Three Essays in Labour Economics and Public Finance

by

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Author's Declaration

This thesis consists of material all of which I authored or co-authored: see Statement of Contributions included in the thesis. This is a true copy of the thesis, including any required final revisions, as accepted by my examiners.

I understand that my thesis may be made electronically available to the public.

Statement of Contributions

Chapter 1 is sole authored. Chapter 2 is co-authored with Professor Anindya Sen. Professor Sen was responsible for the original idea of the paper. I was responsible for collecting the data, the development of the empirical methodology, the data analysis, and writing the version of the paper that appears within this thesis. Finally, Chapter 3 is co-authored with Professor Mikal Skuterud and Professor Tammy Schirle of Wilfrid Laurier University. I was responsible for collecting, preparing, and analyzing the data. The chapter that appears in this thesis pulls together two separate articles, which are forthcoming in Industrial Relations and an edited volume on income inequality entitled "Income Inequality: The Canadian Story" that will be published by the Institute for Research in Public Policy in 2016.

Abstract

This three-chapter thesis evaluates the potential for two major government policy levers to influence income inequality in Canada: the tax and transfer system, and the labour relations framework. The first two chapters are concerned with estimating how tax-filers respond to changes in tax rates, and the extent to which governments are limited in raising income tax rates on higher income individuals to fund transfers to lower income individuals. The final chapter examines the possibility that governments can increase the bargaining power of labour unions through changes in labour legislation, and in turn, reduce wage inequality within the labour market.

The elasticity of taxable income measures the degree of responsiveness of the tax base to changes in marginal tax rates. Recent Canadian estimates of this elasticity have found moderate elasticities for earners in the top decile, and high elasticities for earners in the top percentile (for example Milligan and Smart (2015) and Department of Finance (2010)). In Chapter 1, I explore the underlying mechanisms that generate the relatively higher estimates at the top of the income distribution. Using the Longitudinal Administrative Databank (LAD), I estimate elasticities for several sub-components of taxable income, such as earned employment income and total income. In contrast to other research, I find modest elasticities of taxable income, even within the top percentile. I demonstrate that elasticities estimated using the Gruber and Saez (2002) specification are sensitive to choices of weights.

In Chapter 1, I find small elasticities not only for total and taxable income, but also for another very important income concept: employment income. Specifically, I find employment income elasticites of less than 0.07 for all income deciles. These elasticities, however, represent average estimates for heterogeneous workers who face different constraints and who have different incentives to respond to changes in tax rates. In Chapter 2, therefore, I estimate elasticities for different types of workers by dividing the sample by gender and by attachment to the labour force. Using the Survey of Labour and Income Dynamics (SLID), a survey with detailed information on labour hours and job characteristics, I find higher elasticities for female workers and for workers with a weaker attachment to the labour force. I test for robustness of the estimates by varying the income increment used to calculate the marginal effective tax rates (METRs), as well as varying the number of years between observations. A second-order benefit of Chapter 2 is it serves as a robustness check on the results of Chapter 1. That is, we reproduce the elasticity estimates for total income and taxable income from Chapter 1 with a different dataset, and find similar results.

Chapter 3 turns to the potential role of labour relations reforms to influence Canadian income inequality. Labour relations policy in Canada, studied extensively for its impact on unions, has not been studied more generally for its role in income inequality. In this chapter, I provide evidence on the distributional effects of labour relations' reforms by relating an index of the favorableness to unions of Canadian provincial labour relations laws to changes in industry-, occupation-, education-, and gender-specific provincial unionization rates between 1981 and 2012. The results suggest that shifting every province's 2012 legal regime to the most union-favorable possible (a counterfactual environment) would raise the national union density by no more than 8 percentage points in the steady state. I also project the change in union density rates that would result in the counterfactual situation for several demographic subgroups of the labour force. While there is some evidence of larger gains among blue-collar workers, the differences across these groups are small and in some cases suggest even larger gains among more highly educated workers. The results suggest reforms to labour relations laws would not significantly reduce labour market inequality in Canada.

Acknowledgments

This dissertation is the product of over four years immersing myself in the worlds of Canadian labour relations and income tax policy. I am very grateful to several people who have made this work possible. I first thank my supervisor, Professor Mikal Skuterud, who encouraged me throughout this process to explore new, challenging ideas. He allowed me the flexibility to pursue my own avenues, and refocused my attention when I was not making progress. I will take away several lessons from my experiences working with him, but three stand out. First, he has taught me the importance of formalizing my arguments and convincing myself of my results before I try to convince others. Second, that writing a paper in economics is not just about tables of results. There are many ways in which a convincing paper can be written on a given topic; and it that sense it is an art as much as a (social) science. Third, research is a job. Although there are no requirements to work business hours while doing research, putting myself into a daily routine has allowed me to measure my progress throughout this process on a weekly basis.

I am also grateful to Professor John Burbidge. I really became interested in the idea of studying taxation issues while taking a graduate class with him on tax policy. He is very knowledgeable in the history of Canadian income taxation and many of its associated institutional details. We had many very good conversations about the progress of my research and how it relates to what we already know from the literature. I particularly liked how he encouraged me to seek out puzzles and contradictions while completing my research. Rather than run away or avoid such inconveniences I came to appreciate that seeking out these problems is one of the best parts of doing research.

I would like to thank Professor Anindya Sen for inviting me to work with him on his research in Canadian taxation issues. I credit him with coming up with the idea to use the Survey of Labour and Income Dynamics as a data source for estimating tax elasticities in Canada. Professor Sen gave me the opportunity to complete much of my early work on personal income tax elasticities while taking a graduate class with him on public economics. It was also thanks to Professor Sen's encouragement that I decided to pursue a PhD at Waterloo.

The first chapter of my thesis is the product of a unique opportunity I had to work with administrative data at Statistics Canada in Ottawa. I thank Brian Murphy and Professor Michael Wolfson, of Statistics Canada and the University of Ottawa respectively, for inviting me to be part of research projects using new linkages of personal and corporate taxation data. Brian is a very accommodating host and I value my time working with such a knowledgeable colleague during the more than 25 weeks I travelled to Ottawa. Professor Wolfson has been a pleasure to work with as a co-author for our research on tax planning using Canadian Controlled Private Corporations. I learned a lot from him while conducting our research, particularly how to identify interesting research questions. My travel to Ottawa was funded entirely by a SSHRC grant held by Professor Wolfson and his co-applicants.

Conducting research in tax policy requires a detailed understanding on the institutional details of a country's tax system. Early on in my research I identified that I needed to invest in my understanding of these details. I am very thankful to Professor Alan Macnaughton from the School of Accounting and Finance at Waterloo for the two tax classes I took with him. More importantly, however, I appreciate him reaching out to me regularly to encourage my participation at tax conferences, and for introducing me to a number of people in the tax community in Canada.

I am very fortunate that I had the opportunity early on in my second year of studies to work with Professor Tammy Schirle of Wilfrid Laurier University. Tammy, who has a very good knowledge of Canadian public policy issues, spent many hours helping me work through the details of computing union density rates, estimating various counterfactuals, and tackling econometric puzzles. Tammy is a strong

Canadian tax policy researcher, and her comments on the other two chapters of this thesis proved to be very helpful. Having Wilfrid Laurier University nearby presents an excellent opportunity for Waterloo's graduate students to learn from other accomplished economic researchers, and I am very encouraged that collaboration between our two departments continues to grow.

I would like to thank Pat Shaw for outstanding work as the Administrative Coordinator for our PhD program. Pat was always available to help all of us students get the resources and information that we required while completing our studies.

Finally, I would like to thank my wife, Shannon, for encouraging me to undertake my PhD studies and for supporting me throughout the process. I truly believe that I would not have been able to work through the challenges of completing a thesis and stay on course without her help.

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Dissertation Introduction

The Great Recession of 2008 generated a renewed attention on income inequality issues within the United States and other advanced economies. Most notably, discontent with the status quo manifested itself through various "Occupy" movements aimed at highlighting the relative incomes of the top one percent of earners.

Any debate, however, about the "right" level of inequality in the United States should start with research characterizing the level of (and trends in) inequality in that country. There are a number of papers that have thoroughly documented trends in inequality leading up to and following the Great Recession. Atkinson, Piketty, and Saez (2011) document how the share of national income going to the highest income earners (e.g. top 10%, top 1%) has followed a U-shaped pattern in the U.S. over the last one hundred years. In particular, income inequality was high in the 1920's, decreased following the Great Depression, and remained relatively stable until the 1980s when it began to rise sharply leading up to 2008.

Saez and Veall (2005) do a similar exercise for Canada, characterizing the share of national income going to the highest income earners over the 20th century. The authors include comparisons to the U.S. for a number of inequality measures. While income inequality in Canada also followed a U-shaped pattern over the last century, the increases since the 1980's are milder in Canada than in the U.S. For example, in 2000 the top 0.01% of earners in the U.S. earned over 3.0% of national income; in Canada this figure was about 1.9%. By Canada's own standards, however, the authors show that the 1.9% value is quadruple its value from 1978.

Looking forward, it is natural to ask what governments could do to slow the recent increase in inequality or even reverse it, should they desire to do so. With respect to Canada, Fortin et al. (2012) suggest a number of policy 'levers' available at both the provincial and federal levels for influencing income inequality. The policy levers on which the authors focus are: taxes and transfers, education, minimum wages, and labour relations laws. The authors point out, however, that a number of key gaps still exist in our understanding of the potential for these policy options to influence inequality in Canada. This dissertation attempts to fill some of these gaps in the Canadian research by providing evidence on potential for two of the policy options identified in Fortin et al. (2012): taxes and transfers, and labour relations laws.

The first and second chapters of this thesis explore the role of the tax and transfer system in the inequality debate, arguably the most direct lever for influencing inequality. For example, suppose a government wanted to tax high income citizens to fund transfers to lower income citizens. The government must keep in mind that as it raises tax rates on (or reduces tax credits primarily used by) high income earners, these tax-filers may increase their effort to reduce their taxable income. It is conceivable that if rates are raised on high income earners, tax revenues could actually fall. For example, the government of Quebec raised (federal plus provincial) rates on its highest earners from 48.2 in 2012 to 49.9 in 2013. Between these two years, the number of Quebec tax-filers within the top one percent of the national income distribution fell from 43,360 to 40,825.[1] If this sharp drop in high income filers were due to the tax hike, this would imply a 5.8% drop in the number of tax-filers (and their associated incomes) due to a 3.5% tax increase. It is certainly possible that this tax hike, depending on the incomes of these lost tax-filers, would result in a *decrease* in government revenues. In other words, the Quebec personal income tax base would be "on the wrong side of the Laffer curve."

[1] Source: CANSIM table 204-0001 published annually by Statistics Canada.

Given that this responsiveness to tax reform is important for projecting government revenues, many researchers have attempted to estimate the value of the response in terms of a simple economic statistic: the elasticity of taxable income. This value measures the percentage change in taxable income for a given percentage change in the marginal tax rate, τ (or alternatively, for a percentage change in the net-of-tax rate, $1-\tau$). If the elasticity is high, governments are limited in their ability to raise additional revenue through income taxation. For countries like the U.S. that collect trillions of dollars in personal income taxes, small increases in the value of this elasticity would imply tens of billions of dollars in lost revenue. Unsurprisingly, therefore, a number of researchers have estimated the value of this key parameter for the U.S. personal income tax system.

The number of attempts to estimate this parameter for the Canadian personal income tax system, however, has been few. This is a problem for Canadian policy-making because we should expect the elasticity to vary across countries as each country has its own taxation system and associated opportunities for tax-filer response. Estimates of the U.S. elasticity, therefore, are of limited use to Canadian policymakers. Clearly then, having some confidence in the value of the taxable income elasticity in Canada is important for fiscal policy design. One way to gain this confidence is to check the robustness of existing Canadian estimates to different data sources, tax reform events, identification strategies, and empirical methods. The need for additional research on the elasticity of taxable income in Canada is one of the main arguments in both Bird and Smart (2001) and Milligan (2011). In the spirit of the need for further Canadian research, the goal of Chapter 1 and Chapter 2 of this thesis is to challenge our existing estimates of the elasticity of taxable income in Canada by introducing new data and methods.

In Chapter 1, I estimate elasticities for four definitions of income: of employment, total, net, and taxable income. The tax-on-income (TONI) reform implemented by all provinces except Quebec in 2000-2001 serves as a unique opportunity to estimate elasticities in Canada using a quasi-experimental identification strategy as it allows comparison of observably similar tax-filers who received large tax cuts in Western Canada with those in Eastern Canada who received relatively smaller tax cuts. Specifically, I cut the sample into ten deciles based on the national income distribution, and estimate elasticities within each of these deciles. For a data source I use Statistics Canada's Longitudinal Administrative Databank (LAD). Although the literature has often found large elasticities for high income individuals, within the top decile I do not find elasticities significantly different from zero for all four definitions of income. If I restrict the amount of sample in the right tail of the income distribution to the top 5% or top 1% of earners, I continue to find insignificant elasticities.

The estimates from Chapter 1, while useful for understanding the responsiveness of individual tax-filers on average, do not tell us much about the potential for heterogeneity of responses among different types of workers. For example, the pooled sample used to estimate the elasticities in Chapter 1 includes full-time permanent employees, such as public sector workers, who have few incentives and opportunities to adjust behaviour in response to tax reform. As is often the case in economics, however, many of the interesting responses happen on the margin, among particular subgroups of the population. In Chapter 2, I divide the sample of employed workers according to gender and job characteristics and find evidence of higher elasticities among women with a weak attachment to the labour force. As married women with working spouses traditionally have had a weak attachment to the labour force (for example, see Keane (2011, p. 1045), these results are consistent with the results in Eissa (1995) which found relatively high elasticities for married women for the U.S. tax reforms of the 1980s. Note that I use the Survey of Labour and Income Dynamics (SLID) for this study, as it contains rich detail on job characteristics that is not available in the LAD.

Finally, Chapter 3 of this thesis is also concerned with identifying differential responses to policy among sub-groups of the working population in Canada. As discussed above, however, in Chapter 3 I move away from the role of taxation in policy-making and look at the role of labour relations laws for influencing inequality in Canada. Labour relations laws dictate the rules of interaction between employers and the unions that represent their employees. Unions tend to reduce wage inequality by, among other things, raising wages for unskilled workers. It is plausible, therefore, that adjusting labour relations laws to tilt the balance of bargaining power in favour of unions would reduce wage inequality in Canada. This form of government-initiated income redistribution is less "direct" than the tax-and-transfer system because it occurs through the collective bargaining process. Politically, changes to labour relations laws are relatively obscure, and are much less likely to make headline news in comparison to changes in headline statutory marginal tax rates, such as the federal increase in the top marginal tax rate from 29% to 33% that occurred in late 2015.

To see if there is evidence of union-friendly labour relations laws impacting wage inequality, I use a two-step procedure. First, I estimate the effect that changes in a set of twelve provincial labour relations laws would have on the long-run unionization rate of several well-defined subgroups of the labour force in Canada. Second, I construct a counterfactual wage distribution that would result if each of these subgroups were to be paid the prevailing wage premium that is associated with unionization. It turns out that many of the types of workers who would benefit most from changes in labour relations legislation already have relatively high wages, and it is therefore unlikely that these legal changes would reduce wage inequality.

The evaluation of public policy options for influencing inequality in Canada, namely tax and labour relations reforms, is the common thread tying together this thesis. I provide evidence that although governments may have additional room to redistribute income using taxes and transfers, they are likely limited in doing so through the use of labour relations laws. Conducting policy evaluation of the kind done within this thesis certainly benefits from the unique subnational variation that exists in Canada. The similarity of both tax and labour relations legal frameworks across most Canadian provinces, coupled with provincial legislative authority to unilaterally change laws, permits a quasi-experimental identification strategy of the kind used in all three chapters of this thesis, assuming one accepts that residents of Canada are sufficiently similar from coast to coast. I hope that this thesis serves as evidence of the policy insights that can arise from reliable, national data sources suitable for economic research.

Chapter 1. Estimating Elasticities of Taxable Income: Canadian Evidence from the Tax on Income (TONI) reform of 2000/2001[1]

1 Introduction

In December of 2015, the newly-elected majority Government of Canada introduced Bill C-2 in the House of Commons, proposing to increase the marginal tax rate on annual incomes greater than $200,000 from 29% to 33% for the 2016 tax year.[2] This federal tax increase on high earners follows several similar reforms implemented by provincial governments since 2010 in: Nova Scotia, New Brunswick, Quebec, Ontario, Alberta (abandoning its flat tax), and British Columbia (see Milligan and Smart (2016) for all effective increases). For example, for the 2014 tax year, Ontario introduced a fifth tax bracket for those earning between $150,000 and $220,000 per year, and also lowered the threshold for the top tax bracket from $509,000 to $220,000. This reform had the effect of increasing the top tax rate by two percentage points on those earning just over $220,000 in 2013.[3]As many Canadian provinces struggle with budget deficits and increasing inequality, increasing tax rates on top earners is an attractive policy as it is more politically feasible than increasing tax rates on the middle class.

Raising the statutory marginal tax rates on top earners, however, does not guarantee a substantial increase in government revenues. Tax-filers can respond to the higher rates by working less or engaging in tax avoidance strategies to reduce taxable income, which shrinks the size of the tax base subject to the higher rates.[4] The net effect can lead to realized tax revenues that are only a small fraction of what would be the case without tax-filer response. The deadweight loss that results from income taxation is a further economic cost of raising tax rates on these tax-filers. Ultimately then, to understand the potential for provincial governments to raise taxes, we need to estimate how elastic are the incomes of their highest-earning residents. Milligan and Smart (2016), using income elasticities they estimate for the Canadian provinces, generate counterfactual government revenues that would prevail if each province were to increase its top marginal tax rate by 5%. They find that high elasticities would limit several provinces from raising significant additional revenues; that is, there is an effective upper bound on how much taxes can be raised. This suggests some provinces may be approaching the peak of the "Laffer Curve" for their high income earners, and have less room to manoeuvre than others.[5]

The result in Milligan and Smart (2016), of relatively high elasticities of top earners, is consistent with previous Canadian research (see Sillamaa and Veall (2001), Gagne et al. (2004), as well as with research

[1] The author wishes to acknowledge Brian Murphy for providing all necessary support on site at Statistics Canada headquarters in Ottawa, Ontario, and Paul Roberts and Hung Pham for critical technical assistance with the LAD. This research is partially funded by the 2012 SSHRC grant to Michael Wolfson, Michael Veall and Neil Brooks: "Incomes of the affluent: the role of private corporations".

[2] See Bill C-2 (2015) in Bibliography. This reform was included in the Liberal campaign platform in the fall of 2015. See Liberal Party of Canada (2000).

[3] Note, the above references to marginal tax rates exclude surtaxes and the Ontario Health Premium. They simply refer to the headline statutory rates applied to Line 260 taxable income.

[4] Piketty and Saez (2012) model the net revenue effect of any increase in MTR as the sum of the mechanical effect (the change in the tax revenue that would result if there were no behavioural response), and the behavioural effect, which accounts for the decrease in the tax base (conceptually) following the mechanical effect.

[5] Milligan and Smart (2016) Figure 6 shows the "net revenue effect" (see supra footnote 4) that would result from a 5 percentage point increase on top earners. Alberta has the most flexibility to raise rates, PEI the least. This flexibility is not monotonically decreasing in the top marginal tax rate.

from other countries. Researchers studying the U.S., UK, and France have all found relatively high elasticities on top earners (see Table 3C.7 in Meghir and Phillips (2010) or Chart 1 in Department of Finance (2010) for a summary by country).[6]

While it is attractive to summarize all of the income response of the top earners in the form of a single reduced-form statistic, namely the elasticity of taxable income, the cost of this reduced-form analysis is less insight into the data process generating that statistic. This is problematic because the elasticity is not a structural parameter; rather, it is the aggregate net effect of several possible responses.[7] Slemrod (2001) argues that legal responses to taxation can be categorized as one of either: real responses, or avoidance responses. He defines the former as responses in which the changes in relative prices caused by changes in taxes cause individuals to choose a different consumption bundle. The latter is defined as the activities that tax-filers engage in to reduce their tax liability without altering their consumption bundle. He argues that these two main categories can be further subdivided, and that we can think about all of the possible responses in terms of a tax elasticity "hierarchy".

Understanding the relative importance of each response within such a hierarchical concept can be used to inform better tax policy. For example, consider the potential tax-filer response to a ten percent increase in marginal tax rates. If the response is a real drop in labour supply, the result is increased deadweight loss and (potentially) increased government transfer payments. If the response is mostly due to one-time avoidance responses, such as owners of private businesses issuing above-average amounts of dividends from accumulated retained earnings before the tax hike, the real impacts to the economy would be relatively minimal.[8] Therefore, a relevant policy question is: how much of the observed elasticity on high earners is due to such avoidance responses (tax planning responses), including re-timing of income?[9] Since timing responses cannot be repeated annually, if they account for the majority of the estimated elasticity, then provincial governments may be less constrained in raising the top rates than is suggested by the elasticities estimated in Milligan and Smart (2016).

In this paper, I use a large administrative tax dataset – the Longitudinal Administrative Databank (LAD) – to explore in more detail the nature of the elasticity of taxable income in Canada. The LAD is a 20% random sample of the Canadian tax-filing population, which contains variables for over a hundred of the most commonly-used line items on the T1 General form, its associated schedules, and provincial tax forms.[10] Such a large and detailed dataset contains the disaggregated detail required in order to generate

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[6] There is no *a priori* reason to believe that the magnitudes of estimated elasticities should be comparable across countries; each has its own tax legislation and industrial landscape which affect the constraints and income-earning opportunities respectively of all tax-filers. Also, two countries may have very similar elasticity values for very different reasons. What is notable is the persistence of the within-country result, whatever the tax system, that high income tax-filers have higher elasticities than lower income filers.

[7] See Slemrod (1996) for more discussion and an early attempt to decompose the aggregate elasticity into finer margins. Characterizing all of these responses is also sometimes referred to as the "anatomy" of the response. For a thorough review of the state of the taxable income elasticity literature, see Saez et al. (2012).

[8] Roughly 80% of dividend income earned in Canada within the top decile comes from private corporations. I calculated this value by dividing total "other than eligible" net dividends by total net dividends received in 1999 using T5 data at Statistics Canada. As pointed out by Bauer et al. (2015) this value is a lower bound (and proxy) for private dividends because private companies can issue eligible dividends. They find a value of 79.1% over the period 2006-2009 using public data. Many of the individuals in the top decile own majority positions of these corporations and have full control over dividend timing.

[9] The idea that elasticities can be mostly composed of re-timing responses is not new; Slemrod (1995) argues re-timing is the most responsive among the set of behavioural responses. Goolsbee (2000b) finds that 95% of the elasticity among corporate executives is due to re-timing.

[10] Quebec is the exception as Revenu Quebec does not send its provincial administrative tax records to Statistics Canada.

accurate marginal effective tax rates (METRs) in a tax calculator. Accuracy of the METR is important as missing inputs, such as RRSP deductions, can generate significant measurement error in the actual METR of the tax-filer. With the detailed line-item information, I can generate customized definitions of taxable income, such as a version of taxable income in which capital losses and the lifetime capital gains exemption are excluded. Having the ability to make such adjustments is important given that tax-filers can re-time realizations of capital gains income.

As a source of variation in taxes, I use unilateral cuts in statutory marginal tax rates implemented by most provinces upon implementing the “tax on income” (TONI) reform between 2000 and 2001.[11] This reform granted provinces the discretion to set their own schedule of tax brackets and rates; western Canadian provinces in particular made significant cuts in marginal tax rates at this time. This subnational variation offers a unique opportunity to identify income elasticities using an “experimentalist” identification strategy;[12] namely, by comparing the responses of tax-filers in provinces that made relatively large cuts with observably similar tax-filers in other provinces.

In my baseline specification, I estimate an elasticity of about 0.03 for both taxable and total income. Compared to other Canadian, U.S., and European studies, this value is quite low. Restricting the sample to income earners between the 90thand 99th percentiles, I continue to find a taxable income elasticity of 0.03, but find a higher total income elasticity of about 0.13. This total income elasticity is still low, but approaches other estimates for the top decile from the Canadian literature on the TONI reform.[13]

Within the top decile, when I progressively increase the lower bound on the sample (estimating elasticities for the top 10, top 9, top 8, etc.), I continue to find relatively low elasticities and do not find evidence that elasticities rise with income. If we expect high income tax-filers to increase tax planning efforts as taxes increase, this result is surprising. I argue in this paper that this result may be explained by the fact that I am estimating elasticities using a reform that implements tax cuts and not tax increases. A *high* observed elasticity during a period of tax cuts would require a *reduction* in tax planning efforts in response to these cuts. Given that there are typically high fixed costs of setting up (and taking down) tax planning strategies and low variable costs of maintaining them, there is reason to be skeptical that high income filers would do less tax planning on the margin as tax rates fall. This suggests that tax-filers’ overall responses to tax cuts and hikes are unlikely to be symmetric, even if real responses to tax changes, in terms of changes in labour hours, are symmetric.[14]

The remainder of this paper is organized as follows. The following section describes the relevant aspects of the TONI reform; the third section describes the LAD data; the fourth discusses my empirical approach; and the fifth section presents the results. The final section concludes and interprets the results as they relate to tax reform policy and provides some suggestions for future work.

