom are the number of clusters minus the number of parameters that are invariant within clusters (G-2).[30]

For the OLS regressions on microdata we, therefore, present standard errors and associated levels of statistical significance based on t-statistics with G-1 degrees of freedom, and we also generate the more credible wild cluster bootstrap and report p-values based on the bootstrap distribution of the t-statistic for comparison. However, since the wild bootstrap with clustering is not well understood for panel logit models, we calculate p-values based on the t-distribution with G-2 degrees of freedom for these models estimated in addition to using clustered standard errors. Feng et al. (2011) discuss the wild bootstrap for median regression and find that it works well; we implement a wild cluster bootstrap.

4.5 RESULTS

At the new equilibrium, chiropractors' labour market outcomes and patients' chiropractic utilization are determined by both the price increase affecting demand, and any response on the supply side by chiropractors. The difference-in-differences approach is unable to separately identify the response functions of both chiropractors and patients. However, it can estimate the policy effect on equilibrium outcomes.

4.5.1 CHIROPRACTORS

Chiropractors can change their working time relatively quickly in response to delisting and table 5 presents regression results looking at this issue using individual level data on annual weeks of work and hours of work in the Census week. While the partial delisting has no statistically significant effect on either weeks or hours of work, full delisting increases the annual work

[30] Some software provides the option of cluster-robust standard errors, but the degrees of freedom adjustment may or may not be made in obtaining critical values. While this makes little difference in samples where G is large, it has an appreciable effect on the reported p-values in studies such as ours with a modest number of clusters.

weeks by about one per year after the policy change in the provinces that delist compared to the others. For the individual data in the upper panel the effect is statistically significant with both the clustered and the bootstrapped approaches to inference, although the bootstrap approach has larger p-values. Since the average chiropractor works 48.0 weeks per year before delisting, the estimated effect is equivalent to a semi-elasticity of approximately 2.5% (based on column 3). The lower panel addresses the same issue using aggregate data and has similar findings for the point estimates. Also, the p-values in brackets based on bootstrapping in the upper panel and using a t-statistic with G-1 degrees of freedom in the lower panel are very similar. The results appear to be robust across approaches.

Next we investigate the effect of delisting on chiropractors' annual earned income in table 6. In the top panel, results are presented for OLS ln(earnings) regressions on individual data. The middle panel presents the median regression. The bottom is based on the aggregated data. Although all of the point estimates for the partial delisting are negative, none of them are statistically significant. In contrast, the full delisting causes annual earnings to fall by about 15 to 17%, or $10-$13,000. The preferred p-value in brackets in each panel shows the estimated effects are all statistically significant. However it is notable that the preferred estimates of the p-values are appreciably larger than those obtained by more conventional cluster-robust methods even with the degrees of freedom adjustment – they appear to exhibit the tendency to over-reject with small numbers of clusters as noted by Bertrand, Duflo and Mullainathan (2004). Since average annual income was about $59,700, the estimated semi-elasticities of partial and full delisting using the estimates in column 2 correspond to decreases of around $1,500 and $9,100 in 2002 dollars respectively, which is quite close to the estimates in the lower two panels given their standard errors. Full delisting imposed a statistically and economically significant negative effect on chiropractors' annual earnings.

4.5.2 PATIENTS

Estimates from the NPHS employing Chamberlain's (1980) fixed effect conditional logit model to model the probability of having at least one visit to a chiropractor in the year are displayed in the upper panel on the left hand side of table 7. Results from the negative binomial fixed effect model for the annual number of chiropractic visits are in the upper-right of table 7. Beneath each is the aggregate data specification with an equivalent dependent variable. In none of the four specifications is the coefficient associated with the partial delisting statistically significant and the point estimates are of mixed signs. This differs from the results in Stabile and Ward's (2006) study of the partial delisting since they sometimes find statistically significant reductions in the number of visits among those with at least one visit. However, they did not take the clustered nature of the data (i.e., that the source of exogenous variation is at the level of the province) into account in undertaking inference.