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[11] Quebec did not undergo this reform; it collects its own taxes.
[12] See Chetty (2009) for a contrast of the experimentalist approach vs. structural in the context of taxation research.
[13] For example, while Milligan and Smart (2015) estimate a total income elasticity of 0.42 for the top 10% overall, their estimate for those between the 95th and 99th percentile is only 0.10, and -0.03 for the 90th to 95th. They present strong evidence that most of the elasticities they find are driven by the top 1%.
[14] There have been very few notable tax increases on high income earners in Canada (except very recently) and the U.S. over the past 40 years, and therefore minimum opportunity to see if elasticities are greater when identified off of increases. One exception is the Clinton tax increases of 1993. Goolsbee (2000b) estimates elasticities for corporate executives over this period and finds very large short-term re-timing reductions in taxable income (elasticity greater than 1.0), but little response over longer periods of time.

2 Income Tax Reforms in Canada

2.1 "Tax on Taxable Income" Reforms in 2000 and 2001

At the turn of the century, there was a major reform in the calculation of provincial taxes (with the exception of Quebec).[15] Before the reform, the system was known as a "tax-on-tax" (TOT) system because the provincial tax base was based on the amount of federal tax calculated. For example, Ontario tax-filers filled out Federal Schedule 1, applied the progressive tax rates to their income, subtracted non-refundable credits, and computed their federal tax amount. They would then multiply this amount by a provincial tax rate of 39.5% as well as a number of additional surtaxes, as applicable. The reform changed provincial taxation to a "tax on taxable income" (TONI) system in which each province's tax base became a function of federal *taxable income*; thus, the provincial tax base was no longer explicitly a function of federally set statutory marginal tax rates (MTRs).[16] Rather than make use of surtaxes, the provinces introduced their own set of progressive tax rates to apply on taxable income.[17]Nova Scotia, New Brunswick, Ontario, Manitoba, and British Columbia implemented the TONI reform in 2000; followed by Newfoundland, Prince Edward Island, Saskatchewan and Alberta in 2001 (see Table 1 for a summary).[18] Also in 2001, the federal government added an additional tax bracket, resulting in tax-filers with taxable income between approximately $60,000 and $100,000 facing a lower MTR.[19] Thus, for filers living in the provinces that implemented the TONI reform in 2001, there were some significant single-year cuts in the federal-provincial combined MTR (6.6 percentage points for BC tax-filers in the highest tax bracket in 2000).[20]

In theory, the switch from TOT to TONI need not have changed the total (federal plus provincial) MTR paid by tax-filers; indeed, in some cases it did not.[21] However, most provinces took advantage of the increased fiscal independence by making at least some minor tax cuts. Most notably, Alberta switched to a single-rate MTR, or a "flat tax," in the same year it implemented TONI (see McMillan (2000) for more); Saskatchewan continued to make MTR cuts in 2002 and 2003, in addition to going through the TONI reform in 2001; and, Newfoundland made cuts to MTRs in 2000, a year before it implemented TONI.

In some provinces, such as Nova Scotia and PEI, "bracket creep" counteracted the effect of the tax cuts for tax-filer near bracket thresholds, or kink points. Bracket creep, described extensively in Saez (2003), is a term used to describe situations in which tax-filers who have no change in real income move into a

[15] See LeBlanc (2004) for a detailed summary of the reform, and Hale (2000) for a discussion of the pre-reform planning.
[16] Implicitly, due to behavioural response, provincial revenues are still sensitive to federal statutory tax rate changes.
[17]Alberta introduced a flat tax of 10% which is not progressive, but this was levied on taxable income, and was therefore no longer a surtax.
[18] Quebec had been administering its own collection of income tax since the 1950's (see LeBlanc (2004), and was the only province not to go through this transition. Yukon, Northwest Territories and Nunavut transitioned in 2001, but are not studied in this paper.
[19]Determined by consulting federal Schedule 1 for years 1999 through 2001.
[20] See Department of Finance (2010) Table A2.1 for a summary of the changes over this period for top marginal tax rates. In BC, the combined federal-provincial top marginal tax rate in 1998 was 54.2%; by 2002 it was 43.7%.
[21] Here is a very simple example. Assume an Ontario tax-filer has a taxable income of $\$x$ in 1999. If $x>\$120{,}000$ and she had no non-refundable credits, she would be in the top federal tax bracket with an MTR of 29%, and therefore have $\$(0.29)x$ in federal tax. She would have $\$(0.395)(0.29)x = \$(0.1146)x$ in Ontario tax upon applying the 39.5% provincial tax-on-tax rate. Under the TONI system implemented in 2000 in which Ontario could now apply its tax rates directly on taxable income, x, Ontario could have simply left the top rate at 11.46% to maintain neutrality of the provincial MTR. Ontario chose to set it at 11.16%.

higher marginal tax bracket due to non- or under-indexation of the tax bracket thresholds. Table 1 summarizes provincial tax bracket indexation statuses of all provinces and the federal government over the sample period.[22] The implication of un-indexed provincial tax brackets for interpreting the results in this study is as follows. A tax-filer sitting just below a kink point would experience a drop in their tax rate when tax cuts were implemented, but a small increase in their nominal income would then push them back into their original (higher) tax bracket. While this would have very little impact on their tax payable or average tax rate, it does create a technical annoyance for interpreting elasticities since I assume that tax-filers react to changes in their METR, whether the change was generated by reform or by bracket creep. Canada had relatively low inflation in the early 2000s, however, so the effect of bracket creep on the results in this paper is likely to be modest.

Although minor in any given year in some provinces, the effect of unilateral provincial rate cuts at the same time as or immediately following the TONI reform resulted in some significant *cumulative* cuts in MTRs by the end of 2002. This period represents the most significant cuts to MTRs that Canadian tax-filers have experienced since the federal tax reform that took place in 1988.

2.2 Timing and Importance

With the exception of BC, all other provinces announced tax cuts well in advance of their implementation (see Table 2 for a summary). This timing is important because if a tax-filer were to delay income or "re-time" income around the TONI reform, she would require advanced notice to plan income realizations accordingly. Given that BC made its announcement of tax cuts within-year, or "ex post", many income re-timing opportunities for tax-filers in that province would be unavailable, and any responses that occurred in this province, therefore, would most likely be due to real behavioural responses such as increased hours of work.[23]

The saliency of the tax reforms are also important if we expect to observe tax-filer response through behaviour or re-timing of income.[24] The more widely publicized are the reforms, the more likely are tax-filers to optimize in response to the new information. Thinking about the provinces that made significant tax cuts around the time of the TONI reform, the tax cuts implemented in B.C. were a campaign promise of the Liberals; those in Alberta, including the well-publicized introduction of a flat tax, were announced in Budget 2000 as recommended by the Alberta Tax Review Committee; and finally, those in Saskatchewan and Newfoundland were both announced in their spring 2000 budgets.[25] The reforms in the four provinces that made the most substantial cuts, therefore, should have been covered adequately in the media, and should have been known to the tax-filing population.

[22] Bracket creep was originally introduced by federal Finance Minister Michael Wilson in 1985 as a way of increasing tax revenues without increasing tax rates. Leslie (1986) notes that this type of tax policy is sometimes referred to as the "silent tax". Federally, bracket creep was not an issue in this study because bracket indexation was restored in 2000.

[23] Sophisticated tax planning arrangements that allow a tax-filer to adjust returns of previous years, to the extent they exist, are beyond the scope of this paper (and also beyond the scope of the data because LAD records are not refreshed when CRA records are updated).

[24] An example of non-salient changes in tax rates is the bracket creep concept discussed in the last section. This phenomenon was the subject of the Saez (2003) paper. The advantage of this type of variation – notwithstanding the lack of saliency – is the treatment is applied, and not applied, to individuals with very similar incomes all along the income distribution.

[25] Relevant references in: Kesselman (2002), McMillan (2000), Alberta Treasury Board (2000), Saskatchewan Department of Finance (2000), Newfoundland and Labrador (2000).

I assume throughout this paper that optimizing tax-filers are only concerned with their marginal effective tax rate (METR), regardless of the source of the variation in that rate. That is, they do not care if a change in their METR is due to federal tax reform or provincial tax reform. Furthermore, they do not care if their marginal income is reduced due to a claw-back of a means-tested benefit, or due to the application of a statutory marginal tax rate to their taxable income.[26] Of course, it could be argued that tax-filers respond to federal vs. provincial variation in METR differently, but to estimate this I would have challenges identifying the federal elasticity estimate. Specifically, the primary source of federal tax reform over the TONI period is due to the addition of a tax bracket for those earning between $61,509 and $100,000, and the elimination of the federal surtax, both taking place in 2001. The problem with estimating an elasticity due to a federal reform, in general, is that tax-filers in all provinces receive the same federal "treatment". In order to generate enough variation in the data, I would be forced to compare those with low income and high income, which is precisely what I am trying to avoid in this paper by taking advantage of the subnational variation offered by the provincial reforms.

3 Data

I use the Longitudinal Administrative Databank (LAD), a longitudinal panel representing 20% of the Canadian tax-filing population running from 1982 to the present. The LAD is a randomly-sampled subset of the T1 Family File (T1FF), which is the population file of tax-filers provided by the Canada Revenue Agency to Statistics Canada annually.[27] Note that although the LAD is derived from a "family file", it is a random sample of individuals, not families. Once an individual tax-filer is sampled for the LAD, this tax-filer is sampled annually to maintain the longitudinal nature of the data. As the tax-filing population grows, more T1FF records are randomly sampled to maintain 20% coverage.[28] The LAD augments the raw T1FF data with a number of derived variables such as the ages of children, industry of employment, and the structure of families by using Social Insurance Numbers (SINs) and mailing addresses to merge the T1FF with other administrative datasets.[29] In addition, because the LAD is used by researchers to study public policy issues, it is subject to quality and consistency checks beyond those performed on the raw T1FF data. My baseline specification uses the years 1999 to 2004 to cover the period of the TONI reform. The LAD contains 4.5 million observations in 1999, growing along with the tax-filing population to 4.8 million in 2004.

The primary independent variable of interest in this paper, the METR, is not an administrative data concept and must be derived through simulation. This is because METRs are generated by considering the "general equilibrium" effect of a change in income on tax payable, while MTRs are simply fixed rates applied on that income that ignores other elements of the tax system that are affected by the marginal change in income. To simulate the METR, I calculate individual income tax payable, then add a small

[26] That tax-filers only care about the "bottom line" METR is a standard assumption in the tax literature. Of course it is possible that tax-filers suffer from "tax illusion". In the retail sales tax setting, Chetty et al. (2009) show that consumers respond differentially to a price depending on whether the tax is more or less visible, for the same net price.
[27] For more detail, see Statistics Canada (2012).
[28] The tax-filing population grows not only due to population growth but also due to increases in the percentage of filers, which may be due to increased incentives to file such as eligibility of the Canada Child Tax Benefit. If individuals stop filing taxes for whatever reason, such as leaving the country permanently or death, new records are sampled from the T1FF to maintain the 20% coverage.
[29] Other administrative datasets include, but are not limited to: the T4 slip file, Child Tax Benefit File and BC Family Allowance Benefits file.

(marginal) amount of employment income, and recalculate individual income tax payable. The ratio of additional taxes paid to the additional labour income represents the METR.[30] To do this simulation, I use the Canadian Tax and Credit Simulator [CTaCS] by Milligan (2012), a program that calculates the tax liability of any tax-filer in any province or territory.[31] METRs can diverge quite substantially from MTRs over some ranges of income, depending on the situation of individual tax-filers. Macnaughton et al. (1998) document 19 tax measures that create this divergence between METRs and MTRs. The biggest one by far is the income testing of the Guaranteed Income Supplement (GIS), which is a reduction of benefit income. This benefit reduction can generate METRs of well above 50%. Another item causing outlier METR values is the medical expense tax credit, which applies based on a threshold test; if income changes marginally across this threshold, METRs in excess of 100% result.[32]

Table 3 summarizes the mean changes in METR by province for four sets of two-year pairs. It is clear from this table that tax cuts were in general greater in the western Canadian provinces. Table 4 shows these mean changes in METR again, specifically for the two year period from 1999 to 2001 in which the majority of tax cuts took place. In this table, however, the sample is cut by the deciles of the income distribution. By looking at these changes within income deciles, it is clear that there are some large differences between provinces within the higher deciles. For example, within the ninth decile the mean percentage point decrease in the METR between 1999 and 2001 in BC was 9.1, while in Nova Scotia it was only 4.8, representing a difference of 4.3 percentage points. Within the tenth decile, the same percentage point difference of 4.3 separates Alberta and Nova Scotia. Differences of this magnitude are not apparent for the lower deciles in the same table, nor are they apparent for the pooled sample shown in Table 3. This is the advantage of cutting the sample into income tranches. It is these large differences in tax cuts among individuals with similar incomes, particularly within the top deciles, that I will use as the primary source of identifying variation to estimate income elasticities.

A phenomenon not shown by the *mean values* of the *changes* in METRs is that there can be substantial heterogeneity in the *level* of METRs among similar tax-filers due to the heterogeneity in lines itemized by tax-filers. Using a box-and-whisker plot, Figure 1 highlights this variation in the levels of METRs across the four major federal tax brackets. There is much more variation between the 25th and 75th percentile within the bottom tax bracket (15% MTR) in comparison with the top bracket (29% MTR) due to the greater number of benefits and their associated claw-backs facing the former group.

Concentrating on tax-filers within the top decile, where this variability is lower, Figure 2 presents a similar box-and-whisker plot, except the comparison is between provincial distributions. The figure reveals a fact about the TONI reform that is not picked up by the mean changes in METRs listed in Table 4; namely, that the pre-reform variability in METRs was very small, but then increased greatly following the reform. This phenomenon is explained by the increased provincial autonomy to set tax legislation following TONI.

[30] I use a $100 marginal increment instead of $1 to avoid issues such as rounding within the tax calculator. Note that unlike Chapter 2 where I use the change in *spousal* tax payable, I am forced to use the change in *individual* tax payable because the LAD, unlike the SLID, does not contain tax variables for both spouses.

[31] Program developed by Kevin Milligan: available at http://faculty.arts.ubc.ca/kmilligan/ctacs/. See Table 5 for details of variables used in this analysis.

[32] Such extreme values show up in the CTaCS simulations and I drop these observations as they represent a non-trivial departure of the data from the theory underpinning the econometric specification. See Table 11 for sample implications.

As discussed above, over some ranges of income there can be severe fluctuations in the METR, affecting what would otherwise be relatively smooth progressivity of taxation. To illustrate such income ranges, Figure 3 plots the METR for unmarried Alberta tax-filers with employment income as the only source of earnings in \$100 earnings increments in both 2000 and 2001.[33] To the extent that tax-filers are not informed about their METR to this degree of precision, or think about "marginal income" in a different sense than what is proposed in most models of tax elasticity, these discontinuities may introduce measurement error into the results.[34] In general, the average magnitude of fluctuations tends to decrease as income increases, so these issues will be less relevant for high income tax-filers.

The primary dependent variable of interest for calculating income elasticities is necessarily some measure of income. I estimate the elasticity for the three major definitions of income used for filing taxes in Canada: total income, net income, and taxable income. Estimating elasticities for these three different income definitions informs the degree to which tax-filers respond to taxation through the use of deductions. Specifically, there are two major blocks of deductions within the tax system: one that follows total income and precedes net income, and the other that follows net income and precedes taxable income. If tax-filers adjust deductions in response to the tax reform, these changes would be picked up in net income for the first block, and taxable income for the second block.[35] Due to its importance as the major source of income, I also estimate elasticities for employment income, the definition of income which is the focus of Chapter 2 of this thesis.

4 Empirical Methodology

My empirical approach follows the first-differences specification used in Gruber and Saez (2002). First-differencing removes any time-invariant unobservable characteristics, such as gender.[36] Using six years of the LAD panel from 1999 to 2004, the baseline empirical model (using log ratios instead of subtraction) takes the form:

$$ln\,(I_{i(t)}/\,I_{i(t-1)}) = \beta_0 + \beta_1 ln\,[(1-\tau_{ij(t)})/\,(1-\tau_{ij(t-1)})] + \beta_2 lnI_{i(t-1)} + \beta_{3t}\sum_t Y_t + \beta_4 age_{(t-1)} + \beta_5 age^2_{(t-1)} + \beta_6 self_{(t-1)} + \beta_7 kids_{(t-1)} + \beta_8 married_{(t-1)} + \beta_9 male_{(t-1)} + \sum_k \beta_{10k(t)} IND_k + (\varepsilon_{ij(t)} - \varepsilon_{ij(t-1)}) \quad [1]$$

The subscript i denotes the individual, and j represents the province of residence. I use t to represent the current year and t-1 to represent the previous year. The variable $I_{i(t)}$ represents the income of person i in

[33] Source: author's calculations by increasing employment income in \$100 increments using CTaCS Milligan (2012). Figure 4 plots the *difference* between these two years to show the substantial year-over-year change in METR for tax-filers near discontinuous points.

[34] In other words, we may be incorrectly modelling the data-generating process of tax-filer response. In practice tax-filers may think about "marginal income" in increments of \$5,000 or \$10,000. For tax-filers who respond to taxes through labour market decisions, they may only consider marginal income as the extra income that would be realized in three states of the world: no job, a part-time job, or a full-time job.

[35] In principle, I could estimate elasticities of the aggregate value of these deductions for each tax-filer. This would yield an elasticity of deductions as a whole. Practically, however, there are many tax-filers who claim no deductions, or who only claim union dues which are expected to be non-responsive. Under this approach, I would be estimating elasticities where the majority of the observations have a zero value of the dependent variable, and this would require a substantially different econometric approach.

[36] The reader will notice that gender is in fact included in the specification. This is to control for gender-specific changes in year-over-year income to reflect the fact that labour supply elasticities have been shown to be different between men and women (see Keane (2011). Any true fixed effect for gender disappears in the first-differences specification.

year *t*. The corresponding METR of the individual is represented by $\tau_{ij(t)}$. Therefore $(1-\tau_{ij(t)})$ is a net-of-tax rate.[37] Other independent variables include age, age squared, self-employment status, number of children, marital status, and gender. The term $\sum_t Y_t$ represents a set of year dummies for all year-pairs in the first-difference (equal to 1 in year *t*) which mitigate the potentially confounding effects of macroeconomic shocks that are common to all provinces at a single point in time, such as the well-known stock market crash over the period of study. I also include a set of industry dummy variables to capture year-over-year industry trends in average incomes. For example, primary industry can produce sharp changes in income over short periods due to changing commodity prices. This industry is located primarily in Western Canada where tax cuts were greatest; without this control, therefore, $(1-\tau_{ij(t)})$ would be correlated with $\varepsilon_{ij(t)}$. Table 6 provides summary statistics for several of the covariates in [1] above.

The error term is given by $(\varepsilon_{ij(t)}-\varepsilon_{ij(t-1)})$ and clustered at the province level.[38] The advantage of the Gruber-Saez approach over other specifications, such as panel models with fixed-effects, is it requires weaker assumptions on the error term for the estimator to be consistent. Specifically, if I assume the error term does not follow a moving-average process – that is, $\varepsilon_{ij(t-1)}$ has no history and always starts in a steady-state – then, the first-differenced error term is only correlated with the model's *current-year* independent variables via $\tau_{ij(t-1)}$ since shocks to income in year *t-1* push up the METR in that year. Although not stated, the implicit assumption in the Gruber-Saez model, therefore, is that $\varepsilon_{ij(t-1)}$ is small, or the model is starting close to a steady-state. In a fixed effects model, however, the error term becomes $(\varepsilon_{ij(t)}-\bar{\varepsilon}_{ij})$, where $\bar{\varepsilon}_{ij}$ is the mean error term within the panel unit, which implies $\tau_{ij(t-1)}$ is correlated with *all past error terms* via the term $\bar{\varepsilon}_{ij}$.[39]

The key dependent and independent variables are represented as natural logarithm ratios, an approximation for percentage changes.[40]As a result of this ln-ln form, $\beta_1$is the (uncompensated) elasticity of income parameter. The first-differences specification implies that all other explanatory variables are included to the extent that they explain *changes* in income rather than the *level* of income.

4.1 Endogeneity and Identification Issues:

Given that Canada has progressive marginal tax rates in which individuals who earn more income will face a higher tax rate, τ_{ijt} is mechanically a function of ε_{ijt} in [1] and therefore endogenous. To address this issue I follow Gruber and Saez (2002) and create a "synthetic tax rate" instrument for τ_{ijt} and estimate [1] by 2SLS. Specifically, the instrument is a counterfactual value of what the τ_{ijt} would be if the tax-filer had no change in real income between year *t-1* and year *t*.[41] This variation in the instrument of τ_{ijt}, therefore, is

[37] The literature generally uses a net-of-tax rate to avoid dealing with the ln() operator when the effective marginal tax rate is zero.
[38] I do not cluster at the tax-filer (individual) level as many tax-filers only satisfy the sample restrictions for one first-differenced year pairing. That is, the panel is not balanced.
[39] For a detailed discussion of the identification issues in this literature, see Moffitt and Willhelm (2000). For discussion of fixed effects versus first-differences models using panel data, see Wooldridge (2010).
[40] ln( ) ratios are suitable proxies for percentage changes (positive or negative) of up to 30%. I restrict most change variables within this range; see Section 4.2 for more.
[41] That is, I inflate the *year t-1* values of all nominal dollar-valued inputs (and the ages of family members) in the tax calculator by province-specific Consumer Price Index values up to the *year t* values (see Table 10 for values). For provinces that index many of the nominal thresholds in their tax forms to this measure of inflation, this should maintain a constant tax burden; for those that do not, or who use some other proxy for inflation, some tax-filers may "creep" into higher tax brackets. Note, that any bracket creep caused by this minor difference in inflation proxies is a separate bracket creep issue from the intentional bracket creep implemented by governments described in Section 2.1 above.

only a function of changes in tax legislation, and rules out responses by construction. This instrument is not correlated with any shocks to income that occur in year *t* because it is *predetermined* by income in year *t-1*.[42]

Upon removing the mechanical relationship between τ_{ijt} and ε_{ijt} that exists in all progressive tax systems, there remain two further potential sources of endogeneity due to omitted variables in the error term. The first potential omitted variable is due to income distribution widening. Given that the TONI reform resulted in *relatively greater* tax cuts for those in the top deciles of the income distribution, if incomes of top decile tax-filers grew *relatively more* over the period 1999 to 2004 *due to non-tax reasons*, the model would attribute the variation to the tax reform due to omitted variable bias. For example, Table 7 shows the time-series of real income in Canada over this period. The mean total income of earners in the top two federal tax brackets increased by a greater percentage than those in the bottom two tax brackets, and METR cuts were greater for the former group.

The distribution-widening issue was of particular concern to many researchers estimating elasticities for the U.S. tax reforms in the 1980's. High-income individuals in the U.S. saw their proportion of total income increase relatively faster than other income groups between 1984 and 1989: 2.5 and 2.0 point increases for the top 1% and 0.5% respectively.[43]As with the 1980's cuts in the U.S., Table 4 demonstrates that the METR cuts following TONI were relatively greater for the richest third of the population. However, unlike the U.S. in the 1980s, the Canadian surge in top incomes between 1999 and 2004 was not as pronounced. Table 8 shows that over this period, the proportion of total income going to the top 1% and top 0.1% increased by 0.7 and 0.3 points respectively. Additionally, Figure 5 plots the real income distribution for the years 1999 and 2001, and is consistent with very little widening of the income distribution in the upper tail. Although the increase in Canadian top incomes across the TONI reform period were only about a third the size of the increases in the U.S., I use year *t-1 capital income* as a proxy for location in the income distribution to account for the correlation between the magnitude of cuts and the magnitude of income increases among top earners.[44]

The second omitted variable is due to mean-reversion. Empirically, a large percentage of very low income individuals have higher income in the following year, perhaps due to recovering from a job loss. Correspondingly, many individuals with high incomes have lower incomes the following year, especially for individuals who have bonus income tied to market performance. The natural control for mean-reversion, therefore, is the individual's location in the income distribution in year *t-1*. Given that the mean-reversion is strongest at the tails of the income distribution, I follow Gruber and Saez (2002) and use a ten-piece spline. That is, the sample is divided into ten equal groups (knots) where the marginal impact of the variable is allowed to vary at each knot; the first and last segments of the spline capture the unique dynamics of the lowest and highest deciles of the income distribution.[45] To summarize, I use

[42] See Weber (2014) for a discussion of how this assumption can be violated when there is a national (not province/state) tax reform where the magnitude of cuts varies by income level.
[43] Source: See Table 6.5 in Alm and Wallace (2000).
[44] Auten and Carroll (1999) argue that capital income, more than total income, can be used as a proxy for wealth, or a permanent location within the income distribution.
[45] As noted in Gruber and Saez (2002), if the data only covered a single federal tax reform, identification of the tax effects would be destroyed because location in the top decile would be correlated with the magnitude of the tax cut. However, our sample period includes provincial heterogeneity in cuts, and some provinces cut taxes in multiple years. I maintain the ten-piece spline used by Gruber and Saez (2002) because inspection of unconditional year-over-year income dynamics revealed that less knots

capital income as a control for income distribution widening, and *total income* as a control for mean-reversion.[46]

As discussed in Section 2.2 above, response to taxation reform is unlikely to be observed if tax changes are very small.[47] For it to be worth investing in accounting advice or adjusting labour supply, the tax changes would need to be sufficiently large to get the attention of tax-filers. Expanding the "spacing" between years in [1] from one to two years (or changing *t-1* to *t-2*), therefore, allows for greater cumulative changes in taxes given that most Canadian provinces phased in cuts over multiple years. In fact, Gruber and Saez (2002) use a spacing of three years in their baseline model, arguing that it allows more years for real tax-filer responses to appear, and minimizes the likelihood of short-run, re-timing responses showing up in the elasticity estimate. Using a three-year spacing, however, comes at a cost. The advantage of using adjacent years (*t-1* specification) is tax-filers are less likely to switch jobs or have large changes in income due to non-tax factors such as slowly-changing macroeconomic events.[48] Furthermore, a narrower window ensures that the set of tax planning technologies will not have changed significantly across the period.[49] For the baseline specification in this paper, I start with a two-year (*t-2*) spacing. All sample restrictions in the following section are discussed in the context of this two-year spacing (*t-2*; *t*) assumption.