For the full delisting, the upper panels of table 7 show that the probability of an individual visiting a chiropractor at least once in the year, the extensive margin, decreased by three or four percent. The incidence-rate ratios from the count models show a decrease in the risk of a visit, on a combination of the intensive and extensive margins, of approximately 12% (i.e., approximately [1-0.88]*100%). These estimates vary little across the columns, adding confidence to our interpretation of the difference-in-differences coefficient as the causal impact of delisting since the impact is not affected by the introduction of other variables that should not be relevant. However, the aggregate-level regressions in the lower panels provide a slightly different story for the annual chiropractic visit regressions, which have coefficients that are not statistically different from zero although the point estimates are negative. Greater precision may be allowed by the fixed effects. In the lower right-hand side of the table the aggregate regressions looking at the count of visits have coefficient estimates that are negative and statistically significant. Like those on the left-hand side of the table, the p-values from the aggregate regressions for the count models are larger than those for the fixed effect ones.

In table 8 we look at policy effects across household income groups since families with different incomes may have different responses to delisting. Three mutually exclusive dummy variables, indicate whether the household income is in the bottom quarter (low income), the second or third quarter (middle income), and the top quarter (high income).[31] These income indicators are interacted with the delisting variables. Oddly, for the partial delisting there are some coefficients that are positive and statistically significant for the middle and high income groups. However, unlike all the results seen to this point the statistical significance is not consistent across the three models estimated in each panel. It is possible that this reflects a statistical artifact – we expect to reject the null hypothesis when it should not be rejected 10% of the time when testing at the 10% level. Or, it may reflect a weak trend towards increasing relative utilization in the higher income groups in the provinces undertaking partial delisting that raises questions about the common trend assumption. If the latter is correct then the estimates obtained thus far are biased towards being too small compared to the causal impacts.

In contrast, the results for the full delisting in table 8 accord with our prior expectations. In both panels the coefficients indicate that the reduction in utilization is concentrated among those with low and middle incomes, with the low income households having a larger decrease than the middle income ones. No statistically significant coefficients are observed for individuals

[31] We use the average household income over all periods to derive the income ranking.

from high income households, which is expected not only because of the income effect but also because high income households are more likely to have more generous private health insurance.[32]

4.6 DISCUSSION AND CONCLUSION

We empirically investigate the effect of delisting chiropractic services from Canada's public health insurance programs. Both the labour market outcome of chiropractors and chiropractic service utilization is studied. Overall, the partial delisting is not observed to have had statistically significant impacts in any model we estimate, although most of the point estimates suggest declines in utilization as anticipated. Much larger declines are observed for the full delisting of this service, which had statistically and economically significant impacts for both providers and patients. This difference in impacts should perhaps not be too surprising since the partial delisting were all reductions on the intensive, not the extensive, margin affecting the number of visits subsidized rather than the subsidy per visit; patients not at the upper bound would not have been affected at all and the NPHS evidence suggests that most patients would have been below the upper bound. In contrast, the full delisting affected the price of each and every visit for those without private insurance that covered the entire cost. Moreover, since chiropractic is a complementary and alternative form of care it may well be that it was affected by an additional factor beyond price. Some (potential) patients may have perceived the removal of the government subsidy as an indirect statement about quality and/or effectiveness.

Bringing some of these estimates together, table 7's negative binomial model shows how delisting changes the propensity for any visits to a chiropractor taking both the extensive and

[32] Unfortunately, the NPHS does not have an adequate measure of health insurance to address chiropractic care coverage.

intensive margins into account. Full delisting per capita decreased chiropractic visits by 0.23 per year (or approximately a 23% decrease since the utilization rate is around 10% and a visitor on average visits 10 times per year). In Ontario, the full delisting caused the out-of-pocket payment of a chiropractic visit to increase from around \$20 to \$33 (Ontario Chiropractic Association, 2007), which implies a 65% increase.