Upon making all of the changes to account for income distribution widening, mean-reversion, and a two-year spacing assumption, the model becomes

$$\begin{aligned} ln\,(I_{i(t)} / I_{i(t\text{-}2)}) &= \beta_0 + \beta_1\, ln\,[(1-\tau_{ij(t)})/(1-\tau_{ij(t\text{-}2)})] + \beta_2\, ln\, \mathrm{S}(I_{i(t\text{-}2)}) + \beta_3\, ln\, K_{i(t\text{-}2)} + \beta_{4t} \textstyle\sum_t Y_t + \beta_5\, age_{(t\text{-}2)} \\ &+ \beta_6\, age^2_{(t\text{-}2)} + \beta_7\, self_{(t\text{-}2)} + \beta_8\, kids_{(t\text{-}2)} + \beta_9\, married_{(t\text{-}2)} + \beta_{10}\, male_{(t\text{-}2)} + \textstyle\sum_k \beta_{11k(t)} IND_k + (\varepsilon_{ij(t)} - \varepsilon_{ij(t\text{-}2)})\,, \end{aligned} \qquad [2]$$

where $K_{i(t\text{-}2)}$ is year *t-2* capital income, and $\mathrm{S}(I_{i(t\text{-}2)})$ is a spline function in year *t-2* income. For high income earners, β_2 is expected to be negative, and β_3 positive. All income values have been converted to 2004 dollars using a provincial CPI inflator (see Table 10).[50]

4.1.1 Pooled Models

Most of the U.S. research studying federal tax reforms in the recent tax responsiveness literature use models similar to [2], except without the *j* subscript since the reforms have been at the federal, not state, level.[51] Federal reforms imply that tax-filers with similar incomes face the same tax cuts; therefore, to have any variation in their dataset with which to identify β_1, researchers have pooled high and low income

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would not adequately capture the non-linearity of the relationship. For the lower threshold values of each knot used in this paper, see Table 9.

[46] Note that for high income earners, distribution widening affects income positively, mean-reversion negatively. As discussed in Kopczuk (2005), this is why separate controls are needed for each effect.

[47] In theory, with no adjustment costs, tax-filers would adjust to very small changes. In practice, they are more likely to respond to substantial changes in taxes.

[48] We do not observe whether individuals switch jobs in the tax data; the SLID has this information and so I address it in Chapter 2 of this thesis.

[49] For example, tax planning technologies that diffused very quickly include the conversion of many large corporations into income trusts and the incorporation of professionals such as doctors and dentists in Ontario following the 2001 law permitting incorporations (see Wolfson and Legree (2015)).

[50] Gruber and Saez (2002) use an income inflator by taking average growth in incomes. I prefer using provincial CPI growth rather than provincial income growth because the latter may be endogenous to the tax changes.

[51] For an alternative that uses subnational reform in the U.S., see Long (1999).

tax-filers in their estimation sample.[52]To control for known heterogeneity in income dynamics between high and low income earners, they included splines of total income and capital income. Specifications like [2] are therefore "quasi-pooled" reduced form models because the spline functions allow for some heterogeneity, but β_1 is estimated using a pooled sample.

Ideally, we would observe similar individuals receiving different exogenous changes to their marginal tax rate.[53] The TONI reform, with variation generated at the provincial level, is closer to this type of experimental setting in that researchers can compare individuals who are very similar according to all characteristics except province of residence.[54] For example, the subnational variation in tax rates allows us to compare two individuals, one living in Nova Scotia, the other in B.C., who are similar in age, industry of employment, and income, but who would have received very different tax cuts between 1999 and 2001 (see Table 4 for mean values). For most of the results in this paper I cut my sample into income tranches, estimating each separately, meaning that β_1 is no longer pooled across various income groups. This results in more of the variation in tax rates being generated by the "between-province" effects, or *horizontal variation*, rather than "within-province" effects in the context of this panel model.[55]

4.2 Sample restrictions:

Differencing the data requires changing the unit of observation in the raw LAD data from individual-year *(i,t)* to individual year pairs (*i*,*t*,*t*-*2)*. For example, a tax-filer present in LAD for all six years from 1999 to 2004 represents six observations. To convert the data to a first-differences unit of analysis like in [2], I create a record for each pair of years: 1999-2001, 2000-2002, 2001-2003, and 2002-2004, resulting in only four observations from the original six, or a 2/3 decrease in the record count for a fully balanced panel. Upon converting the 28 million LAD records over six years to two-year pairs, about 18.5 million remain in a "mostly-balanced" panel (see Table 11 for a summary).[56]

Once in year-pair form, I make a number of additional restrictions. I drop anyone who: (1) changed marital status between *t-2* and *t*, as this would likely give rise to changes in income and deductions that are unrelated to tax reform; (2) changed province of residence between *t-2* and *t* as this would invalidate the tax rate instrument by incorrectly predicting the counterfactual year *t* tax rate; and (3) in either *t-2* or *t*, is not between the ages of 25 and 65, inclusive. I restrict to those tax-filers above 25 so that the sample is comparable with the SLID sample in Chapter 2 (the SLID considers anyone over the age of 25 to be in a different census family). I drop those over the age of 65 so as to keep the sample limited to those who are traditionally working age, and to minimize the impact of pension income – such as the GIS benefit

[52] For example, an early influential paper in the literature using pooling was Feldstein (1995). Auten and Carroll (1999) and Gruber and Saez (2002) introduced more control variables to deal with issues associated with pooling low and high income filers. An exception is Saez (2003) in which there is variation within each decile generated by "bracket creep", or un-indexed tax brackets. The magnitude of the cuts were small, and there are issues of saliency and tax-filer awareness.

[53] Similar income also means facing similar opportunities and constraints. RRSPs and capital gains deductions are used more often by and typically only feasible for, higher income earners. Also, high income filers have access to more options (including tax planning advice) for optimizing their taxes.

[54] Other authors using this reform as a source of variation for identifying income elasticities include: Sand (2005), Dostie and Kromann (2013), and Milligan and Smart (2015).

[55] Many Canadian provinces are quite small so the benefit of the subnational provincial variation is confronted with the small sample sizes available in the most commonly used source of Canadian tax data, the Survey or Labour and Income Dynamics (SLID). This is why using LAD is important for this study.

[56] Even if there were no data missing for any individuals, the panel would remain mildly unbalanced due to births, deaths, and new entrants that are sampled to maintain the population coverage rate of 20%.

reduction – on contributing to spikes in METR values. The sample lost from these additional restrictions is summarized in Table 11. For the remaining sample to be an unbiased one, we cannot have tax-filers optimally changing marital status or province of residence in response to the tax reform. In the case of marital status, this assumption could be challenged in countries such as the U.S. where there is a "marriage penalty" from the joint filing system. There is no similar justification for an "optimizing" marriage response in Canada in the late 1990s.

The case of interprovincial migration and is less clear. Alberta's flat tax proposal was well-publicized, and as shown in Figure 2 the resulting top MTR in Alberta in 2001 was substantially lower than rates in Eastern Canada. High income, mobile tax-filers living in Eastern Canada in particular, could substantially increase their after-tax income by taking a job in Alberta or by flowing income through Alberta.[57] Responding in this way has different theoretical underpinnings, as it is assumed the tax-filer optimizes not only with respect to tax rates in his own jurisdiction, but also in response to tax rates in all *other potential* jurisdictions, as is the case in the tax competition literature. I avoid modelling tax competition in this paper (ie. τ_{ik}, $k \neq j$ not in objective function of filer in province j); elasticities shown in this paper, therefore, should be interpreted as responses to *own-province* legislative changes for individuals who did not move provinces.

For the baseline estimation of [2], I follow Gruber and Saez (2002) by setting a minimum total income cut-off. Specifically, I restrict the sample to those who earned at least $20,000 (2004 C$) in total income in *either* year *t-2* or *t*. In addition, I use a similar restriction to that in Sillamaa and Veall (2001) and drop those who paid less than $1000 in federal-provincial combined taxes in year *t-2*.[58] Making all sample restrictions just described, about 6.1 million differenced observations remain to estimate [2].[59] Looking at Table 11, after making all of these restrictions, the starting sample of differenced observations has fallen by about two-thirds, which is substantial. However, many of these restrictions were made to reduce the sample to one that represents that target population of interest, namely working-age, tax-paying individuals. Very few of the observations lost were due to "technical" and data-quality issues, such as values of the METR that are less than zero or greater than one.

4.3 Income Definition:

I exclude capital gains from total income due to their fundamentally different nature from other components of total income.[60] Previous research on U.S. income elasticities has excluded capital gains primarily due to their "lumpy" realization patterns. While I also appreciate this concern, my primary reason for excluding capital gains is to exclude sharp increases and decreases in income around the time

[57] Well-advised tax-filers can find ways to shift non-labour income into Alberta, such as setting up an inter vivos trust, and pay the lower tax rate (see Milligan and Smart (2014). LAD data does not include trusts (T3) data, as it is a database of T1 filers. For treatment of inter-state migration due to changes in tax rates on high income earners, see Young et al. (2014).

[58] Note, $1000 (2004 dollars) is the CPI-adjusted equivalent of the $625 (1988 dollars) used in Sillamaa and Veall (2001). I use total payable instead of basic federal tax as my cut-off. They do this restriction for both years; I only use it for year *t-2* so that the sample (through use of deductions) will not be endogenous to the reform. However, I restrict the total income in year *t* to be above $20,000 as it is less likely for income at these levels to decrease due to income effects following tax cuts along the intensive margin (I am not modelling the extensive margin for low-income individuals or secondary earners in this study).

[59] See Table 11 for a summary of the magnitudes of dropped sample. Observations are dropped in step-wise fashion in the order they are mentioned.

[60] Specifically, I exclude taxable capital gains from income ex post; that is, they are included for the purpose of calculating an METR so that we know where the tax-filer lies on her budget set, but are subtracted from the definition of total and taxable income for the purpose of generating an elasticity. I also add back capital losses that are matched with the capital gains.

of the stock market crash that occurred at the same time as the TONI reform in Canada, as well as the change in the inclusion rate in 2000. Indeed, study of the pattern of capital gains throughout this period likely warrants a separate analysis.[61]

Given that many tax reforms change simultaneously the statutory marginal tax rates and the definition of the income tax base, it is challenging to separately identify the elasticity solely due to the change in rates. To do so requires fixing a constant definition of the tax base, or "constant-law" definition, an approach adopted by many researchers to date.[62] The major 1988 tax reform studied by Sillamaa and Veall (2001) is an example of a reform in which both the tax base and tax rates were changed simultaneously, creating problems for identification. In that reform, the federal government converted a number of deductions to non-refundable credits, resulting in a mechanical increase in taxable incomes. Although non-refundable credits and statutory marginal tax rates were adjusted to minimize changes in the tax burden, it is clear that the original definition of taxable income did not remain constant. Fortunately, the TONI reform studied in this paper involved fewer changes to the tax base. The most significant change was the reduction in the capital gains inclusion rate in 2000, but I address this by removing taxable capital gains amounts from the definition of total income. Minor changes to the tax base over this period included the introduction of the *Canadian forces and police deduction* in 2004, but I do not modify the tax calculator to account for such minor changes in this paper.[63]

I also calculate elasticities for the federal definitions of net income and taxable income. Variation in these values that is not present in total income is due to the existence of various deductions that a tax-filer can report such as union dues, RRSP/RPP contributions, or capital losses from other years. For example, in anticipation of the tax cuts announced far in advance in Alberta and Saskatchewan, a tax-filer in one of these provinces could have made an RRSP contribution while taxes were high and subsequently make a withdrawal when tax rates dropped.[64]An annual summary of the major income items, deductions, and credits by income group can be found in the annual T1 Final Statistics report produced by the Canada Revenue Agency.

5 Results

5.1 Baseline Model:

For the baseline specification defined in equation [2], I estimate elasticities for the two most common definitions of income in the literature; namely total income and taxable income.[65] It is taxable income that is most relevant to policy-makers as this is the tax base on which progressive statutory tax rates are

[61] For a thorough discussion the role of capital gains income in estimating income elasticities, see Saez et al. (2012), Section III. Note that I include employee stock options, which are similar to capital gains due to partial inclusion in taxable income. I include stock options because they are treated as employment income, and therefore are a potential source of income that would be responsive to tax reform that an employee could negotiate receiving. The taxation of stock options, like capital gains, is very complex. Future research would likely involve separate analyses of the elasticities of these forms of income.

[62] Kopczuk (2005) addresses the issue of simultaneous changes in tax bases and rates with a unique empirical specification that controls for changes in the base.

[63] See Table 5 for identification of "constant-law" variables that changed definition between 1999 and 2004.

[64] This is a crude example for illustration of how deductions could be used to pay less tax; other considerations such as residual RRSP contribution room may make this particular tax planning example less appealing.

[65] In the U.S. literature, the comparable definition of total income most commonly used is Adjusted Gross Income (AGI).

applied. Note that I truncate all values of taxable income at zero where removal of taxable capital gains would yield negative values of taxable income.[66]

The Gruber and Saez (2002) specification was originally motivated by marginal changes in income in response to tax rates. In practice, however, some tax-filers experience changes in income between a pair of observed years that can exceed several factors of magnitude in either direction. For large positive changes and large negative changes in the data, values of the $ln\ (I_{i(t)} / I_{i(t-2)})$ term are greater than 20 and less than –4 respectively. By way of comparison, for tax-filers who experience changes in income of a factor of 2 or a factor of ½ – large changes in their own right – the value of $ln\ (I_{i(t)} / I_{i(t-2)})$ is only 0.69 and –0.69 respectively. Therefore, to remove these outlier observations from the sample, I make a few additional sample restrictions beyond those described in Section 4.2. Consistent with the mean-reversion discussion in Section 4.1 above, most of the tax-filers who experience large changes of income are found within the tails. Therefore, I first drop all tax-filers with income greater than \$250,000 in year *t-2*, a cut-off which is between the 99th and 99.9th percentile of the income distribution. The average change in income among this group between 1999 and 2001 is several thousand dollars and negative, reflecting the role of mean-reversion. This restriction does not capture all of the outliers, so I also drop individuals who have increases in taxable income of greater than 100% or income losses of greater than 50%.[67]

The model is not only sensitive to large changes in the dependent variable, but also to large changes in the primary independent variable of interest, $ln\ [(1-\tau_{ij(t)})/(1-\tau_{ij(t-2)})]$. Therefore, I also drop any observations for which the *predicted* log-change in the net-of-tax rate (the instrument) is greater than 0.3 or less than -0.1. The instrument is intended to represent changes in tax law, and changes outside this range were not legislated. Such observations likely show up in the data where the tax-filer is near discontinuities in the METR across some income ranges. I also drop observations where the *actual* log-change in the net-of-tax rate is greater than 0.3 or less than -0.3. Such large changes generally can again be due to proximity to discontinuities, but since these are actual changes in rates, these changes can also be due to major changes in income. As a result of these additional restrictions, I lose 461,000 observations in addition to those restrictions already identified in Table 11.[68]

The baseline elasticity estimates from specification [2] are presented in Table 12. There are eight columns in the table; the first four for taxable income, the latter four for total income. For each income type I add progressively more controls moving from left to right: first, I use the simplest specification; then a ten-piece spline of income; then industry controls; and finally clustered standard errors at the province level.

[66] Removing taxable capital gains from total income is straight-forward. However, deducting taxable capital gains from taxable income can yield negative values of taxable income if other deductions are present. I also add back elected capital losses to the definition of taxable income since losses can only be applied if gains are claimed in the tax year. The truncation results in just over 12,000 observations that have a taxable elasticity of exactly zero. The cost of this truncation is that the dependent variable, the log-ratio of incomes, tends to be very large when one of the values in either year *t-2* or *t* is zero. I therefore drop all observations in which taxable income is less than \$100 in all regressions. Adding these observations back into the sample changes the elasticity in column 1 of Table 17 to a value of less than -2.00, a huge change for a loss of about 0.2% of the sample, reflecting the hugely volatile elasticity estimates when these very small incomes are not dropped from the estimation sample.

[67] The reader may wonder why I did not just implement this more targeted restriction in the first place and eschew the restriction on those with income over \$250,000. Dropping these very high earners serves another purpose, however. I provide evidence in Section 5.5 that pooling very high income earners with tax-filers in the 90th to 99th percentile may be inappropriate. Specifically, in Table 18 I provide evidence that the top 1 percent has a dominating effect on the rest of the top decile for weighted regressions.

[68] The sample of 10.6 million observations in row 10 of Table 11 (the sample representing the target population of interest) represents about \$108B of total tax payable in 1999; upon making the sample restrictions in rows 11, 12, and 13 of that table and those in this section, the remaining sample accounts for \$83B, or 77% of the value of total tax payable.

The differences in elasticities are significant between the first two columns for each income type. This difference is explained by the fact that the first column uses a single variable to control for mean-reversion, while the second column in each case uses a ten-piece spline. Looking at the point estimates of the splines of year *t–2* taxable income column (2), the values in the first five deciles are in the range of –0.16 to –0.41, which is suggestive of much stronger mean-reversion than is captured by the single estimate of –0.095 in column (1). Thus, at least for the bottom half of the income distribution, the spline function seems to appropriately capture year-over-year income dynamics.[69] Adding the industry controls (in columns 3 and 7) has very little impact in each case. By clustering standard errors at the province level, the significance of the estimates vanishes in both cases.

The elasticity of taxable income is greater than that of total income, although not significantly. One reason for this is mechanical: since taxable income is simply total income minus deductions, percentage (or log) changes in taxable income will be larger because its denominator is smaller.[70]A second possible reason for greater values of taxable income elasticities is that tax-filers may reduce RRSP deductions in response to the cuts in tax rates.

5.2 Splitting the sample by income groups

As discussed in Section 4.1.1 above, equation [2] pools individuals with very different incomes to identify the elasticity. In Table 13, and most of the following tables in this paper, I cut the sample into ten distinct income deciles and estimate equation [2] on each separately. In this setting, relatively more of the variation in the tax rates will reflect the province of residence of tax-filers, as opposed to different lagged incomes. I should again emphasize that the advantage of exploiting subnational, rather than national, variation in tax rates is we do not have to pool individuals who have very different incomes in order to generate identifying variation. Table 13, therefore, repeats the specification in column (4) from Table 12, but now split into ten separate samples by year *t-2* income. Threshold values for entry into each decile are shown in the third last row of each column.

The results indicate substantial variation in elasticities ranging from –0.15 within the fifth decile to 0.11 within the eighth decile. The two negative (and significant) elasticities within the fifth and sixth deciles are unexpected. One possible explanation is that there is insufficient tax rate variation within these income tranches. Inspection of Table 4 reveals that the difference, in terms of percentage points, between the province with the greatest cut and that with the smallest cut were only 2.4 and 2.7 in the fifth and sixth deciles respectively. By way of comparison, this difference is 4.3 in the ninth and tenth deciles. Given that the identification strategy I use works best with rich interprovincial variation in tax rate changes, estimates in the middle and lower deciles should be interpreted with more caution than those for the higher deciles.

5.3 Decomposing the income definition

[69] Where the single variable does not capture heterogeneity, it will bias elasticity estimates down. Also, note the very large mean-reversion for the first decile; this effect is likely mechanical since I restrict year *t* income to be greater than $20,000. That is, if a tax-filer starts in the bottom decile just above $20,000, they will only be kept in the sample if their income goes up. This sample restriction therefore biases downward the elasticity estimate of the bottom decile.

[70] For example, if a tax-filer has $50,000 of total income and $5,000 of deductions, and he “increases” his total income by $5,000 in response to a tax cut (with deductions staying at $5,000), his total income goes up by 10%, and his taxable income goes up by 11.1%: ($50,000-$45,000)/$45,000.

Taxable income is simply total income minus a set of deductions. A first step in decomposing the taxable elasticity from Table 13, therefore, is to reproduce the same table, except using total income rather than taxable income. This removes any component of the taxable income elasticity that is due to the use of deductions. I do this in Table 14 and find that the total income elasticities in the fourth through tenth deciles are the larger than those for taxable income. Notably, unlike for some of the deciles of taxable income, none of the total income elasticities is negative and significant.

This process of decomposing the taxable income can be taken even further. Similar to what is done in Sillamaa and Veall (2001), and in Milligan and Smart (2015) using aggregated data, I run separate regressions, within each decile, for net income and employment income, which are other subtotals of taxable income. Table 15 summarizes the elasticity estimates for each of these regressions, where I repeat the elasticities for taxable and total income from the first rows of Table 13 and Table 14 respectively to aid in comparison.

In Table 15, in almost all cases among the top five deciles – which comprise the tax-filers who pay nearly three-quarters[71] of taxes – the total income elasticity is greater than the net and taxable income elasticities. This is somewhat of a puzzle because theoretically the taxable income elasticity should be greater; for a given percentage change in total income, the given percentage change in taxable income should be greater in the presence of a constant, positive amount of deductions.[72] If deductions decrease following a tax cut (for example RRSP contributions could decrease as the tax deferral benefit falls) then the taxable income elasticity should be greater still than the total income elasticity. One possible explanation for higher total income elasticities would be if deductions were to *increase*, rather than decrease, in response to a tax cut. If a tax-filer only needs a fixed real amount of after-tax income for consumption each year, then the filer may respond to having "excess" after-tax income by contributing to an RRSP in that year and therefore decreasing taxable income.[73] Looking at the data, RRSP contributions in the top decile jumped from $12.9B in 1999 to $14.8B in 2000.[74] To the extent that those with greater tax cuts (typically high income earners) made greater RRSP contributions, this is unconditional evidence that RRSP contributions could partly explain the difference between total and taxable elasticities. Of course, this period is further complicated by a volatile stock market environment that certainly could have affected RRSP contribution decisions. Interestingly, Sillamaa and Veall (2001) also estimated a higher elasticity of total income in comparison to taxable income, values of 0.26 and 0.14 respectively for their baseline model.

Another consideration affecting the interpretation of the elasticity of total income is the inclusion of dividend income. Because net dividends are "grossed up" within the Canadian income tax system to reflect their pre-corporate-tax values, a tax-filer such as the owner-manager of a CCPC who substitutes

[71] According to the T1 Income Statistics report of 2006 (for tax year 2004), those earning $50,000 paid 72.4% of total (federal plus provincial) taxes payable. Per Table 9, $50,000 is slightly higher than the cut-off for the top five deciles as defined in this paper, so the actual percentage paid by the top five is even greater.

[72] See supra footnote 70.

[73] A second possible explanation is a change in the inclusion rate of employee stock option benefits. In 2000 the effective inclusion rate was reduced from ¾ to ½ to match the corresponding changes in capital gains. This has the effect of mechanically reducing taxable income due to a change in the definition of the tax base. The 2005 Tax Expenditure Report produced by the Department of Finance shows that the tax expenditure increased by about $300 to $400 million due to the change (if we assume no behavioural response). If this income were added back to the taxable incomes of filers, it could have a material impact on the elasticity. This is a potential issue that could be addressed in future work.