In table 5 we estimate that chiropractors increased their working time by about 2.5% after full delisting. Although we do not know how this time was spent, we can place upper and lower bounds on the price elasticity of chiropractic services under different assumptions. If chiropractors keep the service time (quality) in each visit unchanged and use all extra time on, say, administration and/or promotion, the price elasticity is then around 0.35, which is likely to provide a lower bound on the price elasticity of chiropractic services. Alternatively, if chiropractors use all extra time on patient visits (improving quality), it would increase the service time per visit by 33% ($= \frac{1+2.5\%}{1-23\%} - 1$). Taking this incremental service time into consideration, the quality adjusted per visit price actually increased by 24% ($= \frac{1+65\%}{1+33\%} - 1$), which implies a price elasticity of around one. This gives the upper bound of the quality adjusted elasticity estimate. These bounds are clearly very sensitive to assumptions regarding the behaviour of healthcare providers and on the value placed on visit duration by patients. They are also slightly larger than the elasticities observed by Manning et al. (1987), which is consistent with chiropractic not being a core service without close substitutes.

Comparing the utilization and earnings results, the roughly 23% decrease in utilization appears to have translated into a 15% earnings decrease. These numbers are quite similar, especially once the standard errors are taken into account. Also, it is entirely plausible that annual earnings decrease less than utilization since chiropractors may decrease their cost structure, particularly given the slight increase in annual labour supply despite a reduction in the number of patient visits. Overall, these two estimates of different variables from independent data sources seem to accord reasonably well.

In terms of methodology, this paper suggests that robust estimates can be obtained, and inferences undertaken, even for a relatively small number of jurisdictions and policy changes using a difference-in-differences model if appropriate care is taken. In the presence of clustered data (jurisdiction-level sources of exogenous variation), the cluster-bootstrapped t-statistics and the aggregate regressions provide similar results that are more conservative (had larger p-values) than the more traditional approaches involving clustering the standard errors. Compared to the fixed effect models, the aggregate data approaches generate larger p-values, but the point estimates are consistent in sign. Future research exploring the delisting (or new addition to the insured list) of alternative types of care on the margins of Medicare would be worthwhile and allow comparisons across practitioner types that would be cumulatively more informative than any single study.

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Table 4.1: Delisting policies in Canada from 1990 to 2006

Province	Chiropractic care coverage in 1990	Delisting chiropractic from provincial health insurance plan (1990 to 2006)	
		Partial delisting	
Ontario	$11.75 for the first visit, and $9.65 per subsequent visit, with an annual limit of $220	In 1999 the annual limit was decreased from $220 to $150	Ontario
Manitoba	$11.56 per visit, with an annual limit of 15 visits	In 1996 the annual limit was decreased from 15 to 12 visits	Manitoba
Saskatchewan	$16.25 for the initial visit and $12.25 for each subsequent visit	N.A.	Saskatchewan
Alberta	$14 maximum per chiropractic visit, with an annual limit of $300	In 1995, the annual limit was decreased from $300 to $200	Alberta
British Columbia	$7.35 per chiropractic visit, with an annual limit of 12 visits (age <65) or 15 visits (age >=65)	N.A.	British Columbia

* Premiums are provincial and assistance making premium payments is available for low income households. All dollar values are nominal.

Table 4.2: Descriptive statistics of selected census variables by year

	1990	1995	2000	2005	Total
Median earned income	63,775	57,080	47,170	41,610	47,780
Average earned Income	82,130	71,265	59,725	51,250	62,155
ON, BC	84,015	71,780	61,890	47,690	62,070
AB, MN	72,985	73,815	53,855	55,625	60,345
SK	109,950	78,820	67,395	60,565	73,570
Other provinces	80,380	66,665	56,060	55,040	62,105
Weekly work hours	39.4	40.9	38.5	37.2	38.6
Annual work weeks	47.2	48.0	48.0	48.4	48.0
Age	38.9	38.8	38.9	40.2	39.4
Male (%)	86.5	79.8	72.0	69.3	74.7
Married indicator (%)	80.2	79.4	76.2	76.2	77.4
Live in urban area (%)	82.9	81.1	85.4	82.6	83.1
No. of Observations	480	515	785	1,085	2,863

Notes: The median earned income is based on the full sample before outliers are eliminated. All income measures are adjusted to 2002 Canadian dollars using the Consumer Price Index. Sampling weights are employed.

Table 4.3: Chiropractor utilization over time

	1994	1996	1998	2000	2002	2004	2006	2008	Total
Chiro. visit indicator (%)	10.8	10.4	11.4	12.8	12.5	12.9	13.6	14.3	12.1
No. of chiro. visits for population	1.02	0.98	1.15	1.21	1.30	1.30	1.41	1.34	1.19
No. of chiro. visits for patients	9.5	9.4	10.1	9.5	10.4	10.1	10.4	9.4	9.8
No. of Observations	14,113	13,067	12,430	11,170	10,340	9,364	8,761	7,874	87,119

Notes: Sampling weights are employed. Derived from NPHS data.

Table 4.4: Characteristics of chiropractic patients and non-patients

	Patient	Non-patient	All
Male (%)	47.0	49.4	49.1
Age	44.4	43.7	43.8
Married indicator (%)	69.2	61.4	62.4
HS graduate indicator (%)	81.0	74.0	75.0
Bachelor degree indicator (%)	17.0	17.0	17.0
Household income (‘000s)	63.0	58.0	58.6
Health Utility Index	0.874	0.887	0.885
No. of Observations	9,840	77,279	87,119

Notes: Household income is in $2002 using the Consumer Price Index. Sampling weights are employed. Derived from NPHS data.

Table 4.5: OLS estimates of chiropractor weeks/year and hours/week of work

	Annual working weeks			Weekly working hours		
	Basic	(1)+ controls	(2)+ CMA effect	Basic	(4)+ controls	(5)+ CMA effect
	(1)	(2)	(3)	(4)	(5)	(6)
Ind. level data, OLS						
Partial delisting	0.494	0.194	0.116	-0.459	-0.063	-0.039
	(0.490)	(0.445)	(0.470)	(0.902)	(0.772)	(0.681)
	[0.474]	[0.648]	[0.799]	[0.625]	[0.939]	[0.969]
Full delisting	0.664	1.083**	1.232**	-0.354	0.018	-0.003
	(0.385)	(0.418)	(0.445)	(0.735)	(0.585)	(0.411)
	[0.260]	[0.045]**	[0.073]*	[0.631]	[0.933]	[0.987]
R^2	0.004	0.127	0.156	0.031	0.107	0.160
N	2,863	2,863	2,863	2,863	2,863	2,863
Prov. level data, OLS						
Partial delisting	0.494	0.185	0.204	-0.459	-0.040	-0.246
	(0.611)	(0.574)	(0.596)	(1.124)	(1.010)	(0.802)
	[0.466]	[0.681]	[0.647]	[0.586]	[0.982]	[0.757]
Full delisting	0.664	1.086**	1.181**	-0.354	0.020	0.099
	(0.480)	(0.378)	(0.446)	(0.915)	(0.798)	(0.610)
	[0.234]	[0.045]**	[0.065]*	[0.617]	[0.982]	[0.812]
R^2	0.312	0.286	0.865	0.696	0.636	0.977
N	40	40	40	40	40	40

Notes: Based on census data. Standard errors, clustered at the province level, are reported in parentheses; the associated level of statistical significance is based on a t-test with df=G-2. Bootstrap p-values based on 4,999 repetitions using the wild clustered bootstrap distribution of t-statistics are reported in brackets. *p < 0.1, **p < 0.05, ***p < 0.01. The basic model includes 3 year, and 9 province, indicators. Columns (2) and (5) further add demographic variables including gender, age, squared age, marital status, an urban indicator, and 21 five-year birth cohort indictors. Columns (3) and (6) further include 33 CMA indicators plus 10 provincial non-CMA indicators. See Appendix for estimates of other covariates of OLS model with individual level data.