[74] Here, top decile refers to the full LAD 10% sample with no restrictions applied. The CRA *Tax Statistics on Individuals* publication (the "Greenbook") is unavailable online prior to the 2004 tax year, and is unavailable in print following the 1997 tax year. Therefore I could not consult this data source as a test against the LAD 10% file.

dollar-for-dollar away from salary income in favour of dividend income will report an "inflated" value of total income. That is, the resulting increase in *total income for tax purposes* would not reflect a real increase in total (net) income. Given the TONI reform introduced provincial dividend tax credits for corporate taxes paid, the degree of double-taxation on dividend income in some provinces was likely reduced, and this may have led to such a shift towards dividend income for owner-managers of CCPCs. I did not explicitly test for this income adjustment in the data, but its effect would be to bias upward the elasticity estimates given the introduction of the provincial dividend tax credits would not affect the METR on employment income. Therefore, the already low elasticity estimates of total income presented in Table 14 may be over-stated.[75]

There is a second issue associated with the inclusion of gross dividends in aggregate measures of income. Because of the dividend tax credit, marginal amounts of dividend income are subject to a lower METR than is employment income. For this reason, if a tax-filer earns a high proportion of her income in the form of dividends, the employment income METR used in the regressions presented is possibly inappropriate. Given the nature of the empirical specification in differences form, however, the impact of any mis-specification is minimized.[76] Furthermore, the appropriate METR to use in a regression depends on what source of income is the "marginal income" of the tax-filer, which is unknown to the researcher. For all of the above reasons, future work would likely involve separate analysis of the responsiveness of dividend income to tax reform.[77]

5.4 The 90th to 99th Percentile

Much of the recent Canadian research on elasticities of taxable income has focused on earners above the 90th percentile.This focus is warranted as these earners paid 53% of combined provincial and federal taxes in 2004 (see Table 8), and arguably have the most opportunity to make adjustments in response to tax changes. High income earners, however, tend to have different constraints and opportunities to adjust income in comparison to those in the middle of the income distribution. For this reason it may be more appropriate to modify the empirical specification to capture the year-over-year income dynamics of these tax-filers (see Goolsbee (2000a). In Table 16, I test the robustness of the estimates for the top decile from Table 13 by varying some of the sample restrictions and specification assumptions. The first column of Table 16 is the same specification as column 10 of Table 13. The subsequent variations I test are as follows:

[75] As described in Section 3, I create the METR by simulating an increase in employment income. This increase would not trigger dividend tax credits. The upward bias on the elasticity is due to the fact that we would observe increased dividend (and therefore total) income for a given change in METR. Because high earners tend to have more dividend income, this would create a correlation between greater METR cuts (that went to high earners) and total income. In future work, I would consider changing the definition of dividends included in total, net and taxable income to "net dividends" which are dividends before the gross-up factor is applied.

[76] Because I model the *change in tax rates* based upon an underlying linear model, the degree of mis-specification is likely minor. For example, if the METR on employment income falls by 5 percentage points, and the corporate tax rate, gross-up rate, and dividend tax credit rate do not change, then the METR on dividend income will also fall by 5 percentage points. The only difference is the starting value of the employment income METR could be 48% vs. 33% for dividend income. With a smaller denominator, this implies the *percentage change* (or log-change) in the METR would be biased downward, and as a result the elasticity estimate could be biased downward.

[77] Generally, all income that receives special treatment, such as capital gains and employee stock options, should be analysed separately in recognition of the different incentives and constraints associated with these sources of income.

Add additional ten-piece spline: Inspection of mean year-over-year changes in income within vigintiles of the top 10 percent sample (cuts of 0.5% of the top decile) reveal that those in the 90th to 91st percentile tend to have greater increases in income than those in the 99th percentile. Adding an additional spline will better capture the heterogeneity within the top ten percent.

Dummies for major source of income: Those earning income primarily through paid employment are likely to have different year-over-year income dynamics from those who earn primarily investment income. Department of Finance (2010) includes dummies for those who earn income primarily from paid employment, self-employment, passive investment income, or capital gains income to capture these differences. I try this same approach here.

Drop filers with capital gains income in either year: In all models I subtracted taxable capital gains from the total and taxable income values. However, I had included capital gains in the tax calculator for the purposes of calculating a filer's METR. To test how much these filers impact the overall elasticity, I drop them here.

Drop Quebec: Provincial deductions and tax credits are not made available to Statistics Canada for inclusion in the LAD. This creates the possibility of greater measurement error in the METRs for Quebec filers. I drop Quebec records here to test if this has a significant impact on the overall estimates.

Drop British Columbia: Among the four provinces that made the most substantial cuts between 1999 and 2001, BC was the only one that did not announce its cuts in advance (see Table 2), which would significantly reduce tax planning opportunities such as delaying income realization. Dropping this province would, therefore, allow more of the variation to be identified off Alberta, Saskatchewan, and Newfoundland, where tax cuts would have been known to tax-filers in advance.

The six columns of Table 16 present the results for each of these cases. The most substantial change in elasticity is found between column (3) and column (6); the only difference between these being the exclusion of BC. The point estimate goes from positive and insignificant to negative and insignificant. Given that BC had the second-most substantial tax cuts of all of the provinces within the top decile (see Table 4), and likely most newsworthy, it could be the case that small, *real* responses were induced on the workforce within the top ten percent. By excluding this province, I could be losing one of the only provinces in which responses (real or otherwise) generated a response among tax-filers, perhaps explaining the drop in the elasticity.[78]

5.5 Re-introducing the Top 1 Percent

Up until this point, I have excluded those in the top one percent (more specifically, those with total income greater than $250,000, which is between the 99th and 99.9th percentile) from the sample for several reasons. First, this group of tax-filers is different from the other groups in that they have greater access to tax planning opportunities than do others. Second, mean income changes between year *t-2* and year *t* revealed very strong mean-reversion within this group that was not present within the 98th to 99th

[78] Eissa (1995) studying the elasticity among married women in response to the major U.S. federal reform of 1986, only considers tax-filers with cuts of greater than 10% to be "treated" with the cut. By these standards, the entire sample I study, on average, would be considered untreated. If a 10% cut is in fact required to get the attention of tax filers, it is understandable that dropping high-cut provinces like BC would negatively impact identification.

percentile. Finally, there is a trade-off between homogeneity of individuals and sample size when doing pooled regression analysis on tax-filers; the differences between the 90th percentile filer and 99th percentile and above filers are arguably too great to warrant the inclusion of the additional sample.

In Table 17, I relax the constraint of dropping the top 1 percent within the top decile. Instead, starting with the full sample of the top decile, I incrementally restrict the lower cut-off of the sample by one percent at a time, culminating in an elasticity estimate for the top 1 percent in the tenth column. As the lower cut-off is increased from the 90th to the 94th percentile, the elasticity progressively increases which is consistent with the theory of elasticities monotonically increasing in income;[79] standard errors fall over this range. Starting at the 95th (or the "top 5") percentile, the elasticity decreases and standard errors increase.

This increase in standard errors between P95+ and P99+ may be explained by the fact that one-fifth of the remaining sample in the top 5 percent is comprised of those in the top 1 percent. These tax-filers are very different from those in the 95th to 99th percentiles and outlier effects may be strong. The smaller elasticity estimates, however, are more in contrast with the theory of elasticities monotonically increasing in income due to increased opportunities for tax planning. I think it is worth noting, however, that none of the elasticity estimates is statistically significant from zero, with the exception of P94+ which is significant at the 5% level.

In a model of reported income in which a tax-filer has access to "tax avoidance technology" such as accounting advice, a tax-filer will increase tax avoidance as the opportunity cost of doing no tax planning increases (or, as taxes increase). However, this theory is often presented in the context of a *tax increase*, not a tax cut such as the TONI reform. For example, the theory posits that if the marginal tax rate increases from τ_1 to τ_2, tax-filers will increase tax planning activity on the margin to reduce the value of taxable income. In a model where there are no fixed costs of tax planning, if the tax rate returns to τ_1, the tax-filer would reduce tax planning efforts so as to return taxable income to its original level; if this were not the case, the tax-filer was not optimizing in the first place. In such a model therefore, we expect symmetry of the response over tax cuts and tax hikes.

If we introduce fixed costs, however, the symmetry is challenged. Much of the cost of tax advice is up-front, such as setting up a corporation to use for tax deferral or income splitting. Once this structure is in place, annual maintenance costs for such a tax structure are low. If taxes were to then fall, and the cost of doing no tax planning decreases, there is little incentive for the tax-filer to dismantle an existing tax avoidance structure, especially given such a dismantling would likely involve additional legal and accounting fees. This line of reasoning suggests it may be warranted to model this asymmetry in the tax-planning decision that arises in the case of tax hikes versus tax cuts. The corollary of this is that it may be inappropriate empirically to assume the tax-filer is only concerned with the level of the METR and will respond symmetrically to tax cuts and tax hikes.

It is puzzling, therefore, that other studies have found high elasticities within the top one percent while using the TONI reform (a period of tax cuts) as the source of identifying variation. The only study of which I am aware that uses a microeconometric approach is a white paper by the Department of Finance

[79] In particular, Goolsbee (2000a) provides evidence that "time-shiftable compensation" rises dramatically with income in the U.S.

(2010). They find an elasticity of 0.19 for the top 10 percent, and 0.72 for the top 1 percent. However, the regressions that produced these elasticities were weighted by taxable income, implying that the estimates are meant to be interpreted as elasticities of the *tax base*, rather than the *individual elasticity* of all tax-filers in these income groupings.[80] While the former is useful as a guidepost for informing how responsive overall government revenues are to tax changes, it does not tell us *where* the responsiveness is occurring. The distinction is important. For example, if the tax-filers who are in the top one percent of the top one percent (or who are above the 99.99th percentile overall) have much higher elasticities than those in the rest of the top decile, weighting a pooled regression by real incomes will cause these very high income observations to have a dominating influence on the overall elasticity of the top decile.

To make the results of that Department of Finance (2010) paper comparable to the results presented in this paper, I would need the unweighted results; unfortunately I was not able to obtain access to these estimates from the authors. However, given that I have access to the same data and use much of the same variation, I attempt to reproduce their tax base (weighted) elasticity estimates using their specification approach. The results of this attempt are shown in Table 18. I find a similar pattern of increasing elasticities of taxable income as the sample is restricted to the top ten, five, two, and one percent. The estimates I obtain are not exactly the same as those from their paper as there are a number of minor elements in that paper which I am unable to reproduce.[81] I find a tax base elasticity of taxable income of 0.57 for the top one percent, which I consider reasonably close their estimate of 0.72. This estimate is also close to the *macro-share estimates* of 0.62 and 0.66 in Department of Finance (2010) and Milligan and Smart (2016), respectively.

To make the attempted replication of the Department of Finance (2010) elasticities comparable to mine, in the final four columns of the table I re-run the regressions except that I replace the real income weights with log-income weights to reduce the influence of those above the 99.99th percentile. Given that log-values of high incomes do not discriminate as severely as the real incomes, I argue that the new set of results can again be interpreted as elasticities of individual incomes instead of elasticities of the tax base. Upon making this change, elasticities remain small and significant for the top 10 and top 5 groups, but the elasticities for the top 2 and top 1 are *not significantly different from zero*. This zero-elasticity result provides suggestive evidence that the income weights among the top 0.01% in the tax base regressions may have a dominating effect on the elasticities within the top 2 and top 1. Given that the elasticity weighted by log-income is a better representation of the mean individual elasticity (as opposed to the tax base elasticity), the results suggests that my results in this paper are not very different from those in Department of Finance (2010).

To test if the elasticity in the top 0.01% (and its corresponding weights) may have dominated the result for the top 1% in Department of Finance (2010), I remove the overlapping definitions of the "top"

[80] Gruber and Saez (2002) discuss the idea of weighting regressions to convert mean individual elasticities to tax base elasticities. For example, a tax-filer with income above the 99.99th percentile increasing income by 10% in response to a cut would have the same effect on government revenues as adjustments of the same magnitude by *many* "lower income" earners just above the 90th percentile.

[81] I could not exactly reproduce their results as I use the period 1999-2004 while they use 1994 to 2006. These missing years, however, have very little variation in tax rates. I also add back capital losses in addition to subtracting capital gains. I also included capital gains and losses in the tax calculator *only for the purpose of* calculating the METR. They use a one-year spacing between years, but this is not the source of the difference as I get very similar elasticities when using this assumption (see Table 21). Their paper uses a T1 calculator internal to the Department of Finance and therefore does not use CTaCS. Finally, I do not include some province-year interaction terms identified in their paper as they are not listed in the published version.

groupings in favour of mutually exclusive income categories. In addition, I add two more categories of income, namely the top 0.1% and the top 0.01%. The results are presented in Table 19. Due to confidentiality issues around these very high income groups, I provide only the key covariates and round sample sizes to the nearest 50. The elasticity is highest for the P95-P98 group and decreases for subsequent income groups with the exception of the top 0.01%. For this highest group, the point estimate is 1.73, a very large elasticity by the standards of the literature. It is possible, therefore, that this income group is responsible for the high elasticities of the top 2 and top 1 percent in Table 18. This elasticity is not significant, however, and therefore does not imply that this top income group on average reduced tax planning efforts in response to the tax cuts delivered by the TONI reform.[82]

The results in Table 18 and Table 19 highlight the sensitivity of elasticities to assumptions about weighting and pooling different income levels. This is problematic because the different sets of results can have very different policy implications. Looking at the weighted result of 0.57 from Table 18 can give the impression that if the government were to, for example, increase marginal tax rates on the top 1 percent, that this would imply large revenue leakage from this entire group. Removing the weights and splitting the sample into mutually-exclusive groups, however, shows that although the very highest earners may be driving the high elasticity for the whole group, the response among this group is imprecisely estimated.

5.6 Robustness Check: Different year spacing

In the baseline model equation [2], I assume a two-year spacing between pairs of years in the first-differences model. Expanding the spacing will tend to pick up more long-run effects, whereas contracting it more will pick up short-run tax planning effects. To generalise the year spacing, we can write the model as

$$ln\,(I_{i(t)} / I_{i(t-s)}) = \beta_0 + \beta_1\, ln\,[(1-\tau_{ij(t)})/(1-\tau_{ij(t-s)})] + \beta_2\, ln S(I_{i(t-s)}) + \beta_3\, lnK_{i(t-s)} + \beta_{4t} \sum_t Y_t + \beta_5\, \mathrm{age}_{(t-s)} + \beta_6\, \mathrm{age}^2_{(t-s)} + \beta_7\, \mathrm{self}_{(t-s)} + \beta_8\, \mathrm{kids}_{(t-s)} + \beta_9\, \mathrm{married}_{(t-s)} + \beta_{10}\, \mathrm{male}_{(t-s)} + \sum_k \beta_{11k(t)} IND_k + (\varepsilon_{ij(t)} - \varepsilon_{ij(t-s)}) \,, \qquad [3]$$

where *t-2* has been replaced with *t-s* to represent the spacing between years. The accuracy of the instrument for $ln\,[(1 - \tau_{ij,t})/(1 - \tau_{ij,(t-s)})]$, however, tends to decrease in the spacing, *s*. For example, consider the last row in Table 20. The mean absolute deviation between the instrument value and the actual value for all tax-filers for a one-year spacing is 1.8%, while for a three-year spacing it is 2.5%. This means that the instrument will tend to better explain the actual tax rate change when pairs of observed years are closer together.

Table 21 presents the results of the estimation of equation [3], repeating the baseline specifications from column (4) and column (8) of Table 12 for taxable and total income respectively. For both types of income, the elasticity is increasing in the year spacing assumption. In all cases the point estimate is insignificant, so while there may be weak evidence of longer-run responses, it is not conclusive. The

[82] Cross-province variation in taxes is the key to my identification strategy. Although not presented here for confidentiality reasons, I verified that tax-filers from Alberta and British Columbia, the two provinces with the greatest tax cuts, represent just over 25% of the top 0.01%, the same proportion as for the top 1% as a whole. Therefore, it is not the loss of cross-province variation that is driving the high standard errors.

three-year spacing estimate of 0.078 for taxable income is small in comparison to other estimates in the literature.

6 Conclusion

Taxable income elasticities depend critically on the unique features of the tax environment within each tax jurisdiction. For this reason, elasticities estimated from other countries, such as the U.S., are not appropriate for use in models projecting deadweight loss or revenue sensitivity to tax reform in Canada. As such, more "made in Canada" research is needed to increase confidence in our understanding of the responsiveness of the Canadian tax base to tax reforms (see Milligan (2011) for a discussion). Furthermore, many models that use an elasticity parameter as an "input" for projecting some policy counterfactual are very sensitive to the elasticity value. For example, Milligan and Smart (2016) show that at an elasticity value of 0.664, PEI would retain only 6.4 cents of every additional dollar raised if it were to increase its statutory rate on the top 1% of its earners by 5 percentage points. This result is due to the size of the behavioural response term in the marginal revenue formula.[83] If this elasticity were half the magnitude (0.332), PEI would retain 0.532 cents, which is over eight times greater. With the policy implications under these two scenarios being so different, it is easy to make the case that Canadian research should continue in an effort to get elasticity estimates "right".

One of the key insights from this chapter is that *unweighted elasticities*, or the mean elasticities of individuals (rather than the elasticity of the tax base as a whole) may be very low. I cannot compare my unweighted results with Milligan and Smart (2016) because these authors used aggregated income data, and therefore could not produce unweighted elasticities.[84] It is likely, therefore, that much of the elasticity of high income earners is driven by the very highest earners. Comparing columns 4 and 8 in Table 18 shows that simply weighting the regression for the top one percent sample by income increases the elasticity from near zero to 0.57. The elasticity estimate for the top 0.01% of 1.72 in Table 19 provides further evidence that high income dominance could be very significant. Given the difference in estimates between the top 1% and top 0.01% samples, pooling of the tax-filers in the top 1% is likely inappropriate. Future estimation of the elasticities of top earners in Canada should likely focus on cutting the sample of the top 1% into finer groups, and perhaps also by major source of income to recognize the unique nature of these tax-filers. Furthermore, econometric specifications such as those used in this paper may be inappropriate for such higher earners. To look for the existence of behavioural response, researchers may want to consider turning to more descriptive methods and testing more narrowly-defined hypotheses to uncover the existence of tax-planning. For example, using aggregated data, Bauer et al. (2015) focus specifically on income splitting to minor children through the use of CCPCs. If micro data are to be used, many research questions would require population datasets (such as the T1 Family File) due to the smaller sample sizes for top earners.

What are possible explanations for the low individual elasticities found in this paper? The top one percent of earners is mostly comprised of individuals who work full-time, and who on average work well in

[83] The formula is not shown explicitly in their paper. However, given the other formulas in the paper, I have determined it to be: $dR/dM = [(1-\varepsilon*a*\tau^p)/(1-\tau)]$, where ε is the elasticity, a is the Pareto parameter, τ^p is the top provincial rate, and τ is the top combined provincial-federal rate.

[84] In principle, the authors of Department of Finance (2010) would have likely generated unweighted results, but these were not shown in the published version of the paper.

excess of 2,000 hours per year.[85] That these individuals cannot increase their labour supply is not surprising. This is why most of the discussion of the elasticity of income among top earners focuses on the tax planning response margin. Tax planning theory predicts that high income tax-filers will reduce tax avoidance effort when tax rates are cut as the marginal benefit of avoidance falls (tax rates are reduced). The low taxable income elasticities found within this paper, however, imply that even tax planning responses are negligible. This is a puzzle because the very existence of the personal income tax planning industry in Canada implies that individuals *do respond* to taxation by seeking tax planning advice, and the aggregate financial benefits of doing so in terms of tax-savings are arguably at least as great as the revenues of personal tax advisory practices.[86] There is a possible explanation that reconciles these two conflicting observations. The fact that I find very small elasticities does not negate the existence of this industry, but rather suggests we do not find evidence of a substantial response on the margin *over the range of tax rate reductions* observed during the TONI reform. This outcome may be explained by the high initial set-up fees associated with some tax planning strategies. There is little reason to believe why tax-filers would dismantle a tax planning strategy, such as income splitting through the use of corporations, when rates become marginally lower.[87]

The existence of such frictions implies that tax planning would not decrease unless cuts in statutory rates were much more substantial, such as the federal U.S. cuts in the 1980s, and may not occur through tax-filers exiting tax planning, but rather by *reducing the flow* of non-planners into tax planning. For example, this could be the case for entrepreneurs and start-up firms. With lower tax rates, these firms could spend more of their time running their business, and less of their time on tax planning. If this dynamic is in operation, my identification strategy would not pick up this effect as it involves a counterfactual which is unobservable using micro-level tax data, and would take years to unfold.[88] The frictions in tax planning efforts caused by the high setup costs may also imply asymmetric elasticities. For example, one could imagine that if the TONI reform involved a series of tax hikes rather than cuts, forward-looking tax-filers may decide to make the investment in tax planning advice on the margin if they expected these hikes to persist indefinitely.

I should make a few cautionary notes about the elasticities found within this study. First, due to the potential asymmetric response just discussed, the elasticities within this paper may not be appropriate for forecasting the potential response of a tax increase. Second, some of the response margins tax-filers use in response to tax reform are outside the scope of this paper. These include migration patterns

[85] Moffitt and Willhelm (2000) show 60% of those in the highest tax bracket in the U.S. work more than 2,500 hours per year, compared with about 20% for everyone else. I reproduced a similar statistic using SLID (not shown) and found Canada's highest earners to be approaching the possible upper limit of labour supply, measured in annual hours paid.

[86] Without loss of generality, by tax-planning advice I am really concerned with more sophisticated advice beyond the use of tax-preparation services.

[87] Furthermore, even in the case of a tax increase, new tax planning technologies do not necessarily arise instantaneously due to an increase in demand. These technologies may arise on the supply side of the market as they are "invented" by individuals. Some tax planning technologies may diffuse throughout the market quickly, e.g. corporate income trusts, while others may be adopted more slowly. For all of these reasons, we should not necessarily expect a rapid tax planning response to occur within the two-year window on which the elasticities in this paper are based.

[88] Tax-filer age and income trajectory may provide one way to test the hypothesis of reduced flows into tax planning in the presence of lower METRs. For example, future research could compare the response of younger and older high income taxpayers in the presence of tax cuts to see if the former, who are likely less established tax-planners, are more likely to substitute away from tax planning efforts on the margin. Furthermore, one could use the identification strategy of Chapter 3 contained within this thesis and estimate a rate of incorporation (a flow) and see if this rate decreases when METRs fall.

(interprovincial or international),[89] labour market entry decisions on the extensive margin, and tax evasion (because I rely on *reported* income to represent real income). Third, the reform period used to estimate these elasticities took place fifteen years ago, and since then both the Income Tax Act and labour force have changed. Applying these tax elasticities to forecasts today, while more appropriate than using U.S. elasticities, nonetheless represents an out-of-sample prediction and ought to be done with caution. Finally, the definition of income in this paper is of income reported on the T1 form. As shown in Wolfson et al. (2016), among controlling owners of a Canadian-controlled private corporation (CCPC), income that flows into a corporation that is not paid out as dividends would be real economic income for that individual, which does not show up in the T1 records (LAD). For such individuals, I would understate their income, and overstate their METR because the tax rate they *effectively* face on the retained income in a given year is much lower than the METR they would pay on that income if it were paid out as dividends. Furthermore, TONI would have no impact on the METR of income earned within a corporation that is not paid out; with a zero change in tax rate, we should of course expect no tax-planning or behavioural response.[90]

Rather than pose the problem facing the government as one in which it chooses statutory tax rates optimally in response to some exogenously given elasticity, we could think of the government as influencing the proportion of the elasticity that is within its span of control (e.g. non-real responses). We can do this because the elasticity itself is a function of the tax legislation the government writes and enforces. This could include eliminating sophisticated tax-planning technologies such as earning business income through trusts. Such measures would refine the set of opportunities to save on taxes to fewer response margins such as real labour supply responses, reporting income outside of Canada, or even tax evasion. While it is arguable that the government may not want to raise the relative profile of tax evasion within the tax planning toolkit, eliminating well-known loopholes would have the benefit of simplifying the tax code and removing the grey area between what constitutes avoidance versus evasion. Under these conditions we would expect headline statutory rates to have a greater meaning, or more “bite”, in the budget decisions of tax-filers, and would therefore expect the public debate surrounding elasticities to have greater meaning as well.

[89] I assume tax-filers optimize with respect to their own-jurisdiction tax rate and the tax rates of other jurisdictions are not included in the tax-filers objective function. In other words, I am not estimating a model of tax competition.
[90] A more comprehensive model of tax-filer behaviour would calculate a combined personal-corporate METR to account for the effective incentives faced by individuals with access to CCPCs.

7 Tables and Figures

Table 1. TONI reform implementation and tax bracket indexation status by province and year

Year	CAN	NL	PE	NS	NB	QC[b]	ON	MB	SK	AB[d]	BC
2000	indexed[a]	TOT	TOT	TONI	TONI	indexed	TONI	TONI	TOT	TOT	TONI
2001	indexed	TONI	TONI	constant	indexed	constant	indexed	indexed	TONI	TONI	indexed
2002	indexed	constant	constant	constant	indexed	indexed	indexed	constant	constant	no brackets	indexed
2003	indexed	constant	constant	constant	indexed	indexed	indxed	constant	constant[c]	no brackets	indexed
2004	indexed	constant	constant	constant	indexed	indexed	indexed	constant	indexed	no brackets	indexed
2005	indexed	constant	constant	constant	indexed	indexed	indexed	constant	indexed	no brackets	indexed

Notes: The purpose of this table is twofold. First, to indicate the year in which each province implemented TONI; second, to indicate whether tax bracket thresholds were indexed thereafter. The constant/indexed status is determined by comparing the nominal value of the bracket threshold in the reference year to the previous year. Any modest increase in the threshold is considered to be "indexing" even if it does not follow a formal rule. TOT indicates last year province used tax-on-tax system; TONI indicates year province implemented TONI reform. Source of province-year provincial bracket thresholds: CTaCS parameter database v2012-1 Milligan (2012).[a] The federal government reintroduced indexation of tax brackets in 2000; inspection of archived federal Schedule 1 forms reveals that the threshold for entry into the second tax bracket had been fixed at a value of $29,590 since 1992. [b] QC did not complete the TONI reform as it was already applying its own tax rates to a definition of income.[c] There was a major reform of the bracket thresholds in SK this year. [d]AB used a flat tax upon implementing TONI in 2001; therefore, AB did not have progressive tax brackets.