Table 4.6: Estimates of earned income

	Basic	(1) + controls	(2) + CMA effect
	(1)	(2)	(3)
Logged earned income, individual level data, OLS model			
Partial delisting	-0.034	-0.026	-0.009
	-0.075	-0.051	-0.062
	[0.705]	[0.700]	[0.918]
Full delisting	-0.174**	-0.152**	-0.146**
	-0.066	-0.049	-0.05
	[0.030]**	[0.036]**	[0.065]*
R^2	0.048	0.163	0.215
N	2,863	2,863	2,863
Earned income, individual level data, Median regression model			
Partial delisting	-2,492	-3,969	-1,842
	-4,684	-4,327	-4,260
	[0.609]	[0.386]	[0.677]
Full delisting	-8,434**	-7,871**	-9,309**
	-3,814	-3,787	-3,940
	[0.058]*	[0.071]*	[0.046]**
R^2	0.029	0.087	0.126
N	2,904	2,904	2,904
Earned income, provincial level data, OLS model			
Partial delisting	-1,501	-992	-479
	(2,930)	(2,417)	(3,214)
	[0.708]	[0.585]	[0.877]
Full delisting	-12,730**	-11,266**	-10,574***
	(4,023)	(3,591)	(3,037)
	[0.033]*	[0.064]*	[0.076]*
R^2	0.897	0.900	0.954
N	40	40	40

Notes: Based on census data. Standard errors, clustered at the province level, are reported in parentheses; the associated level of statistical significance is based on a t-test with df=G-2. Bootstrap p-values based on 4,999 repetitions for OLS and 1,999 for median regression using the wild clustered bootstrap distribution of t-statistics are reported in brackets. *p < 0.1, **p < 0.05, ***p < 0.01. The basic model includes 3 year, and 9 province, indicators. Columns (2) and (5) further add demographic variables including gender, age, squared age, marital status, an urban indicator, and 21 five-year birth cohort indictors. Columns (3) and (6) further include 33 CMA indicators plus 10 provincial non-CMA indicators. See Appendix for estimates of other covariates of OLS model with individual level data.

Table 4.7: Estimates of changes in the chiropractor utilization

	Annual chiropractic visit Indicator			Annual chiropractic visit number		
	Basic	(2) +	(3) + CMA	Basic	(6) +	(7) + CMA
	Ind. data, FE logit model			Ind. data, FE Negative binomial model		
Partial delisting	0.0174	0.0122	0.0127	1.052	1.055	1.061
	(0.0151)	(0.0142)	(0.0152)	(0.047)	(0.047)	(0.047)
	[0.284]	[0.414]	[0.427]	[0.280]	[0.259]	[0.220]
Full delisting	-0.0405***	-0.0331**	-0.0335**	0.867***	0.877***	0.882***
	(0.0146)	(0.0156)	(0.0161)	(0.038)	(0.038)	(0.039)
	[0.0243]**	[0.0668]*	[0.0712]*	[0.0113]**	[0.0170]**	[0.0215]**
$pseudo\ R^2$	0.009	0.023	0.025	0.010	0.015	0.016
N	3,847	3,847	3,847	4,134	4,134	4,134
$\bar{T}$	6.5	6.5	6.5	6.4	6.4	6.4
$N * \bar{T}$	24,880	24,880	24,880	26,392	26,392	26,392
	Prov. level data, OLS model					
Partial delisting	-0.0322	-0.0345	-0.0248	-0.00542	0.000870	0.0109
	(0.0720)	(0.0738)	(0.0771)	(0.0673)	(0.0562)	(0.0613)
	[0.6123]	[0.5649]	[0.7205]	[0.9287]	[0.9728]	[0.8474]
Full delisting	-0.150	-0.150	-0.145	-0.220*	-0.231**	-0.227**
	(0.0964)	(0.0978)	(0.103)	(0.100)	(0.0864)	(0.0941)
	[0.1726]	[0.1782]	[0.2226]	[0.0836]*	[0.0542]*	[0.0656]*
R^2	0.957	0.954	0.676	0.928	0.929	0.669
N	80	80	80	80	80	80