Table 2. Timing of elections, tax reform announcements, and tax reform events for the four provinces with greatest tax cuts over the sample period

Province	Government status before and after announcement(s)	Announcement month	Major cuts (>4 p.p.) apply in tax year	TONI implementation
BC	1996 (NDP-maj), 2001(LIB-maj)	April 2001 (Liberal campaign document).	2001	2000
AB	1997(PC-maj), 2001(PC-maj)	March 1999 Budget	2001	2001
SK	1999(NDP-min), 2003 (NDP-maj)	March 2000 Budget	2001	2001
NL	1999(LIB-maj), 2003(PC-maj)	November 16, 1999 Press Release	2000, 2001	2001

Notes: The Election Years column provides the timing of all provincial elections around the time of the TONI reform for the four provinces selected. "maj" indicates party winning election won a majority; "min" indicates minority. The cuts in tax year 2001 in BC were announced mid-year as the election took place in late spring 2001. Sources for the information in the above table are from: Kesselman (2002), McMillan (2000), Alberta Treasury Board (2000), Saskatchewan Department of Finance (2000), Newfoundland and Labrador (2000).

Table 3. Mean values of percentage point changes in *predicted* METR by pairs of observed years and province

Spacing	Year Pair	NL	PE	NS	NB	QC	ON	MB	SK	AB	BC
1	1999-2000	-2.0%	-1.3%	-0.8%	-1.2%	-1.7%	-1.6%	-1.2%	-2.0%	-1.6%	-1.5%
	2000-2001	-2.9%	-2.1%	-1.8%	-2.3%	-3.3%	-2.8%	-2.4%	-2.9%	-3.4%	-4.4%
	2001-2002	0.0%	0.0%	0.1%	-0.2%	-1.4%	-0.6%	-0.7%	-0.3%	1.0%	-1.8%
	2002-2003	-0.1%	0.2%	0.3%	0.1%	-0.1%	-0.3%	-0.6%	-1.0%	0.0%	0.0%
	2003-2004	-0.6%	-0.5%	-0.9%	-0.5%	-0.7%	-0.2%	-1.2%	-0.7%	-0.6%	-0.5%
2	1999-2001	-4.4%	-3.6%	-3.1%	-3.8%	-4.9%	-4.5%	-3.3%	-4.8%	-4.9%	-5.9%
	2000-2002	-2.5%	-2.4%	-1.8%	-2.8%	-4.5%	-3.4%	-2.7%	-3.5%	-2.5%	-6.2%
	2001-2003	-0.2%	0.0%	0.2%	-0.1%	-1.2%	-0.3%	-1.1%	-1.3%	0.9%	-1.8%
	2002-2004	-0.4%	-0.4%	-0.9%	-0.4%	-0.8%	-0.3%	-1.5%	-1.5%	-0.7%	-0.6%
3	1999-2002	-4.4%	-3.6%	-3.1%	-4.0%	-6.2%	-4.9%	-3.7%	-5.3%	-3.8%	-7.5%
	2000-2003	-2.5%	-2.4%	-2.2%	-2.9%	-4.5%	-3.5%	-2.9%	-4.4%	-2.6%	-6.3%
	2001-2004	-0.6%	-0.6%	-0.8%	-0.8%	-1.8%	-0.6%	-1.8%	-1.9%	0.3%	-2.3%

Notes: Values represent the *mean* percentage point change in the *predicted* METRs between various pairs of years for each province. ‘Predicted’ refers to the variation in METRs generated by the instrument described in Section 4.1. The predicted METR is the METR that would result if the tax-filer had no change in real income. “Spacing” refers to the number of years separating observations used in the first-differences specification. The baseline specification in [2] uses a two-year spacing, ie. (*t-2 and t*).The statistics apply to a sample that is subjected to all of the sample restrictions in Table 11. For the two-year spacing, this sample is therefore about 6.1 million observations.

Table 4. Mean values of percentage point changes in *predicted* METR by decile and province for the 1999-2001 year pair

Decile	NL	PE	NS	NB	QC	ON	MB	SK	AB	BC
1	-2.0%	-1.0%	-0.9%	-1.4%	-4.2%	-1.4%	-0.4%	-0.8%	-0.1%	-2.0%
2	-1.8%	-0.8%	-0.7%	-1.2%	-3.9%	-1.3%	-0.2%	0.2%	0.8%	-1.8%
3	-3.9%	-2.8%	-2.1%	-3.4%	-4.5%	-3.7%	-2.8%	-1.4%	-0.4%	-4.9%
4	-5.5%	-5.7%	-4.0%	-5.5%	-5.3%	-5.0%	-4.2%	-4.7%	-4.6%	-6.1%
5	-5.5%	-5.4%	-3.7%	-4.7%	-4.9%	-4.7%	-4.1%	-5.4%	-5.3%	-6.1%
6	-6.0%	-5.7%	-4.2%	-5.1%	-5.4%	-5.3%	-4.7%	-6.9%	-6.1%	-6.6%
7	-6.0%	-5.7%	-4.3%	-5.1%	-5.7%	-5.4%	-4.8%	-8.2%	-6.4%	-6.7%
8	-6.1%	-6.2%	-4.4%	-5.2%	-5.8%	-6.1%	-4.9%	-8.8%	-7.0%	-7.5%
9	-6.8%	-6.1%	-4.8%	-5.9%	-5.8%	-6.7%	-5.6%	-9.0%	-8.3%	-9.1%
10	-6.1%	-4.0%	-3.7%	-4.8%	-4.9%	-4.3%	-4.4%	-7.7%	-8.0%	-7.9%

Notes: The values represent the mean percentage point change in *predicted* METRs between 1999 and 2001 for each province and total income decile. 'Predicted' refers to the variation in METRs generated by the instrument described in section 4.1. Deciles are calculated based on the same sample as in the 1999-2001 row in Table 3, about 6.1 million observations. Deciles are defined by the national (Canada-wide) thresholds listed in Table 9.

Table 5. Mapping of LAD variables into CTaCS variables

CTaCS Variable	Description	2012 Line	LAD Variable	Year Available	Exact	CL
addded	COMPOSITE VARIABLE – SEE DETAIL BELOW	256	** See below **			
adoptex	Adoption expenses	313	adexp	2005-	yes	
age	age	301	age__	1982-	yes	
caregiver	Caregiver claim. Reported line 236 income.	315	careg	1998-	yes	
cginc	Capital gains income	127	clkgx	1982-	yes	
chartex	Qualifying children art and culture expenses	370	none	2011-		
chfitex	Qualifying children sport expenses	365	cfa__	2007-	yes	
cqpinc	CPP/QPP income	114	cqpp_	1982-	yes	
dcexp	daycare expenses	214	ccexd	1982-	yes	
disabled	disability status	316, 215	disdn	1983-	no	yes
dmedexp	dependent medical expenses.	331	mdexc, grsmd	1984-, 1984-	no	yes
dongift	charitable donations and gifts	349	cdonc	1983-	yes	
dues	Union dues or professional association fees	212	dues_	1982-	yes	
dvdinc	Dividend income (Eligible Dividend Income from 2006 on)	120	xdiv_	1982-	yes	
dvdincne	Not Eligible Dividend income (Matters 2006 on)	180		2006-		
earn	Earned income	101	t4e__, oei__	1982-, 1982-	yes	
equivsp	Spousal equivalent dependant. Reported line 236 income	305	eqmar, spsnic, neticp	1993-, -	yes	
fullstu	Number of months full time student	322	edudc	1995-	no	
gisspainc	GIS and SPA income	146, 235, 250	nfsl_	1992-	no	
id	identification variable		lin__	1982-	yes	
infdep	Infirm dependant age 18+. Reported line 236 income	306, 5820	apxmp, eqmar, neticp	1982-, 1993-	no	
intinc	interest income	121	invi_	1982-	yes	
kidage1	Age of the youngest child	306	kid1_	1982-	yes	
kidage2	Age of the 2nd youngest child	306	kid2_	1982-	yes	
kidage3	Age of the 3rd youngest child	306	kid3_	1982-	Yes	
kidage4	Age of the 4th youngest child	306	kid4_	1982-	Yes	
kidage5	Age of the 5th youngest child	306	kid5_	1982-	Yes	
kidage6	Age of the 6th youngest child	306	kid6_	1982-	Yes	
kidcred	Credits transferred from child's return	327	edudt, disdo	1995-, 1986-	No	
male	Reference person is male		sxco_	1982-	Yes	
mard	marital status		mstco	1982-	Yes	
medexp	medical expenses	330	grsmd	1984-	Yes	
north	Proportion of the year resided in area eligible for Northern Deduction	255	nrdn_	1987-	No	
northadd	Proportion of the year eligible for additional residency amount of Northern Deduction.	256	nrdn_	1987-	No	
oasinc	OAS income	113	oasp_	1982-	Yes	
othcredf	Other non-refundable credits federal	313				
othcredp	Other non-refundable credits provincial	5833				
othded*	COMPOSITE VARIABLE – SEE DETAIL BELOW	256	** See below **	1988-	Yes	
othinc*	COMPOSITE VARIABLE – SEE DETAIL BELOW	130	** See below **			

CTaCS Variable	Description	2012 Line	LAD Variable	Year Available	Exact	CL
partstu	Number of months part time student	321	edupt	1999-	No	
peninc	Pension / RPP income	115	sop4a	1982-	Yes	
political	political contributions	409	fplcg	1982-	Yes	
politicalp	political contributions - provincial	6310	pplc_	1982-1997	Yes	
proptax	Property tax payments for provincial credit		none			
province	province of residence		prco_	1982-	Yes	
pubtrex	Qualifying public transit expenses	364	ptpa_	2006-	Yes	
qmisded	Quebec miscellaneous deductions before Taxable Income	[ ]			Yes	
qothded	Quebec other deductions before Net Income	[ ]			Yes	
rent	Rent payments for property tax credits	6110	none			
rppcon	RPP contributions	207	t4rp_	1986-	Yes	
rrspcon	RRSP contributions	208	rrspc		Yes	
rrspinc	RRSP income	129	t4rsp, rrspo	1988-	No	
sainc	social assistance income	145, 250	saspy	1992-	Yes	
schinc	Scholarship income	130	none			
self	self-employment income	135	sei__	1982-	Yes	
spaddded	Additional deductions before Taxable Income	256				
spage	age	301	age__	1982-	Yes	
spcginc	Capital gains income	127	Clkgx	1982-	Yes	
spcqpinc	CPP/QPP income	114	cqpp_	1982-	Yes	
spdisabled	disability status	316, 215	Disdn	1983-	No	Yes
spdues	Union dues or professional association fees	212	dues_	1982-	Yes	
spdvdinc	Dividend income (post 2006: eligible only)	120	xdiv_	1982-	Yes	
spdvdincne	Dividend income - not eligible	180		2006-		
spearn	Earned income	101	t4e__, oei__	1982-, 1982-	yes	
spfullstu	Number of months full time student	322	edudc	1995-	no	
spgisspainc	GIS and SPA income	146, 235, 250	nfsl_	1992-	no	
spintinc	interest income	121	invi_	1982-	yes	
spmale	spouse person is female	0	sxco_	1982-	yes	
spoasinc	OAS income	113	oasp_	1982-	yes	
spothcredf	Other non-refundable credits federal	313				
spothcredp	Other non-refundable credits provincial	5833				
spothded	Other deductions before Net Income	256		1988-	yes	
spothinc	all other sources of income	130				
sppartstu	Number of months part time student	321	edupt	1999-	No	
sppeninc	RPP / other pension income	115	sop4a	1982-	Yes	
sppolitical	political contributions	409	fplcg	1982-	Yes	
sppoliticalp	political contributions - provincial	6310	pplc_	1982-1997	Yes	
spqmisded	Quebec miscellaneous deductions before Taxable Income	[ ]			Yes	
spqothded	Quebec other deductions before Net Income	[ ]			Yes	
sprppcon	RPP contributions	207	t4rp_	1986-	Yes	
sprrspcon	RRSP contributions	208	rrspc		Yes	

CTaCS Variable	Description	2012 Line	LAD Variable	Year Available	Exact	CL
sprrspinc	RRSP income	129	t4rsp, rrspo	1988-	No	
spsainc	social assistance income	145, 250	saspy	1992-	Yes	
spschinc	Scholarship income	130	none			
spself	self-employment income	135	sei__	1982-	Yes	
spstuloan	Interest on student loan	319	loanc	1999-	Yes	
spteachex	Teaching supply expenditures (for PEI credit)	0	none			
sptuition	Tuition paid	320	tutdn	1982-	Yes	
spuiinc	Unemployment insurance income	119	eins_	1982-	Yes	
spvolfire	Volunteer firefighter etc.	362	none			
spwcinc	Workers' compensation income	144, 250	wkcpy	1992-	yes	
stuloan	Interest on student loan	319	loanc	1999-	yes	
teachex	Teaching supply expenditures (for PEI credit)		none			
tuition	Tuition paid	320	tutdn	1982-	yes	
Uiinc	Unemployment insurance income	119	eins_	1982-	yes	
volfire	Volunteer firefighter etc.	362	none			
Wcinc	Workers' compensation income	144, 250	wkcpy	1992-	Yes	
COMPOSITE VARIABLES						
addded	Additional deductions before Taxable Income	256				
addded	Non capital losses of other years	252	nklpy	1984-	yes	
addded	Stock option benefit deduction	249	stkdn	1984-	yes	
addded	Capital gains exemption	254	ggex_	1986-	yes	
addded	Employee home relocation	248	hrldn	1986-	yes	
addded	Social benefits repayment	235	rsbcl	1989-	yes	
addded	Other payments deduction	250	DERIVE	n/a	no	
addded	Net federal supplements	146	nfsl_	1992-	yes	
addded	Canadian forces personnel and police	244	cfpdn	2004-	yes	Yes
addded	Net capital losses of other years	253	klpyc	1983-	yes	
addded	Universal child care benefit	117	uccb_	2006-	yes	
addded	Universal child care benefit repayment	213	uccbr	2007-	yes	
addded	Registered Disability savings plan	125	rdsp_	2008-	yes	
addded	Additional deductions before Taxable Income	256	odnni	1988-		
addded	Limited partnership losses of other years	251	ltplp	1991-	yes	
othded	Other deductions before Net Income	232				
othded	Moving expenses	219	mvexp	1986-	yes	
othded	Clergy residence deduction	231	clrgy	1999-	yes	
othded	Attendant care expenses / disability supports	215	acexp	1989-	yes	
othded	Universal child care benefit repayment	213	uccbr	2007-	yes	
othded	Exploration and development expense	224	cedex	1988-	yes	
othded	Carrying charges and interest expenses	221	cycgi	1986-	yes	

CTaCS Variable	Description	2012 Line	LAD Variable	Year Available	Exact	CL
othded	Other deductions before Net Income	232	odn			
othded	Deduction for elected split pension amount	210	espad	2007-	yes	
othded	Allowable business investment loss (ABIL)	217	klcbc	1988-	yes	
othded	Support payments made	220, 230	almdc, talip	1997-,1998-	yes	
othded	CPP paid on self-employment income	222	cppse, ppip_	2002-,2006-	yes	yes
othded	All other expenses	229	alexp	1982-	yes	
othinc	all other sources of income	130				
othinc	Universal child care benefit	117	uccb_	2006-	yes	
othinc	Registered Disability savings plan	125	rdsp_	2008-	yes	
othinc	Taxable Support payments received	128, 156	almi_, talir	1986-, 1998-	yes	
othinc	Other income	130	oi___	1982-	yes	
othinc	Limited net partnership income	122	ltpi	1988-	yes	
othinc	Rental income	126	rnet_	1982-	yes	
othinc	Taxable capital gains	127	clkgl	1982-	yes	yes

Notes: Not all variables provided for in CTaCS could be computed using the available information in LAD. The detailed Stata code file in which all LAD variables were converted into CTaCS variables, with assumptions, is available upon request. Composite variables refer to "catch-all" or subtotalled CTaCS variables into which more detailed administrative variables can be included. The headings in the above table are as follows:

CL: a variable that affects the constant-law assumption. That is, legislation changed the definition within the sample period 1999-2004 resulting in artificial bias of the tax base definition.

Exact: indicates whether or not the LAD variable can be entered into CTaCS "as-is" or if it requires some modification to meet the CTaCS definition.

Year available: indicates years that each variable is available in the LAD database.

2012 line: as a frame of reference, refers to the line number of the item within the 2012 T1 General forms.

LAD variable: administrative name of LAD variable. See Statistics Canada (2012) for the data dictionary.

CTaCSvariable: administrative name of tax calculator variable. See Milligan (2012) for tax calculator documentation.

Table 6. Means and standard deviations for key variables in Table 12 regression

Variable	Mean	Standard Deviation
Year 1 total income	$ 58,400	$ 104,500
Year 1 taxable income	$ 52,400	$ 94,800
Year 1 wage & salary income	$ 49,200	$ 85,500
Absolute change in total income	$ 1,800	$ 96,900
Absolute change in taxable income	$ 1,800	$ 87,600
Absolute change in wage and salary incomes	$ 660	$ 78,900
Percentage point tax cut	- 0.019	0.062
Percentage point tax cut (IV)	- 0.024	0.037
Year 1 age	43	9.39
Flag: Self-employment income in Year 1	0.08	0.28
Number of kids	1.12	1.10
Married or Common Law	0.73	0.44

Notes: Summary statistics based on the sample described in the last row of Table 11, a set of differenced observations with two years between each year. The self-employment flag indicates tax-filers with self-employment income of at least $100 in the tax year. The mean tax cut is around 2% because the sample includes pairs of years in which there were few significant tax cuts such as the period between 2002 and 2004. All dollar values are in 2004 Canadian dollars. All dollar values are rounded in accordance with the LAD confidentiality rules.

Table 7. Real values of key variables over sample period, by tax year and tax bracket of last dollar of income

Variable	Year	MTR 29 & 26	MTR 22	MTR 15
Total Income	1999	129,600	50,700	15,200
	2000	130,300	50,500	15,000
	2001	132,500	50,400	15,300
	2002	130,600	50,600	15,200
	2003	128,200	50,200	15,100
	2004	140,300	52,900	15,900
Taxable Income	1999	116,100	45,700	12,300
	2000	116,500	45,700	12,200
	2001	119,900	45,900	12,500
	2002	118,800	46,200	12,500
	2003	116,400	45,900	12,500
	2004	126,300	48,200	13,200
Employment Income*	1999	92,200	39,700	8,300
	2000	94,500	39,600	8,300
	2001	96,500	39,400	8,400
	2002	95,700	39,600	8,300
	2003	94,900	39,300	8,300
	2004	101,800	41,600	9,000
METR	1999	49.4%	42.6%	18.7%
	2000	48.0%	40.7%	18.1%
	2001	44.0%	36.8%	17.4%
	2002	43.5%	36.4%	17.1%
	2003	43.4%	36.4%	17.2%
	2004	43.8%	36.2%	17.9%

Notes: The mean values in the table are drawn from the *full sample* of about 28m shown in row 2 of Table 11. The only restriction is that tax-filers living in one of the three territories are excluded. Thus, the category MTR15 includes individuals who paid no tax. The 29% MTR did not exist in 1999 and 2000; it is imputed by back-casting and deflating the 2001 cut-off. All income values have been converted into 2004 dollars using a CPI deflator. Tax brackets used are the federal statutory brackets, and are used as an indicator of place within the taxable income distribution. Both total and taxable income values shown are those that are produced by the tax calculator, minus taxable capital gains. The METR shown is the actual METR in each cell, not the predicted value using the instrument. *Employment income does not include self-employment.

Table 8. Income Statistics by Income Group

Income group	Statistic	1999	2000	2001	2002	2003	2004
Top 0.01	Percentage in the same quantile last year	45.6	42.8	39.7	43.9	51.1	48.4
Top 0.1	Percentage in the same quantile last year	61.0	58.0	56.7	60.3	63.4	63.3
Top 1	Percentage in the same quantile last year	71.9	71.1	70.8	72.1	73.5	74.2
Top 5	Percentage in the same quantile last year	77.2	76.2	76.5	77.5	78.4	79.0
Top 10	Percentage in the same quantile last year	81.3	80.1	80.5	81.7	82.3	82.6
Top 50	Percentage in the same quantile last year	89.7	89.7	90.0	90.4	90.6	90.6
Top 0.01	Share of federal and provincial or territorial income taxes paid	2.7	3.1	2.9	2.8	2.8	2.9
Top 0.1	Share of federal and provincial or territorial income taxes paid	7.9	8.8	8.6	8.3	8.2	8.4
Top 1	Share of federal and provincial or territorial income taxes paid	20.2	21.5	21.5	21.1	20.9	21.4
Top 5	Share of federal and provincial or territorial income taxes paid	38.4	39.7	39.8	39.5	39.3	39.8
Top 10	Share of federal and provincial or territorial income taxes paid	51.9	53.0	53.0	53.0	52.9	53.1
Top 50	Share of federal and provincial or territorial income taxes paid	95.4	95.7	95.7	95.9	96.0	95.9
Top 0.01	Share of income	1.4	1.6	1.5	1.3	1.4	1.4
Top 0.1	Share of income	3.8	4.3	4.2	3.9	3.9	4.1
Top 1	Share of income	10.4	11.2	11.1	10.8	10.8	11.1
Top 5	Share of income	23.1	23.9	24.0	23.7	23.7	24.1
Top 10	Share of income	34.2	35.0	35.0	34.8	34.8	35.2
Top 50	Share of income	82.9	83.2	83.0	83.1	83.2	83.2
Top 0.01	Threshold value (thousands of current dollars)	$ 1,881	$ 2,401	$ 2,288	$ 2,232	$ 2,197	$ 2,418
Top 0.1	Threshold value (thousands of current dollars)	$ 469	$ 532	$ 557	$ 548	$ 555	$ 598
Top 1	Threshold value (thousands of current dollars)	$ 137	$ 146	$ 154	$ 156	$ 160	$ 168
Top 5	Threshold value (thousands of current dollars)	$ 73	$ 77	$ 79	$ 81	$ 83	$ 86
Top 10	Threshold value (thousands of current dollars)	$ 58	$ 60	$ 62	$ 64	$ 65	$ 68
Top 50	Threshold value (thousands of current dollars)	$ 21	$ 21	$ 22	$ 23	$ 23	$ 24

Notes: Source of table is CANSIM 204-0001 (accessed Nov 6, 2015). All dollar values are in current dollars. “Top” categories are based on Statistics Canada definition of total income as defined in CANSIM 204-0001 notes, and do not align with income groupings / deciles used in this paper.

Table 9. Threshold values for total income deciles used in regression results

Decile	CAN	NL	PE	NS	NB	QC	ON	MB	SK	AB	BC
1	$ 20,000	$ 20,000	$ 20,000	$ 20,000	$ 20,000	$ 20,000	$ 20,000	$ 20,000	$ 20,000	$ 20,000	$ 20,000
2	$ 26,400	$ 24,300	$ 23,800	$ 25,000	$ 24,600	$ 25,400	$ 27,500	$ 25,100	$ 25,700	$ 27,300	$ 27,100
3	$ 31,400	$ 27,900	$ 27,200	$ 28,900	$ 28,100	$ 29,700	$ 33,100	$ 29,100	$ 30,100	$ 33,200	$ 32,500
4	$ 35,900	$ 31,200	$ 30,200	$ 32,900	$ 31,600	$ 33,500	$ 38,100	$ 32,900	$ 34,000	$ 38,400	$ 37,400
5	$ 40,800	$ 34,900	$ 33,500	$ 37,300	$ 35,500	$ 37,700	$ 43,300	$ 36,900	$ 38,400	$ 44,000	$ 42,100
6	$ 46,100	$ 39,400	$ 37,100	$ 42,300	$ 40,000	$ 42,500	$ 49,000	$ 41,400	$ 43,200	$ 50,200	$ 47,300
7	$ 52,400	$ 44,700	$ 41,600	$ 48,000	$ 45,500	$ 47,900	$ 55,900	$ 46,600	$ 49,000	$ 57,500	$ 53,300
8	$ 60,200	$ 51,200	$ 47,400	$ 54,600	$ 51,700	$ 54,800	$ 64,400	$ 53,300	$ 56,300	$ 66,800	$ 60,700
9	$ 70,500	$ 59,400	$ 55,100	$ 62,900	$ 59,900	$ 64,200	$ 75,000	$ 61,600	$ 64,100	$ 79,000	$ 69,800
10	$ 89,300	$ 74,700	$ 68,900	$ 79,000	$ 75,500	$ 79,900	$ 95,900	$ 76,000	$ 79,500	$ 103,200	$ 86,900

Notes: Cut-off values are generated from the baseline sample in the final row of Table 11;thus,the lower bound of the first decile is $20,000. For regression results involving deciles and splines in this paper, I use the "CAN" values as the threshold values. Provincial values are shown for comparison. These "deciles" are different from familiar national definitions to divide the population such as those found in CANSIM Table 204-0001 (see Table 8) which include low-income observations. All values have been rounded to the nearest $100 in accordance with the confidentiality rules of the LAD. All dollars values are in 2004 Canadian dollars.

Table 10. Alternative choices of income deflator/inflator: price-based vs. income-based

Year	CPI index	INCOME index	$\Delta[defl_y/defl_{(y+1)}]$	$\Delta[defl_y/defl_{(y+2)}]$	$\Delta[defl_y/defl_{(y+3)}]$
1999	0.89	0.84	0.023	$\boxed{0.034}$	0.034
2000	0.9	0.87	0.012	0.012	0.022
2001	0.93	0.91	0.000	0.011	0.020
2002	0.95	0.93	0.011	0.020	-
2003	0.97	0.96	0.010	-	-
2004	1	1	-	-	-

Notes: The national CPI deflator values presented above are from CANSIM Table 326-0021, using the "all-items CPI". The income deflator is generated using the Income Statistics Division (ISD) definition of total income (xtirc), which is equal to Line 150 total income minus – dividend gross-up – capital gains + refundable tax credits + other non-taxable income. The Δ variables demonstrate the ***difference*** in deflator value that would result from using an income, rather than CPI deflator, for the year-spacing possibilities of 1, 2 and 3, represented with subscripts y+1, y+2 and y+3 respectively. For example, by using an income deflator to compare real values between 1999 and 2001, the formula yields: (0.84/0.91)= 0.923. For a CPI deflator, the formula yields: (0.89/0.93)=0.957. The difference between the two values is 0.034, as shown in the highlighted box in the table above. The larger value of the CPI deflator in all cases implies that it reduces nominal incomes by less than would an income inflator. Nominal values in the paper are calculated using provincial CPI deflators to account for regional movements in nominal values, not the national CPI shown above.

Table 11.Sample selection assumptions for baseline model

Item	**Change**	**Remaining Sample**	**Row ID**
Individuals			
Starting Sample	-	28,190,948	1
Less: Territory, missing province	156,331	28,034,617	2
Differenced	-	18,420,226	3
Less: Missing data in year t or year *t-2*	992,011	17,428,215	4
Less: MTR in year *t*-2 or *t* not in (0,1)	26,142	17,402,073	5
Less: MTR instrument not in (0,1)	19,268	17,382,805	6
Less: Moved province	284,854	17,097,951	7
Less: Changed marital status	1,251,313	15,846,638	8
Less: Age less than 25	1,974,680	13,871,958	9
Less: Age greater than 61	3,252,794	10,619,164	10
Less: Pays tax less than $1000 in year *t-2*	3,267,382	7,351,782	11
Less: Total income less than $20,000 in year *t-2*	756,749	6,595,033	12
Less: Total income less than $20,000 in year *t*	517,057	6,077,976	13

Notes: All frequencies are raw, unweighted LAD sample counts over the years 1999 to 2004 inclusive. "Differenced" refers to converting the data from individual-year observations to all possible combinations of first-difference observations with two calendar years between years. For example, for an individual present in the LAD in all six years from 1999 to 2004, six individual records become four records, one in each of: 1999-2001, 2000-2002, 2001-2003, and 2002-2004. Note that multiplying the value in row 2 by (6/4) is only slightly less than the value in row 3, indicating an almost perfectly-balanced panel. All "change" values reflect step-wise deletion of records. Year *t-2* and year *t* refer to the first and second year in a first-difference specification. Starting sample represents six years of LAD data, starting with 4.5m observations in 1999, and increasing to 4.8m in 2004.

Table 12. Elasticity of taxable and total Income: baseline second-stage results

	Taxable Income				Total Income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
change in log (1-τ)	-0.1400***	0.0339***	0.0340***	0.0340	-0.1155***	0.0231***	0.0263***	0.0263
	(0.0029)	(0.0037)	(0.0036)	(0.0410)	(0.0026)	(0.0031)	(0.0031)	(0.0366)
log of base year(*t-2*) income	-0.0947***				-0.0765***			
	(0.0002)				(0.0002)			
year *t-2* capital income	0.0004***	0.0001***	0.0002***	0.0002***	-0.0002***	-0.0003***	-0.0002***	-0.0002**
	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0001)
year *t-2* age	0.0002**	0.0000	-0.0025***	-0.0025***	-0.0013***	-0.0013***	-0.0036***	-0.0036***
	(0.0001)	(0.0001)	(0.0001)	(0.0005)	(0.0001)	(0.0001)	(0.0001)	(0.0004)
year *t-2* age squared	-0.0000***	-0.0000***	0.0000***	0.0000	-0.0000***	-0.0000***	0.0000***	0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
self-employment dummy	0.0022***	-0.0098***	0.0170***	0.0170***	0.0068***	0.0005	0.0264***	0.0264***
	(0.0003)	(0.0003)	(0.0004)	(0.0027)	(0.0003)	(0.0003)	(0.0004)	(0.0037)
number of kids	0.0047***	0.0039***	0.0039***	0.0039***	0.0039***	0.0034***	0.0035***	0.0035***
	(0.0001)	(0.0001)	(0.0001)	(0.0005)	(0.0001)	(0.0001)	(0.0001)	(0.0004)
married dummy	0.0001	-0.0005**	-0.0008***	-0.0008	0.0001	0.0004*	0.0002	0.0002
	(0.0002)	(0.0002)	(0.0002)	(0.0011)	(0.0002)	(0.0002)	(0.0002)	(0.0007)
male	0.0199***	0.0198***	0.0270***	0.0270***	0.0139***	0.0138***	0.0222***	0.0222***
	(0.0002)	(0.0002)	(0.0002)	(0.0023)	(0.0002)	(0.0002)	(0.0002)	(0.0021)
base year 2000 dummy	-0.0196***	-0.0172***	-0.0170***	-0.0170***	-0.0204***	-0.0186***	-0.0184***	-0.0184***
	(0.0003)	(0.0003)	(0.0003)	(0.0032)	(0.0002)	(0.0002)	(0.0002)	(0.0028)
base year 2001 dummy	-0.0242***	-0.0129***	-0.0125***	-0.0125***	-0.0205***	-0.0115***	-0.0110***	-0.0110***
	(0.0003)	(0.0004)	(0.0003)	(0.0037)	(0.0003)	(0.0003)	(0.0003)	(0.0036)
base year 2002 dummy	-0.0256***	-0.0142***	-0.0135***	-0.0135***	-0.0179***	-0.0090***	-0.0082***	-0.0082*
	(0.0003)	(0.0004)	(0.0004)	(0.0039)	(0.0003)	(0.0003)	(0.0003)	(0.0045)
Spline Variables								
spline 1		-0.4100***	-0.4196***	-0.4196***		-0.4138***	-0.4311***	-0.4311***
		(0.0022)	(0.0022)	(0.0161)		(0.0027)	(0.0027)	(0.0187)
spline 2		-0.2782***	-0.2990***	-0.2990***		-0.2243***	-0.2437***	-0.2437***
		(0.0034)	(0.0034)	(0.0222)		(0.0033)	(0.0032)	(0.0086)
spline 3		-0.1592***	-0.1741***	-0.1741***		-0.1542***	-0.1737***	-0.1737***
		(0.0047)	(0.0046)	(0.0241)		(0.0044)	(0.0044)	(0.0343)
spline 4		-0.1606***	-0.1812***	-0.1812***		-0.1149***	-0.1346***	-0.1346***
		(0.0055)	(0.0054)	(0.0342)		(0.0045)	(0.0045)	(0.0120)

			Taxable Income				Total Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
spline 5		-0.0706***	-0.0831***	-0.0831***		-0.0143***	-0.0270***	-0.0270**
		(0.0055)	(0.0054)	(0.0216)		(0.0048)	(0.0047)	(0.0125)
spline 6		-0.0498***	-0.0623***	-0.0623***		-0.0485***	-0.0632***	-0.0632***
		(0.0050)	(0.0049)	(0.0080)		(0.0044)	(0.0044)	(0.0051)
spline 7		-0.0299***	-0.0490***	-0.0490***		-0.0270***	-0.0435***	-0.0435***
		(0.0044)	(0.0044)	(0.0043)		(0.0040)	(0.0040)	(0.0093)
spline 8		-0.0469***	-0.0635***	-0.0635***		-0.0212***	-0.0406***	-0.0406***
		(0.0038)	(0.0038)	(0.0061)		(0.0035)	(0.0035)	(0.0046)
spline 9		-0.0718***	-0.0839***	-0.0839***		-0.0626***	-0.0708***	-0.0708***
		(0.0029)	(0.0029)	(0.0140)		(0.0025)	(0.0025)	(0.0114)
spline 10		0.0035***	0.0081***	0.0081		-0.0077***	-0.0016*	-0.0016
		(0.0010)	(0.0010)	(0.0055)		(0.0009)	(0.0009)	(0.0053)
Industry Dummies								
Agriculture, Forestry, Fishing and Hunting			0.0208***	0.0208*			0.0166***	0.0166*
			(0.0009)	(0.0120)			(0.0008)	(0.0096)
Mining, Quarrying, and Oil and Gas Extraction			0.1139***	0.1139***			0.1039***	0.1039***
			(0.0009)	(0.0165)			(0.0008)	(0.0141)
Utilities			0.1231***	0.1231***			0.1127***	0.1127***
			(0.0009)	(0.0098)			(0.0008)	(0.0084)
Construction			0.0635***	0.0635***			0.0583***	0.0583***
			(0.0006)	(0.0049)			(0.0005)	(0.0029)
Manufacturing			0.0578***	0.0578***			0.0530***	0.0530***
			(0.0004)	(0.0069)			(0.0004)	(0.0041)
Wholesale Trade			0.0635***	0.0635***			0.0599***	0.0599***
			(0.0005)	(0.0061)			(0.0005)	(0.0037)
Retail Trade			0.0403***	0.0403***			0.0361***	0.0361***
			(0.0005)	(0.0048)			(0.0005)	(0.0032)
Transportation and Warehousing			0.0609***	0.0609***			0.0616***	0.0616***
			(0.0006)	(0.0058)			(0.0005)	(0.0039)
Information and Cultural Industries			0.0868***	0.0868***			0.0823***	0.0823***
			(0.0007)	(0.0067)			(0.0006)	(0.0045)
Finance and Insurance			0.0885***	0.0885***			0.0854***	0.0854***
			(0.0006)	(0.0066)			(0.0005)	(0.0041)
Real Estate and Rental and Leasing			0.0684***	0.0684***			0.0643***	0.0643***
			(0.0009)	(0.0058)			(0.0008)	(0.0037)
Professional, Scientific and Technical Services			0.0887***	0.0887***			0.0810***	0.0810***

	Taxable Income				Total Income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			(0.0006)	(0.0056)			(0.0005)	(0.0034)
Management of Companies and Enterprises			0.0755***	0.0755***			0.0704***	0.0704***
			(0.0012)	(0.0070)			(0.0011)	(0.0054)
Administrative and Support, Waste Management and Remediation Services			0.0395***	0.0395***			0.0354***	0.0354***
			(0.0007)	(0.0046)			(0.0006)	(0.0025)
Educational Services			0.0881***	0.0881***			0.0854***	0.0854***
			(0.0005)	(0.0050)			(0.0004)	(0.0044)
Health Care and Social Assistance			0.0658***	0.0658***			0.0677***	0.0677***
			(0.0005)	(0.0063)			(0.0004)	(0.0055)
Arts, Entertainment and Recreation			0.0438***	0.0438***			0.0413***	0.0413***
			(0.0010)	(0.0047)			(0.0010)	(0.0037)
Accommodation and Food Services			0.0104***	0.0104***			0.0097***	0.0097***
			(0.0008)	(0.0036)			(0.0007)	(0.0022)
Other Services (except Public Administration)			0.0444***	0.0444***			0.0442***	0.0442***
			(0.0006)	(0.0050)			(0.0006)	(0.0036)
Public Administration			0.0886***	0.0886***			0.0877***	0.0877***
			(0.0005)	(0.0074)			(0.0004)	(0.0058)
Not associated to T4 slip			0.0684***	0.0684***			0.0643***	0.0643***
			(0.0007)	(0.0062)			(0.0006)	(0.0045)
Constant	1.0943***	4.2960***	4.3751***	4.3751***	0.9415***	4.3846***	4.5419***	4.5419***
	(0.0028)	(0.0221)	(0.0220)	(0.1639)	(0.0026)	(0.0277)	(0.0276)	(0.1881)
Spline in year (t-2) income	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Industry dummies	No	No	Yes	Yes	No	No	Yes	Yes
Errors Clustered at province level	No	No	No	Yes	No	No	No	Yes
N	5,616,976	5,616,976	5,616,976	5,616,976	5,568,168	5,568,168	5,568,168	5,568,168
First-stage F statistic	-	-	-	282	-	-	-	254

Notes: The first-stage F-statistic is reported in the last row of the table. The exclusion restriction is the predicted change in log (1-τ) as described in Section 4.1. The definition of *year t-2 income*,either represented as a single variable or as a spline, is the same as the dependent variable. Deciles used to form the spline function are calculated by dividing the sample into ten equal groups according to the year *t-2* value of the income definition used in the regression (ie. either total income or taxable income). In all cases, the sample restrictions applied to the sample are the same as in Table 11, plus those in Section 4.2. All year *t-2* values of taxable income less than $100 have been dropped. Such small values are not appropriate to use in a log-ratio operator to represent approximations in percent change. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13. Elasticity of taxable income: By decile of total income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
change in log (1-τ)	-0.2565*	0.0154	0.0908***	-0.0192	-0.1457***	-0.1152***	-0.0419	0.1054	0.0553	0.0236
	(0.1463)	(0.0970)	(0.0303)	(0.0209)	(0.0354)	(0.0359)	(0.0666)	(0.0683)	(0.0426)	(0.1191)
log of base year(*t-2*) income	-0.4452***	-0.4294***	-0.4645***	-0.4459***	-0.4269***	-0.4157***	-0.3990***	-0.3716***	-0.2769***	-0.0342***
	(0.0060)	(0.0124)	(0.0189)	(0.0175)	(0.0223)	(0.0183)	(0.0146)	(0.0147)	(0.0103)	(0.0035)
year *t-2* capital income	-0.0004**	-0.0007***	-0.0008***	-0.0009***	-0.0006***	-0.0007***	-0.0007***	-0.0007***	-0.0005***	0.0001
	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0003)
year *t-2* age	-0.0093***	-0.0087***	-0.0077***	-0.0064***	-0.0052***	-0.0029***	-0.0018**	-0.0002	0.0037***	0.0075***
	(0.0003)	(0.0004)	(0.0008)	(0.0003)	(0.0004)	(0.0006)	(0.0007)	(0.0004)	(0.0005)	(0.0009)
year *t-2* age squared	0.0001***	0.0001***	0.0001***	0.0001***	0.0000***	0.0000*	-0.0000	-0.0000***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
self-employment dummy	0.0229***	0.0004	-0.0125***	-0.0138***	-0.0150***	-0.0150***	-0.0049	0.0102***	0.0271***	0.0499***
	(0.0038)	(0.0024)	(0.0027)	(0.0041)	(0.0041)	(0.0028)	(0.0042)	(0.0038)	(0.0057)	(0.0091)
number of kids	0.0002	0.0036***	0.0053***	0.0051***	0.0047***	0.0054***	0.0045***	0.0041***	0.0036***	0.0019**
	(0.0011)	(0.0008)	(0.0010)	(0.0007)	(0.0004)	(0.0003)	(0.0004)	(0.0005)	(0.0004)	(0.0008)
married dummy	-0.0051***	-0.0037**	-0.0031*	-0.0040**	-0.0035***	-0.0038**	-0.0018***	0.0020	0.0072***	0.0133***
	(0.0012)	(0.0017)	(0.0018)	(0.0017)	(0.0008)	(0.0015)	(0.0003)	(0.0019)	(0.0016)	(0.0016)
male	0.0319***	0.0271***	0.0251***	0.0257***	0.0237***	0.0216***	0.0214***	0.0183***	0.0221***	0.0222***
	(0.0021)	(0.0038)	(0.0047)	(0.0037)	(0.0031)	(0.0022)	(0.0018)	(0.0011)	(0.0020)	(0.0024)
base year 2000	-0.0096***	-0.0112***	-0.0148***	-0.0141***	-0.0173***	-0.0178***	-0.0140**	-0.0169***	-0.0221***	-0.0376***
	(0.0023)	(0.0021)	(0.0025)	(0.0028)	(0.0031)	(0.0031)	(0.0059)	(0.0050)	(0.0042)	(0.0045)
base year 2001	-0.0164***	-0.0099***	-0.0100***	-0.0113***	-0.0208***	-0.0187***	-0.0132	-0.0004	-0.0097**	-0.0441***
	(0.0049)	(0.0036)	(0.0028)	(0.0038)	(0.0022)	(0.0032)	(0.0085)	(0.0035)	(0.0042)	(0.0103)
base year 2002	-0.0153***	-0.0084**	-0.0096***	-0.0130**	-0.0236***	-0.0235***	-0.0165**	-0.0059	-0.0114***	-0.0361***
	(0.0051)	(0.0035)	(0.0031)	(0.0052)	(0.0030)	(0.0044)	(0.0083)	(0.0037)	(0.0034)	(0.0096)
constant	4.7802***	4.6205***	4.9854***	4.8091***	4.6330***	4.5059***	4.3230***	4.0147***	2.9256***	0.2109***
	(0.0579)	(0.1294)	(0.2114)	(0.1915)	(0.2410)	(0.1881)	(0.1500)	(0.1572)	(0.1212)	(0.0325)
Lower threshold of total income value of decile	$20,000	$26,400	$31,400	$35,900	$40,800	$46,100	$52,400	$60,200	$70,500	$89,300
N	560,545	571,425	567,605	573,605	579,925	573,170	560,710	570,635	570,200	489,165
First-stage F statistic	87.7097	130.8993	688.5875	215.2227	481.6839	104.0257	29.7944	164.2371	100.8388	263.3783

Notes: The regression specification [2] is estimated on ten different total income groups (deciles), defined by the lower cut-offs shown in the third last row of the table. The 10^{th}decile has the smallest sample because those with income of $250,000 and greater have been excluded (see Section 5.4). All of the notes in Table 12apply to this table. All estimations in the above table include the full set of industry dummies (not shown) from Table 12. All standard errors are clustered at the province level. Standard errors in parentheses: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 14. Elasticity of total income: By decile of total income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
change in log (1-τ)	-0.2569*	-0.0544	0.0130	0.0297	0.0935***	0.0614*	0.1105	0.1142**	0.1475***	0.1295
	(0.1533)	(0.1063)	(0.0334)	(0.0249)	(0.0249)	(0.0360)	(0.0778)	(0.0505)	(0.0405)	(0.1107)
log of base year(*t-2*) income	-0.4526***	-0.2574***	-0.1681***	-0.1383***	-0.0162***	-0.0593***	-0.0489***	-0.0406***	-0.0675***	-0.0064**
	(0.0198)	(0.0229)	(0.0413)	(0.0117)	(0.0040)	(0.0032)	(0.0090)	(0.0052)	(0.0101)	(0.0030)
year *t-2* capital income	0.0005*	-0.0000	-0.0001	-0.0002***	-0.0003***	-0.0003***	-0.0004***	-0.0004***	-0.0005***	0.0000
	(0.0002)	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0003)
year *t-2* age	-0.0088***	-0.0079***	-0.0064***	-0.0052***	-0.0039***	-0.0022***	-0.0011	-0.0000	0.0029***	0.0064***
	(0.0006)	(0.0006)	(0.0007)	(0.0003)	(0.0005)	(0.0008)	(0.0010)	(0.0006)	(0.0008)	(0.0008)
year *t-2* age squared	0.0001***	0.0001***	0.0001***	0.0000***	0.0000***	0.0000	-0.0000	-0.0000***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
self-employment dummy	0.0506***	0.0293***	0.0149***	0.0119***	0.0105***	0.0075**	0.0160**	0.0265***	0.0341***	0.0380***
	(0.0022)	(0.0021)	(0.0031)	(0.0035)	(0.0040)	(0.0034)	(0.0068)	(0.0057)	(0.0068)	(0.0084)
number of kids	0.0008	0.0036***	0.0052***	0.0053***	0.0044***	0.0046***	0.0034***	0.0026***	0.0020***	0.0003
	(0.0012)	(0.0006)	(0.0008)	(0.0006)	(0.0003)	(0.0004)	(0.0004)	(0.0005)	(0.0006)	(0.0004)
married dummy	0.0018**	0.0003	-0.0017*	-0.0034***	-0.0023**	-0.0027**	-0.0015***	0.0020	0.0073***	0.0174***
	(0.0009)	(0.0007)	(0.0010)	(0.0011)	(0.0009)	(0.0012)	(0.0004)	(0.0018)	(0.0011)	(0.0015)
male	0.0291***	0.0240***	0.0232***	0.0224***	0.0215***	0.0187***	0.0180***	0.0143***	0.0178***	0.0207***
	(0.0024)	(0.0039)	(0.0046)	(0.0037)	(0.0026)	(0.0019)	(0.0018)	(0.0012)	(0.0020)	(0.0019)
base year 2000	-0.0109***	-0.0126***	-0.0169***	-0.0163***	-0.0140***	-0.0163***	-0.0135**	-0.0190***	-0.0224***	-0.0343***
	(0.0020)	(0.0020)	(0.0024)	(0.0027)	(0.0029)	(0.0037)	(0.0058)	(0.0059)	(0.0040)	(0.0037)
base year 2001	-0.0165***	-0.0107***	-0.0127***	-0.0081*	0.0002	-0.0052	-0.0015	0.0007	0.0002	-0.0257***
	(0.0047)	(0.0034)	(0.0028)	(0.0046)	(0.0029)	(0.0051)	(0.0096)	(0.0061)	(0.0048)	(0.0087)
base year 2002	-0.0148***	-0.0084**	-0.0103**	-0.0076	0.0035	-0.0034	-0.0010	-0.0008	0.0045	-0.0104
	(0.0048)	(0.0037)	(0.0043)	(0.0069)	(0.0049)	(0.0071)	(0.0096)	(0.0059)	(0.0050)	(0.0082)
constant	4.8922***	2.8786***	1.9155***	1.5650***	0.2258***	0.6600***	0.5050***	0.3765***	0.6048***	-0.0939*
	(0.1972)	(0.2290)	(0.4117)	(0.1123)	(0.0467)	(0.0464)	(0.1000)	(0.0687)	(0.1307)	(0.0481)
Lower threshold of total income value of decile	\$20,000	\$26,400	\$31,400	\$35,900	\$40,800	\$46,100	\$52,400	\$60,200	\$70,500	\$89,300
N	555,097	568,621	565,385	571,862	577,120	569,573	556,618	565,879	563,113	474,900
First-stage F statistic	80.8301	125.2021	1467.7776	262.1423	247.6361	96.2710	28.5802	175.9435	132.6594	161.6617

Notes: The regression specification [2] is estimated on ten different total income groups (deciles), defined by the lower cut-offs shown in the third last row of the table. The 10^{th}decile has the smallest sample because those with income of \$250,000 and greater have been excluded (see Section 5.4). All of the notes in Table 12 apply to this table. All estimations in the above table include the full set of industry dummies (not shown) from Table 12. All standard errors are clustered at the province level. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 15. Elasticities by income source: by decile of total income

	Decile 1	Decile 2	Decile 3	Decile 4	Decile 5	Decile 6	Decile 7	Decile 8	Decile 9	Decile 10
Employment Income	-0.1901	-0.0843*	-0.0212	-0.0414***	-0.0709**	-0.0899***	-0.0699**	0.0404*	0.0691	0.0683
Standard Error	(0.1290)	(0.0485)	(0.0243)	(0.0087)	(0.0337)	(0.0309)	(0.0277)	(0.0223)	(0.0443)	(0.0715)
N	461,932	493,802	502,745	512,969	520,139	525,091	529,315	533,150	528,922	457,249
Total Income	-0.2569*	-0.0544	0.0130	0.0297	0.0935***	0.0614*	0.1105	0.1142**	0.1475***	0.1295
Standard Error	(0.1533)	(0.1063)	(0.0334)	(0.0249)	(0.0249)	(0.0360)	(0.0778)	(0.0505)	(0.0405)	(0.1107)
N	555,097	568,621	565,385	571,862	577,120	569,573	556,618	565,879	563,113	474,900
Net income	-0.2337*	0.0089	0.0966***	0.0066	-0.1261***	-0.0966**	-0.0306	0.1160*	0.0659	0.0387
Standard Error	(0.1419)	(0.1003)	(0.0311)	(0.0204)	(0.0385)	(0.0428)	(0.0794)	(0.0683)	(0.0424)	(0.1210)
N	560,095	571,180	567,395	573,435	579,685	572,885	560,435	570,335	569,765	487,505
Taxable Income	-0.2565*	0.0154	0.0908***	-0.0192	-0.1457***	-0.1152***	-0.0419	0.1054	0.0553	0.0236
Standard Error	(0.1463)	(0.0970)	(0.0303)	(0.0209)	(0.0354)	(0.0359)	(0.0666)	(0.0683)	(0.0426)	(0.1191)
N	560,545	571,425	567,605	573,605	579,925	573,170	560,710	570,635	570,200	489,165
Lower threshold of total income value of decile	$20,000	$26,400	$31,400	$35,900	$40,800	$46,100	$52,400	$60,200	$70,500	$89,300