Notes: In the first panel, estimates are based on an unbalanced panel of the NPHS. Marginal effects from a conditional logit model are reported for the chiropractic indicator and incidence rate ratios from a negative binomial model for the number of chiropractic visits. Standard errors are reported in parentheses and are clustered at the province level. P-values are based on the t-distribution with G-2 degrees of freedom are reported in brackets. In the second panel, estimates are based on provincial aggregates from the NPHS. All estimates are OLS coefficients. Standard errors are reported in parentheses and are clustered at the province level. P-values based on the clustered bootstrap distribution of t-statistics are reported in brackets. *p < 0.1, **p < 0.05, ***p < 0.01. The basic model includes year and province dummies only. Columns (2) further controls for: gender, 15 five-year age indicators, two education indicators, marital status, and household income, and four self-assessed health status indictors. Column (3) further includes 33 CMA indicators plus 10 non-CMA indicators – one for each province.

Table 4.8: Estimates of changes in the number of chiropractic visits using individual level

	Basic	(1) +controls	(2) + CMA effect
	(1)	(2)	(3)
	Annual chiropractic visit indicator, fixed effect logit model		
Partial delisting * low income	-0.014	0.020	0.019
	(0.029)	(0.031)	(0.034)
	[0.652]	[0.536]	[0.592]
Partial delisting * middle income	0.000	-0.006	-0.009
	(0.017)	(0.018)	(0.020)
	[0.987]	[0.744]	[0.671]
Partial delisting * high income	0.043**	0.031	0.036
	(0.021)	(0.021)	(0.022)
	[0.071]*	[0.171]	[0.147]
Full delisting * low income	-0.105***	-0.094**	-0.104**
	(0.028)	(0.043)	(0.043)
	[0.005]***	[0.059]*	[0.041]**
Full delisting * middle income	-0.035**	-0.034*	-0.036*
	(0.016)	(0.020)	(0.021)
	[0.060]*	[0.125]	[0.127]
Full delisting * high income	-0.018	-0.021	-0.017
	(0.018)	(0.021)	(0.022)
	[0.344]	[0.348]	[0.464]
$pseudo\ R^2$	0.010	0.023	0.026
$N * \bar{T}$	24,880	24,880	24,880
	Annual chiropractic visit number, fixed effect negative binomial model		
Partial delisting * low income	1.000	1.095	1.110
	(0.079)	(0.089)	(0.091)
	[0.992]	[0.295]	[0.236]
Partial delisting * middle income	1.078	1.108**	1.105**
	(0.055)	(0.056)	(0.056)
	[0.176]	[0.078]*	[0.086]*
Partial delisting * high income	1.027	0.978	0.990
	(0.055)	(0.053)	(0.054)
	[0.628]	[0.698]	[0.864]
Full delisting * low income	0.633***	0.698***	0.697***
	(0.070)	(0.078)	(0.077)
	[0.003]***	[0.012]**	[0.012]**
Full delisting * middle income	0.867***	0.887**	0.893**
	(0.048)	(0.049)	(0.050)
	[0.032]**	[0.063]*	[0.076]*
Full delisting * high income	0.936	0.920	0.924
	(0.054)	(0.053)	(0.054)
	[0.288]	[0.188]	[0.223]
$pseudo\ R^2$	0.010	0.015	0.016
$N * \bar{T}$	26,392	26,392	26,392