Notes: The regression specification [2] is estimated on ten different total income groups (deciles), defined by the lower cut-offs shown in the third last row of the table. The 10thdecile has the smallest sample because those with income of $250,000 and greater have been excluded (see Section 5.4). All of the notes in Table 12 apply to this table. All estimations in the above table include the full set of industry dummies (not shown) from Table 12. Total and net income definitions used are net of taxable capital gains. Taxable income is net of capital gains and net (added back) of applicable capital losses. First-stage F-statistics are not shown for net income and employment income; for other two definitions see Table 13 and Table 14. All standard errors are clustered at the province level. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 16. Elasticity of taxable income of Decile 10: robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)
change in log (1-τ)	0.0236	0.0833	0.0778	0.1138	0.0810	-0.0630
	(0.1191)	(0.1111)	(0.1149)	(0.1130)	(0.1202)	(0.1839)
log of base year (*t-2*) income	-0.0342***					
	(0.0035)					
year *t-2* capital income	0.0001					
	(0.0003)					
year *t-2* age	0.0075***	0.0072***	0.0071***	0.0075***	0.0070***	0.0070***
	(0.0009)	(0.0008)	(0.0008)	(0.0009)	(0.0009)	(0.0009)
year *t-2* age squared	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
self-employment dummy	0.0499***	0.0465***	0.0149**	0.0142**	0.0089	0.0167**
	(0.0091)	(0.0091)	(0.0076)	(0.0067)	(0.0087)	(0.0080)
number of kids	0.0019**	0.0024***	0.0021***	0.0020**	0.0016**	0.0024***
	(0.0008)	(0.0007)	(0.0007)	(0.0008)	(0.0007)	(0.0007)
married dummy	0.0133***	0.0133***	0.0133***	0.0156***	0.0134***	0.0123***
	(0.0016)	(0.0017)	(0.0017)	(0.0018)	(0.0020)	(0.0020)
male	0.0222***	0.0208***	0.0226***	0.0224***	0.0241***	0.0216***
	(0.0024)	(0.0022)	(0.0023)	(0.0023)	(0.0029)	(0.0027)
base year 2000	-0.0376***	-0.0369***	-0.0366***	-0.0349***	-0.0353***	-0.0412***
	(0.0045)	(0.0043)	(0.0044)	(0.0041)	(0.0051)	(0.0042)
base year 2001	-0.0441***	-0.0386***	-0.0387***	-0.0314***	-0.0386***	-0.0510***
	(0.0103)	(0.0098)	(0.0101)	(0.0096)	(0.0108)	(0.0127)
base year 2002	-0.0361***	-0.0301***	-0.0303***	-0.0260***	-0.0305***	-0.0424***
	(0.0096)	(0.0092)	(0.0094)	(0.0090)	(0.0098)	(0.0111)
<u>Spline Variables (total income)</u>						
spline 1		-0.0919***	-0.0991***	-0.0819***	-0.0982***	-0.0830***
		(0.0121)	(0.0140)	(0.0177)	(0.0181)	(0.0185)
spline 2		-0.1186**	-0.1213**	-0.0890	-0.1386**	-0.1269**
		(0.0494)	(0.0487)	(0.0554)	(0.0545)	(0.0537)
spline 3		-0.2780***	-0.2780***	-0.3103***	-0.2953***	-0.2766***
		(0.0267)	(0.0272)	(0.0447)	(0.0243)	(0.0358)
spline 4		0.0214	0.0166	-0.0010	0.0085	0.0012

	(1)	(2)	(3)	(4)	(5)	(6)
		(0.0220)	(0.0201)	(0.0432)	(0.0250)	(0.0210)
spline 5		-0.0113	-0.0135	-0.0016	-0.0058	-0.0447
		(0.0355)	(0.0353)	(0.0401)	(0.0428)	(0.0310)
spline 6		-0.0230	-0.0281	-0.0177	-0.0406	-0.0230
		(0.0382)	(0.0383)	(0.0292)	(0.0506)	(0.0282)
spline 7		-0.0117	-0.0136	-0.0451	-0.0218	0.0216
		(0.0299)	(0.0297)	(0.0343)	(0.0326)	(0.0240)
spline 8		0.0022	-0.0048	0.0145	0.0017	-0.0331*
		(0.0244)	(0.0244)	(0.0293)	(0.0288)	(0.0184)
spline 9		0.0203	0.0119	0.0069	0.0139	0.0099
		(0.0131)	(0.0133)	(0.0129)	(0.0161)	(0.0195)
spline 10		0.0137	0.0070	0.0135	0.0104	0.0065
		(0.0120)	(0.0131)	(0.0150)	(0.0148)	(0.0126)
Spline Variables (capital income)						
spline 1-5 (capital income)		0.0011***	0.0011***	0.0008***	0.0011***	0.0012***
		(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
spline 6 (capital income)		0.0004	0.0002	-0.0014	0.0013	-0.0004
		(0.0013)	(0.0013)	(0.0018)	(0.0009)	(0.0016)
spline 7 (capital income)		0.0021	0.0018	0.0003	0.0014	0.0037***
		(0.0020)	(0.0020)	(0.0015)	(0.0024)	(0.0006)
spline 8 (capital income)		0.0086***	0.0082***	0.0130***	0.0084**	0.0063***
		(0.0030)	(0.0031)	(0.0033)	(0.0039)	(0.0022)
spline 9 (capital income)		-0.0161***	-0.0165***	-0.0272***	-0.0152***	-0.0171***
		(0.0026)	(0.0029)	(0.0046)	(0.0029)	(0.0037)
spline 10 (capital income)		-0.0197***	-0.0223***	-0.0201***	-0.0216***	-0.0214***
		(0.0016)	(0.0014)	(0.0020)	(0.0018)	(0.0017)
major income source = pension			0.0927***	0.0971***	0.0926***	0.0881***
			(0.0078)	(0.0069)	(0.0097)	(0.0060)
major income source = self-employment			0.0548***	0.0484***	0.0587***	0.0530***
			(0.0122)	(0.0112)	(0.0133)	(0.0146)
major income source = CCPC-source income			0.0158***	0.0172***	0.0124***	0.0157***
			(0.0047)	(0.0049)	(0.0040)	(0.0053)

	(1)	(2)	(3)	(4)	(5)	(6)
constant	0.2109***	0.8688***	0.9214***	0.7090***	0.9102***	0.7606***
	(0.0325)	(0.1169)	(0.1350)	(0.1849)	(0.1769)	(0.1731)
Splines of year *t-2* total income and capital income *within* top decile	No	Yes	Yes	Yes	Yes	Yes
Dummies for major source of income	No	No	Yes	Yes	Yes	Yes
Exclude those with capital gains in either *t-2* or *t*	No	No	No	Yes	No	No
Drop Quebec	No	No	No	No	Yes	No
Drop British Columbia	No	No	No	No	No	Yes
N	489,165	489,165	489,165	375,858	402,037	436,934

Notes: The sample used in the regressions above is Decile 10, the same sample used in Table 15.All estimations in the above table include the full set of industry dummies (not shown) from Table 12. Total and net income definitions used are net of taxable capital gains. Taxable income is net of capital gains and net (added back) of applicable capital losses. The robustness check introduced in column 4 is concerned with tax-filers who have capital gains. A tax-filer is considered to have capital gains in either year *t-2* or year *t* if he or she has at least $100 (as a de minimis rule). Major source of income is calculated by comparing four sources and choosing the greatest value: paid worker employment, pension, self-employment, CCPC-sourced. Paid worker employment is the excluded group. All standard errors are clustered at the province level. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 17. Elasticities of taxable income for progressively increasing lower thresholds of total income

	P90+	P91+	P92+	P93+	P94+	P95+	P96+	P97+	P98+	P99+
change in log (1-τ)	0.0663	0.0788	0.0945	0.0991	0.1096**	0.0868	0.0051	-0.0228	0.0183	0.0832
	(0.0948)	(0.0823)	(0.0707)	(0.0630)	(0.0556)	(0.0582)	(0.0660)	(0.0815)	(0.0817)	(0.1167)
log of base year (*t-2*) income	-0.0191***	-0.0179***	-0.0168***	-0.0158***	-0.0143***	-0.0133***	-0.0138***	-0.0130***	-0.0155***	-0.0194***
	(0.0019)	(0.0022)	(0.0024)	(0.0019)	(0.0018)	(0.0015)	(0.0015)	(0.0012)	(0.0015)	(0.0028)
year *t-2* capital income	0.0002	0.0002	0.0003	0.0003	0.0003	0.0004*	0.0004*	0.0004*	0.0004**	0.0009***
	(0.0003)	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
year *t-2* age	0.0074***	0.0075***	0.0078***	0.0083***	0.0086***	0.0086***	0.0089***	0.0087***	0.0086***	0.0072***
	(0.0008)	(0.0006)	(0.0007)	(0.0006)	(0.0006)	(0.0004)	(0.0005)	(0.0006)	(0.0013)	(0.0019)
year *t-2* age squared	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
self-employment dummy	0.0491***	0.0492***	0.0489***	0.0487***	0.0481***	0.0457***	0.0438***	0.0406***	0.0345***	0.0301***
	(0.0083)	(0.0083)	(0.0083)	(0.0081)	(0.0080)	(0.0084)	(0.0080)	(0.0080)	(0.0067)	(0.0048)
number of kids	0.0019**	0.0019**	0.0019**	0.0022***	0.0021***	0.0023***	0.0020***	0.0018***	0.0012	-0.0005
	(0.0008)	(0.0008)	(0.0008)	(0.0007)	(0.0008)	(0.0007)	(0.0007)	(0.0006)	(0.0007)	(0.0012)
married dummy	0.0125***	0.0127***	0.0131***	0.0127***	0.0130***	0.0119***	0.0132***	0.0110***	0.0082***	0.0113**
	(0.0016)	(0.0017)	(0.0015)	(0.0016)	(0.0014)	(0.0014)	(0.0017)	(0.0018)	(0.0018)	(0.0044)
male	0.0218***	0.0211***	0.0201***	0.0188***	0.0173***	0.0174***	0.0172***	0.0161***	0.0149***	0.0173***
	(0.0022)	(0.0024)	(0.0028)	(0.0030)	(0.0033)	(0.0033)	(0.0030)	(0.0027)	(0.0023)	(0.0018)
Base year 2000	-0.0382***	-0.0381***	-0.0380***	-0.0376***	-0.0385***	-0.0389***	-0.0412***	-0.0444***	-0.0477***	-0.0522***
	(0.0042)	(0.0041)	(0.0042)	(0.0042)	(0.0043)	(0.0047)	(0.0052)	(0.0056)	(0.0046)	(0.0068)
Base year 2001	-0.0411***	-0.0415***	-0.0425***	-0.0443***	-0.0451***	-0.0473***	-0.0532***	-0.0543***	-0.0521***	-0.0456***
	(0.0084)	(0.0076)	(0.0069)	(0.0065)	(0.0060)	(0.0058)	(0.0067)	(0.0080)	(0.0058)	(0.0065)
Base year 2002	-0.0303***	-0.0296***	-0.0290***	-0.0286***	-0.0277***	-0.0271***	-0.0292***	-0.0255***	-0.0181***	-0.0038
	(0.0073)	(0.0063)	(0.0053)	(0.0048)	(0.0039)	(0.0034)	(0.0037)	(0.0043)	(0.0046)	(0.0066)
Constant	0.0484***	0.0336**	0.0178	-0.0009	-0.0204	-0.0232	-0.0145	-0.0104	0.0319	0.1083***
	(0.0107)	(0.0137)	(0.0154)	(0.0163)	(0.0157)	(0.0145)	(0.0233)	(0.0186)	(0.0340)	(0.0283)
N	531,995	475,570	419,310	363,440	307,845	252,750	198,485	144,965	92,985	43,395
First-stage F statistic	309.0738	258.0343	207.8802	171.2450	139.0820	164.7589	485.7570	3708.6722	6776.6384	9087.9283

Notes: The regression specification [2] is estimated on ten different total income groups *within the top decile*. These income groups are *not mutually exclusive*, but are defined by all tax-filers above a given percentile of total income, *x*, in year *t-2*. Moving from left to right, *x* is increased in each column in one percentile increments starting at the value at the 90th percentile (P90+), ending with the 99th percentile (P99+). Those with income of $250,000 and greater have *been reintroduced* in all columns (see Section5.5). For this reason, the sample size (N) shown for P90+ is greater than the sample size in column 10 of Table 13. All of the notes in Table 12 apply to this table. All estimations in the above table include the full set of industry dummies (not shown) from Table 12. Taxable income is net of capital gains and net (added back) of applicable capital losses. All standard errors are clustered at the province level. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 18. Reproduction of Table 1 from Department of Finance (2010)

	Weighted by taxable income				Weighted by log (taxable income)			
	Top 10	Top 5	Top 2	Top 1	Top 10	Top 5	Top 2	Top 1
change in log (1-τ)	0.0255*	0.0930***	0.2188***	0.5701***	0.0351***	0.0489**	-0.0803*	-0.0501
	(0.0141)	(0.0283)	(0.0603)	(0.1033)	(0.0087)	(0.0190)	(0.0420)	(0.0789)
log of base year (*t-1*) income	-0.1800***	-0.2026***	-0.2328***	-0.2609***	-0.0870***	-0.1058***	-0.1403***	-0.1707***
	(0.0003)	(0.0006)	(0.0010)	(0.0015)	(0.0004)	(0.0008)	(0.0013)	(0.0020)
married dummy	0.0205***	0.0276***	0.0306***	0.0321***	0.0101***	0.0182***	0.0230***	0.0268***
	(0.0007)	(0.0014)	(0.0027)	(0.0046)	(0.0005)	(0.0009)	(0.0018)	(0.0032)
male	0.0544***	0.0713***	0.0977***	0.1262***	0.0282***	0.0400***	0.0543***	0.0730***
	(0.0007)	(0.0013)	(0.0025)	(0.0042)	(0.0004)	(0.0008)	(0.0016)	(0.0029)
age	-0.0003***	-0.0002***	-0.0000	0.0002	-0.0011***	-0.0011***	-0.0008***	-0.0004***
	(0.0000)	(0.0001)	(0.0001)	(0.0002)	(0.0000)	(0.0000)	(0.0001)	(0.0001)
any children	0.0093***	0.0089***	0.0094***	0.0080**	0.0110***	0.0131***	0.0173***	0.0202***
	(0.0006)	(0.0010)	(0.0020)	(0.0032)	(0.0004)	(0.0007)	(0.0014)	(0.0023)
Major income source								
pension	-0.1109***	-0.2108***	-0.3698***	-0.5371***	-0.0591***	-0.1430***	-0.2757***	-0.4335***
	(0.0024)	(0.0056)	(0.0140)	(0.0288)	(0.0014)	(0.0033)	(0.0083)	(0.0181)
capital income	-0.3141***	-0.3633***	-0.4250***	-0.4890***	-0.1527***	-0.1945***	-0.2428***	-0.2938***
	(0.0026)	(0.0041)	(0.0068)	(0.0104)	(0.0021)	(0.0033)	(0.0054)	(0.0084)
self-employment	0.1093***	0.1257***	0.1279***	0.1294***	-0.0039***	0.0258***	0.0558***	0.0829***
	(0.0011)	(0.0017)	(0.0028)	(0.0044)	(0.0009)	(0.0013)	(0.0020)	(0.0030)
any CCPC-source	0.0099***	0.0138***	0.0147***	0.0200***	-0.0209***	-0.0280***	-0.0333***	-0.0309***
	(0.0008)	(0.0012)	(0.0021)	(0.0033)	(0.0006)	(0.0009)	(0.0016)	(0.0025)
other	-0.0432***	-0.0626***	-0.0908***	-0.1370***	-0.0144***	-0.0146***	-0.0035	-0.0189***
	(0.0010)	(0.0020)	(0.0035)	(0.0056)	(0.0007)	(0.0015)	(0.0026)	(0.0042)
Outlier changes								
(TXI/M)<0.5%	-5.8009***	-5.8371***	-5.8546***	-5.8717***	-5.8498***	-5.9059***	-5.8750***	-5.8546***
	(0.0772)	(0.1212)	(0.1996)	(0.3205)	(0.0584)	(0.0871)	(0.1334)	(0.2107)
0.5%<(TXI/M)<1%	-2.9753***	-2.9658***	-2.9686***	-3.0111***	-2.7811***	-2.7349***	-2.6775***	-2.6891***
	(0.0066)	(0.0100)	(0.0159)	(0.0232)	(0.0084)	(0.0122)	(0.0183)	(0.0264)
1%<(TXI/M)<5%	-1.3676***	-1.4070***	-1.4524***	-1.5084***	-1.1810***	-1.2340***	-1.2710***	-1.3336***
	(0.0025)	(0.0041)	(0.0070)	(0.0101)	(0.0023)	(0.0040)	(0.0070)	(0.0108)
95%<(TXI/M)<99%	0.5978***	0.6379***	0.6626***	0.6760***	0.4793***	0.5466***	0.5920***	0.6151***
	(0.0017)	(0.0026)	(0.0042)	(0.0062)	(0.0016)	(0.0023)	(0.0035)	(0.0051)
99%<(TXI/M)<99.9%	0.9103***	0.9474***	0.9610***	0.9655***	0.8837***	0.9852***	1.0238***	1.0511***
	(0.0052)	(0.0076)	(0.0117)	(0.0167)	(0.0054)	(0.0078)	(0.0112)	(0.0151)

	Weighted by taxable income				Weighted by log (taxable income)			
	Top 10	Top 5	Top 2	Top 1	Top 10	Top 5	Top 2	Top 1
(TXI/M)>99.9%	0.8447***	0.9353***	0.9963***	1.0481***	0.6008***	0.8329***	1.0008***	1.1850***
	(0.0058)	(0.0085)	(0.0129)	(0.0184)	(0.0065)	(0.0097)	(0.0142)	(0.0202)
Constant	1.9683***	2.2405***	2.6199***	2.9781***	0.9629***	1.1662***	1.5631***	1.9120***
	(0.0036)	(0.0074)	(0.0134)	(0.0217)	(0.0049)	(0.0090)	(0.0155)	(0.0251)
N	2,382,565	1,064,135	431,605	207,995	2,382,565	1,064,135	431,605	207,995
F statistic	178389.8401	91449.0402	36084.5178	18666.4679	180648.7456	79924.4792	32076.0316	15797.6393

Notes: The regression specification [2] has been modified to match the specification described in Department of Finance (2010).The sample size (N) for Decile 10 in this table is much greater than the corresponding sample size for P90+ in Table 17 because the Department of Finance (2010) uses fewer sample restrictions. See Section 5.5 for a description of these modifications. Income groups are *not mutually exclusive*, but are defined by all tax-filers above a given percentile of total income, defined by the column headings in the table. Taxable income is net of capital gains but not net (added back) of applicable capital losses, as losses are not discussed in the paper. Note that the spacing between years is only *one* in this table, so the base year is defined as *t-1*. Standard errors in parentheses: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 19. Reproduction of Table 1 from Department of Finance (2010) using mutually exclusive income categories

	P90-P95	P95-P98	P98-P99	P99-P99.9	P99.9-P99.99	P99.99+
change in log (1-τ)	0.0164*	0.2688***	0.1070**	0.0275	-0.8671**	1.7270
	(0.0086)	(0.0196)	(0.0430)	(0.0798)	(0.3619)	(1.0717)
log of base year (*t-1*) income	-0.0538***	-0.0224***	-0.0476***	-0.1161***	-0.1990***	-0.6298***
	(0.0027)	(0.0040)	(0.0078)	(0.0034)	(0.0118)	(0.0323)
Constant	0.6085***	0.2343***	0.5083***	1.2693***	2.1238***	8.4604***
	(0.0297)	(0.0459)	(0.0902)	(0.0419)	(0.1635)	(0.5169)
N	1,318,450	632,550	223,600	183,250	22,300	2,450
First-stage F Statistic	97145.1796	43939.2517	16951.3822	13887.1627	1957.2660	612.2561

Notes: The regression specification [2] has been modified to match the specification described in Department of Finance (2010). See Section 5.5 for a description of these modifications. Income groups are mutually exclusive in this table, defined by the column headings in the table. Taxable income is net of capital gains but not net (added back) of applicable capital losses, as losses are not discussed in the paper. All covariates used in Table 18 were included in the estimations in this table. Only key variables are shown here. Note that the spacing between years is only *one* in this table, so the base year is defined as *t-1*. Other covariates are suppressed for confidentiality reasons. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 20. Mean absolute deviation between predicted and actual METR values

		Number of years between observations, s		
Decile	Lower threshold value	1	2	3
1	$ 20,000	2.3%	3.0%	3.5%
2	$ 26,400	2.7%	3.3%	3.7%
3	$ 31,400	3.5%	4.0%	4.3%
4	$ 35,900	3.7%	4.3%	4.6%
5	$ 40,800	2.6%	3.1%	3.2%
6	$ 46,100	1.7%	2.1%	2.4%
7	$ 52,400	2.0%	2.5%	2.9%
8	$ 60,200	2.6%	3.1%	3.5%
9	$ 70,500	2.9%	3.5%	3.7%
10	$ 89,300	1.8%	2.4%	2.5%

Notes: To maintain constancy of the second year for all differenced observations, year *t* is 2002 in all cases. For example, for a year spacing assumption of *three*, the pair of years is (1999,2002). The values in the table represent the mean of the absolute value of the difference between the actual METR in year *t* and the predicted value. As described in Section 4.1, the instrument is based on *year t-s* income where *s* corresponds to the spacing between years represented in each column.