Notes: Estimates are based on an unbalanced panel of the NPHS. Marginal effects from a conditional logit model are reported for the chiropractic indicator and incidence rate ratios from a negative binomial model for the number of chiropractic visits. Standard errors are reported in parentheses and are clustered at the province level. P-values are based on the t-distribution with G-2 degrees of freedom are reported in brackets. *p < 0.1, **p < 0.05, ***p < 0.01. Basic model controls for year dummies and province dummies only. Columns (2) further controls for demographics: gender, 15 five-year age indicators, two education indicators, marital status, and household income, and four self-assessed health status indictors. Column (3) further includes 33 CMA indicators plus 10 non-CMA indicators – one for each province.

APPENDIX:

Table 4.A1: OLS estimates of annual working weeks

	Basic	(1) + demographics	(2) + cohort effect	(3) + CMA effect
	(1)	(2)	(3)	(4)
Partial delisting	0.494	0.654	0.194	0.116
	(0.490)	(0.411)	(0.445)	(0.470)
Full delisting	0.664	0.712**	1.083**	1.232**
	(0.385)	(0.271)	(0.418)	(0.445)
Age		1.463***	1.501***	1.513***
		(0.219)	(0.441)	(0.464)
Age^2		-0.016***	-0.015***	-0.015***
		(0.002)	(0.004)	(0.004)
Male		1.441***	1.512***	1.395***
		(0.420)	(0.359)	(0.385)
Married		1.967***	1.880***	1.681***
		(0.460)	(0.403)	(0.513)
Urban		-0.422	-0.513	-0.401
		(0.341)	(0.332)	(0.491)
R^2	0.004	0.095	0.127	0.156
N	2,863	2,863	2,863	2,863

Notes: Based on census data. Standard errors, clustered at the province level, are reported in parentheses; the associated level of statistical significance is based on a t-test with df=G-1. *p < 0.1, **p < 0.05, ***p < 0.01.

Table 4.A2: OLS estimates of weekly working hours

	Basic (1)	(1) + demographics (2)	(2) + cohort effect (3)	(3) + CMA effect (4)
Partial delisting	-0.459	-0.522	-0.063	-0.039
	(0.902)	(0.891)	(0.772)	(0.681)
Full delisting	-0.354	-0.134	0.018	-0.003
	(0.735)	(0.664)	(0.585)	(0.411)
Age		1.133***	1.428***	1.307***
		(0.184)	(0.290)	(0.307)
Age^2		-0.014***	-0.017***	-0.017***
		(0.002)	(0.002)	(0.002)
Male		7.458***	7.519***	7.392***
		(1.184)	(1.084)	(1.105)
Married		-0.687	-0.645	-0.440
		(0.420)	(0.439)	(0.435)
Urban		0.480	0.431	-0.397
		(0.726)	(0.723)	(0.457)
R^2	0.031	0.091	0.107	0.160
N	2,863	2,863	2,863	2,863

Notes: Based on census data. Standard errors, clustered at the province level, are reported in parentheses; the associated level of statistical significance is based on a t-test with df=G-1. *p < 0.1, **p < 0.05, ***p < 0.01.

Table 4.A3: OLS estimates of earned income

	Basic	(1) + demographics	(2) + cohort effect	(3) + CMA effect
	(1)	(2)	(3)	(4)
Partial delisting	-0.034	-0.014	-0.026	-0.009
	(0.075)	(0.045)	(0.051)	(0.062)
Full delisting	-0.174**	-0.169***	-0.152**	-0.146**
	(0.066)	(0.050)	(0.049)	(0.050)
Age		0.196***	0.159**	0.159**
		(0.023)	(0.051)	(0.0524)
Age^2		-0.002***	-0.002***	-0.002**
		(0.0003)	(0.0005)	(0.0005)
Male		0.159**	0.159**	0.148*
		(0.055)	(0.055)	(0.069)
Married		0.163**	0.160**	0.171***
		(0.055)	(0.053)	(0.051)
Urban		-0.038	-0.039	-0.087
		(0.056)	(0.056)	(0.072)
R^2	0.048	0.153	0.163	0.215
N	2,863	2,863	2,863	2,863

Notes: Based on census data. Standard errors, clustered at the province level, are reported in parentheses; the associated level of statistical significance is based on a t-test with df=G-1. *p < 0.1, **p < 0.05, ***p < 0.01.

CONCLUSION

In this thesis, I examine the impacts of some important policy interventions on individuals' behaviour and attempt to identify the relevant economic mechanisms. I focus especially on two areas that have drawn attention not just in the economics community, but in other academic circles and among the general public. The first three chapters address some interesting questions on individuals' charitable behaviour and the last one is on the healthcare service market. Theoretical models are developed in an attempt to explain the observed phenomena. Empirical investigations are conducted based on high quality data sets from Statistics Canada.

In the first chapter, I develop an overlapping generation public good model, which suggests that, even though a compulsory volunteer policy can stimulate social capital accumulation and hence encourage subsequent volunteering, it may not increase post-school volunteering unless a price elasticity of the public good has sufficient magnitude.

The second chapter empirically investigates a provincial compulsory volunteer policy for high school students in Ontario, Canada. Using the YITS, we find that the policy increased volunteer participation in all observed years during high school. The estimated direct effect is about 6% around ages 17 to 18. For volunteer participation after high school completion, the results show that those affected by the policy volunteered about 5% less than they otherwise would have. This reduction is mostly concentrated among students who, when surveyed at age 15, indicated that they did not intend to go to university after high school. We also find that those who were not directly affected by the policy volunteered less after the introduction of the policy, suggesting the compulsory volunteer policy may have crowded out some true volunteering. The total indirect effect is estimated to be about -6.45% in the subsequent eight years after the introduction of the policy, with some evidence of a diminishing effect.

The third chapter analyzes the relationship between donations of time and money from both theoretical and empirical perspectives. Our model shows that this heterogeneity affects individuals' charitable behaviour. A correlation of volunteer behaviour with the tax price of a charitable donation in a cross-section may just reflect the association of labour market ability with either the preference for the charitable good or the productivity of volunteer hours. Exploiting a series of tax policy changes in Canada, we find that individuals increase their volunteer behaviour on both the extensive and intensive margins as the tax price of charitable donations increases. The cross-price elasticity of daily volunteer participation is estimated to be 1.385 while that of the annual volunteer participation is estimated to be 0.441. We estimate that donations of time and money are substitutes rather than complements. Hence while tax deductions or tax credits increase monetary donations, our estimates suggest that individuals also decrease their volunteer time in response.

The last chapter empirically investigates the effect of delisting chiropractic services from Canada's public health insurance programs. For example in Ontario, full delisting caused the out-of-pocket payment of a chiropractic visit to increase from around $20 to $33, which implies a 65% increase. Both the labour market outcome of chiropractors and chiropractic service utilization are studied. We find that full delisting per capita decreased chiropractic visits by 0.23 per year (or approximately a 23% decrease, since the utilization rate is around 10% and a patient on average visits 10 times per year). On the service provider side, we estimate that chiropractors increased their working time by about 2.5% after full delisting. These estimates allow us to bound the price elasticity of chiropractic services based on different assumptions. If chiropractors keep the service time (quality) in each visit unchanged and use all extra time on, say, administration and/or promotion, the price elasticity is then around 0.35, which is likely to provide a lower bound on the price elasticity of chiropractic services. Alternatively, if chiropractors use all extra time on patient visits (improving quality), the quality-adjusted price implies a price elasticity of around one. This gives the upper bound of the quality-adjusted elasticity estimate.