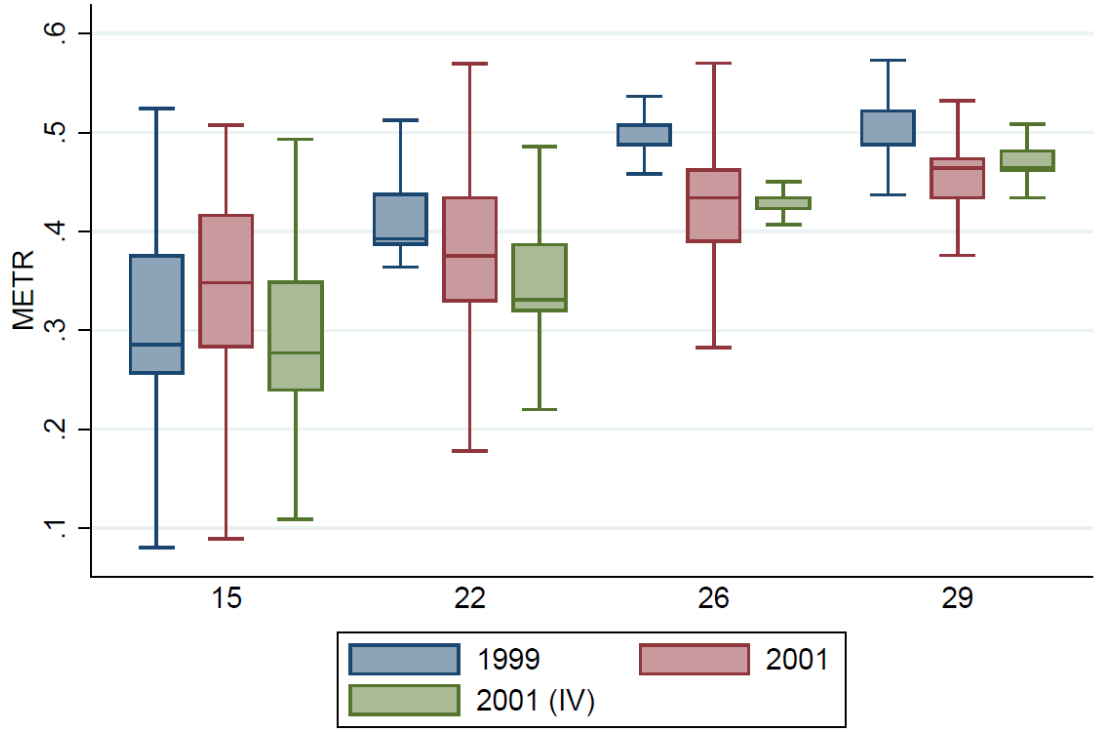
Table 21. Elasticity of taxable income: robustness of year spacing assumption

	Taxable Income			Total Income		
	t-1	t-2	t-3	t-1	t-2	t-3
change in log (1-τ)	-0.0116	0.0340	0.0781	-0.0143	0.0263	0.0702
	(0.0261)	(0.0410)	(0.0543)	(0.0244)	(0.0366)	(0.0477)
Spline Variables						
spline 1	-0.3698***	-0.4196***	-0.4373***	-0.3836***	-0.4311***	-0.4519***
	(0.0132)	(0.0161)	(0.0145)	(0.0200)	(0.0187)	(0.0166)
spline 2	-0.2514***	-0.2990***	-0.3324***	-0.1934***	-0.2437***	-0.2755***
	(0.0249)	(0.0222)	(0.0157)	(0.0132)	(0.0086)	(0.0106)
spline 3	-0.1375***	-0.1741***	-0.2102***	-0.1223***	-0.1737***	-0.2193***
	(0.0075)	(0.0241)	(0.0377)	(0.0160)	(0.0343)	(0.0517)
spline 4	-0.1047***	-0.1812***	-0.2209***	-0.0868***	-0.1346***	-0.1679***
	(0.0196)	(0.0342)	(0.0496)	(0.0088)	(0.0120)	(0.0136)
spline 5	-0.0758***	-0.0831***	-0.0874***	-0.0261***	-0.0270**	-0.0118
	(0.0119)	(0.0216)	(0.0302)	(0.0086)	(0.0125)	(0.0175)
spline 6	-0.0555***	-0.0623***	-0.0610***	-0.0405***	-0.0632***	-0.0737***
	(0.0034)	(0.0080)	(0.0096)	(0.0040)	(0.0051)	(0.0083)
spline 7	-0.0371***	-0.0490***	-0.0592***	-0.0374***	-0.0435***	-0.0546***
	(0.0031)	(0.0043)	(0.0123)	(0.0066)	(0.0093)	(0.0170)
spline 8	-0.0517***	-0.0635***	-0.0912***	-0.0261***	-0.0406***	-0.0668***
	(0.0060)	(0.0061)	(0.0080)	(0.0057)	(0.0046)	(0.0104)
spline 9	-0.0586***	-0.0839***	-0.0940***	-0.0514***	-0.0708***	-0.0768***
	(0.0081)	(0.0140)	(0.0222)	(0.0077)	(0.0114)	(0.0199)
spline 10	0.0027	0.0081	0.0129**	-0.0082**	-0.0016	0.0033
	(0.0045)	(0.0055)	(0.0054)	(0.0042)	(0.0053)	(0.0050)
year 1 capital income	0.0001***	0.0002***	0.0000	-0.0001	-0.0002**	-0.0004***
	(0.0000)	(0.0001)	(0.0000)	(0.0001)	(0.0001)	(0.0001)
year 1 age	-0.0008***	-0.0025***	-0.0034***	-0.0020***	-0.0036***	-0.0044***
	(0.0002)	(0.0005)	(0.0006)	(0.0002)	(0.0004)	(0.0005)
year 1 age squared	-0.0000	0.0000	0.0000	0.0000***	0.0000***	0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
self-employment dummy	0.0067***	0.0170***	0.0224***	0.0143***	0.0264***	0.0365***
	(0.0016)	(0.0027)	(0.0032)	(0.0022)	(0.0037)	(0.0042)
number of kids	0.0017***	0.0039***	0.0052***	0.0017***	0.0035***	0.0042***
	(0.0004)	(0.0005)	(0.0005)	(0.0003)	(0.0004)	(0.0005)

	Taxable Income			Total Income		
	t-1	t-2	t-3	t-1	t-2	t-3
married dummy	-0.0003	-0.0008	-0.0002	0.0004	0.0002	0.0015*
	(0.0008)	(0.0011)	(0.0012)	(0.0005)	(0.0007)	(0.0008)
male	0.0219***	0.0270***	0.0285***	0.0175***	0.0222***	0.0231***
	(0.0018)	(0.0023)	(0.0029)	(0.0017)	(0.0021)	(0.0025)
base year 1999	0.0190***	0.0135***	0.0101**	0.0175***	0.0082*	0.0039
	(0.0029)	(0.0039)	(0.0042)	(0.0030)	(0.0045)	(0.0048)
base year 2000	-0.0012	-0.0035	-0.0043	-0.0045*	-0.0102***	-0.0079***
	(0.0027)	(0.0029)	(0.0029)	(0.0023)	(0.0039)	(0.0024)
base year 2001	-0.0006	0.0009		-0.0041*	-0.0029	
	(0.0019)	(0.0017)		(0.0024)	(0.0022)	
base year 2002	0.0003			-0.0002		
	(0.0019)			(0.0017)		
constant	3.8024***	4.3617***	4.5730***	3.9905***	4.5337***	4.7757***
	(0.1292)	(0.1635)	(0.1517)	(0.2046)	(0.1908)	(0.1680)
N	7,719,151	5,616,976	3,891,644	7,670,257	5,568,168	3,849,089
First-stage F statistic	327.8839	282.1009	310.9480	265.7270	253.5093	280.9718

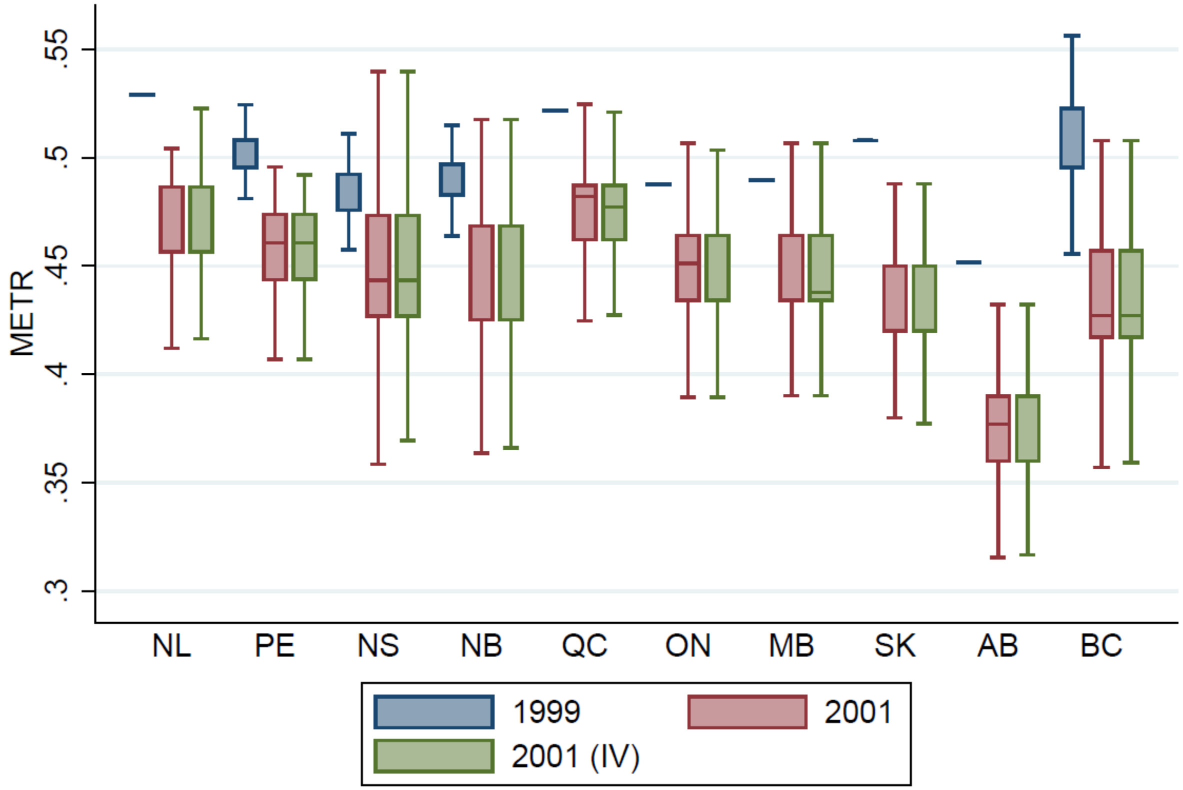
Notes: All of the notes in Table 12 apply to this table. The results in the *t-2* columns of this table are reproductions of the results in the corresponding columns *t-2*from Table 12. Those with income of $250,000 and greater have been excluded in all columns (see Section 5.4). All estimations in the above table include the full set of industry dummies (not shown) from Table 12. Taxable income is net of capital gains and net (added back) of applicable capital losses. The number of year dummies decreases with the spacing between years; in all cases it is the latest (more recent) year that is the omitted year dummy variable. All standard errors are clustered at the province level. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1. Distribution of METRs in 1999 (actual) and in 2001 (actual and predicted (IV)) by federal statutory MTR



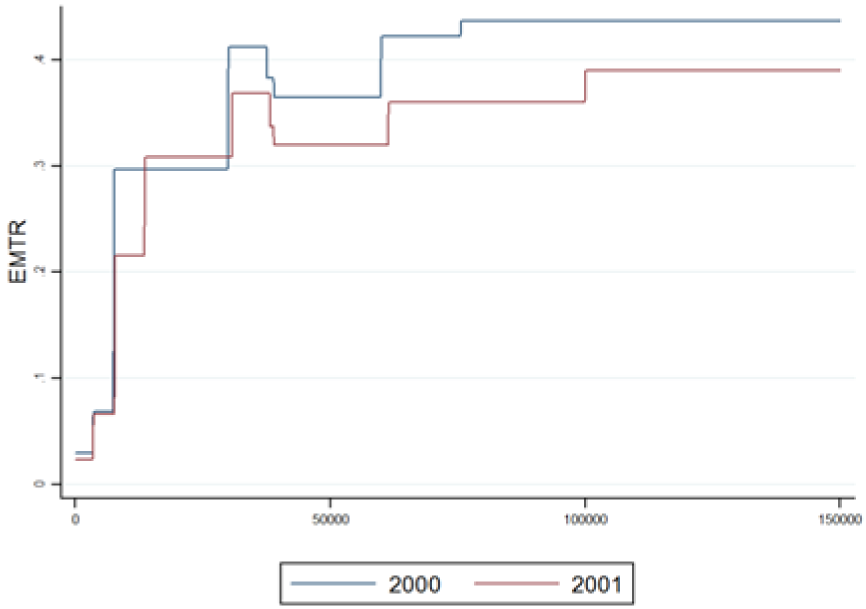
Note: The bottom and top of the boxes represent the 25th and 75th percentile respectively of the observations within each MTR grouping. The horizontal bar through each box represents the median. Federal statutory MTR is determined by comparing each tax-filer's taxable income with the rates in federal Schedule 1 of the T1 General package. These statistics are based on the restricted sample described in Table 11. Only the years 1999 and 2001 are used. All "outside values" beyond the whiskers in each box-whisker plot are suppressed for confidentiality reasons.

Figure 2.Distribution of METRs in 1999 (actual) and in 2001 (actual and predicted (IV)) by province for tax-filers with income in the top decile.



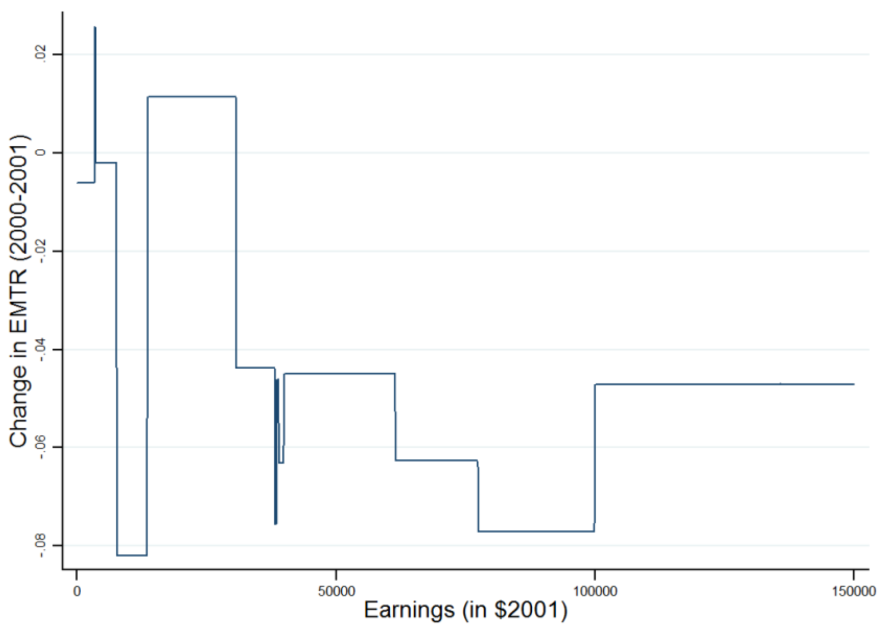
Note: The bottom and top of the boxes represent the 25th and 75th percentile respectively of the observations within each MTR grouping. The horizontal bar through each box represents the median. Federal statutory MTR is determined by comparing each tax-filer's taxable income with the rates in federal Schedule 1 of the T1 General package. These statistics are based on the restricted sample described in Table 11; however in this figure only for tax-filers in the top decile. The cut-off for the top decile is shown in Table 9. Only the years 1999 and 2001 are used. All "outside values" beyond the whiskers in each box-whisker plot are suppressed for confidentiality reasons.

Figure 3. Marginal effective tax rate (METR) by level of employment income for hypothetical Alberta tax-filer in both 2000 and 2001.



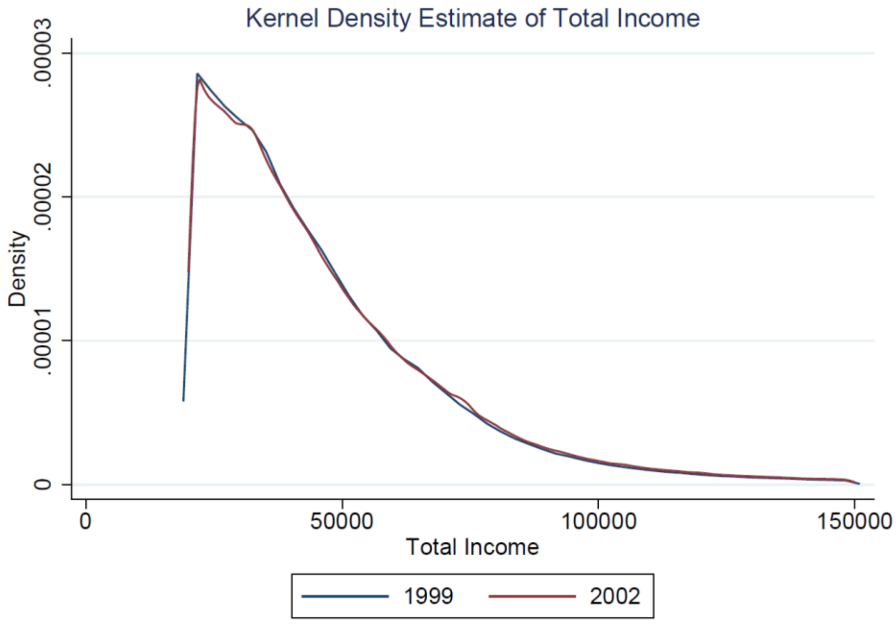
Notes: EMTR/METR simulated using Canadian Tax and Credit Simulator, CTaCS Milligan (2012). Simulation based on a single tax-filer with employment income as only source of income. To calculate each EMTR/METR, I increment the income by $100, recalculating total tax payable each time. All values have been converted to 2001 Canadian dollars.

Figure 4. Percentage point change in METR by level of employment income for hypothetical Alberta tax-filer in both 2000 and 2001.



Notes: EMTR/METR simulated using CTaCS. Simulation based on a single tax-filer with employment income as only source of income. To calculate each EMTR/METR, I increment the income by $100, recalculating total tax payable each time. All values have been converted to 2001 Canadian dollars. Values in this figure are simply the 2001 value minus the 2000 value in Figure 3.

Figure 5. Kernel density of total income distribution for years 1999 and 2002.



Notes: All values in 2004 Canadian dollars. Distribution truncated at $20,000 to cover the same sample as is used in the regression in Table 12. There is a three-year gap between the "before" and "after" years as this is the longest spacing between years I estimate in this paper. Epanechnikov kernel, with bandwidth = 974. Underlying samples are N(1999)=2.3m, and N(2002)=2.5m.

Chapter 2. The Elasticity of Labour Market Earnings: Canadian Evidence from the Tax on Income (TONI) reform of 2000/2001[1]

1 Introduction

The elasticities of income presented in the previous chapter focused primarily on the aggregate definitions of total and taxable income which are common in the literature on tax elasticity. Running regressions on such broad, aggregated definitions of income has the advantage that these definitions are not sensitive to changes in the composition of income. For example, if a tax-filer substitutes between self-employment and regular employment income while maintaining a very similar total income, the dependent variable will remain relatively stable across time. Both forms of income are taxed at the same rate, so if the policy question is to broadly quantify the response of the total income base to changes in tax rates, then such changes in composition are of secondary importance.

If, however, the policy question is to understand which income sources are driving the response to tax rate reform, we should estimate elasticities at the line-item level of detail. The most significant of the income sources that make up total income in Canada is employment income, which represents about two-thirds of total assessed income for tax purposes.[2] Paid workers change their employment income in response to tax reform in two primary ways. First, they can adjust their total hours of work by working more, or less, hours. Second, they can also adjust their *level of effort* on the job for a given amount of hours. In the previous chapter I estimated elasticities of employment income by each decile of the population. The estimated elasticity of employment income for the top decile was 0.07, just over half the magnitude of the corresponding elasticity of 0.13 for total income within the same decile.[3] These values suggest that the employment income elasticity plays an important role in the total income elasticity.[4]

Given that employment income is a product of hours of work and the effective hourly wage rate, in any study estimating employment income elasticities, it is natural to inquire how much of the estimated response is due to changes in hours of work.[5] The LAD data used in Chapter 1, however, do not contain labour market information on hours of work, number of jobs in the year, and whether any jobs are full-time. For this reason, we are forced to speculate on the relative importance of wages and hours in any interpretation of employment income elasticities estimated using the LAD.

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[1] This research was conducted under Research Data Centre contract number 12-SSH-SWO-3332, with principal investigator Anindya Sen.

[2] Source of two-thirds figure is from the 2004 T1 final statistics report produced by the CRA each year (see Canada Revenue Agency (2006); exact estimate is \$531B/\$808B = 65.7%.

[3] Note, the cut-offs for dividing the sample into deciles were based on total income. Many of the tax-filers in the top decile may have very little employment income if they have income from other sources.

[4] A decomposition of the total income elasticity into the elasticity from employment income and that from everything else requires a more formal characterization that includes the relative weights of each type of income in total income. Such a decomposition is discussed in Section 4.2.

[5] Studies estimating the response of labour supply to changes in marginal tax rates number in the hundreds (see Keane (2011) for a comprehensive summary). Many of these studies are estimations of structural models that estimate the labour supply response along a particular margin (intensive or extensive) and for particular sub-groups of the population (such as single mothers with children).

Fortunately, the Survey of Labour and Income Dynamics (SLID) asks respondents a comprehensive set of questions on *both* labour market activity and line item detail from their tax returns. The advantage of the SLID, therefore, is we can estimate an elasticity of employment income, and also estimate the elasticity of hours worked using the *same sample*. This allows for direct inference of the importance of hours in the overall employment income elasticity. The only U.S. study of which we are aware that does something similar is Moffitt and Willhelm (2000), using the Survey of Consumer Finances (SCF), in which they estimate elasticities for both an aggregate measure of income and hours of work using a sample of 406 high income tax-filers. They find modest elasticities of total income (Adjusted Gross Income in the U.S.), but insignificant responses in hours of work, and conclude that the response is primarily due to wages.

In this paper, we further decompose the employment income elasticity results presented in Chapter 1. We do this by making several adjustments to the empirical specification and sample selection that were not possible to do with the LAD data. First, we introduce occupation dummy variables into our specification that were not available in the LAD. Including these data in the empirical specification should reduce bias in the elasticity estimates to the extent changes in taxes are correlated with year-over-year income dynamics for some occupations. Second, we estimate elasticities for tax-filers who have various levels of attachment to the labour force to see if there are significant differences in response. For example, we contrast elasticity estimates for those who have full-time jobs with those who do not. Third, with the information available on hours of work, we estimate a labour supply model and interpret the results alongside the employment income elasticities. Finally, we split our sample by gender and compare our results with previous studies that have estimated labour supply elasticities for women and men separately. Given the SLID's relative advantage for studying labour market responses, and its relative disadvantage for studying very high income earners (discussed more in Section 2.3 below) in *this paper* we focus primarily on the response of employment income and labour supply to changes in tax rates. Specifically, in comparison to Chapter 1, tax planning responses are not expected to play a major role in our reported elasticities.

This chapter is organized as follows. The next section describes the data used. Section 3 outlines the empirical methodology, adapted for employment elasticities. Section 4 contains the results, followed by concluding remarks in Section 5.

2 Data

2.1 Data Sources

All income and labour market data are from the Survey of Labour and Income Dynamics (SLID), a series of six-year overlapping longitudinal panels produced by Statistics Canada over the period 1993 to 2011. We use data from Panel 3 of the SLID, which runs from 1999 to 2004, and therefore covers the TONI reform period that we are interested in. Representing about 17,000 households, there are exactly 43,683 individuals surveyed per year over six years from 1999 to 2004. The full starting sample of individual-year observations, therefore, before any sample restrictions are made, is 262,100. SLID respondents complete an annual phone interview between January and March of each year following the reference year. Respondents are asked several questions about their labour market activity and income during the previous year. Respondents have the option to give Statistics Canada permission to access their income tax records for questions about specific line items in their income tax returns. Eighty percent of respondents permit access to their income tax records.[6] The variables for these records, therefore, constitute “administrative” rather than “survey” data.

The SLID contains rich information on the labour market activity of respondents, much of which was not available in the LAD. Quantitative data include hours of work, hourly wage, number of jobs, and months of continuous employment on the same job. Qualitative data that are relevant to the observed income of tax-filers include: labour market participation status, class of worker, occupation class, industry of employment, part-time vs. full-time status, and highest level of education.[7]

Separate variables for all of the income sources that make up total income are available in the SLID. As with the LAD, to generate a value for total income, we enter each of the individual income components into CTaCS (see Milligan (2012). The CTaCS program applies the appropriate inclusion rate for capital gains income and the appropriate gross-up factor to dividend income to arrive at the accurate definition of total income for tax purposes.[8]

As in Chapter 1, we also use CTaCS to calculate the marginal effective tax rate (METR) for each filer, which determines the effective tax paid on an additional dollar of income.[9] Unlike in Chapter 1, however, the METRs in this paper are overstated for some tax-filers. This is because the SLID does not ask respondents to report some deductions and credits. Failing to include these line items in the tax calculator will overstate the values of taxable income and tax payable respectively.[10] The value of the METR in this paper, therefore, can be thought of as a proxy for the true METR that includes some measurement error.[11]

2.2 Sample restrictions

[6] These respondents authorized Statistics Canada to link their survey, using their Social Insurance Number (SIN), to the T1 Family File (T1FF), provided to Statistics Canada every year by the Canada Revenue Agency. The 80% figure is from the reference file “SLID Overview E.pdf” available to SLID users in the Research Data Centres.

[7] Most of these labour market variables are available annually for the “main job” in the individual file, but in the job file many of these variables are available by job (for up to several jobs in the year) and, in some cases, even by month.

[8] The SLID contains a variable for a Statistics Canada definition of total income that is different from the definition of total income for tax purposes. The former definition includes non-taxable government transfers and excludes capital gains. When we adjust this definition to make it comparable to total income for tax purposes, we find that it is an exact match with the total income generated by CTaCS in over 99% of cases, validating that we used the tax-calculator correctly. We thank Kevin Milligan of UBC for some Stata code files that got us started linking SLID with CTaCS.

[9] Because the SLID surveys a family unit of analysis, we make use of the “spouse” variables in CTaCS and families are entered into the calculator as a family unit. The family unit feature of CTaCS is important for data sources such as SLID, where there are missing tax variables, as it will assign items such as non-refundable credits appropriately to the lower income spouse. I do not use spousal information in LAD as the audited records indicate which spouse claimed each credit. Also, the LAD is a random sample of individual tax-filers, not families, so in most cases I only have data for one spouse. To calculate the METR for each spouse, we hold the income of the other spouse constant, add an additional $100 of labour income, and calculate the marginal tax paid on total family tax payable. See Table 12 in which we vary this $100 increment amount.

[10] Examples of the missing deductions include contributions to personal savings plans (RRSPs), capital losses from other years, employee stock option deductions, and the capital gains deduction. For a list of all variables which are available in SLID and used in our CTaCS calculations, see Table 13.

[11] Although I do not quantify the measurement error, in principle it could be done by re-running my estimates of the METR on LAD after excluding the variables that are not available in SLID.

The SLID is a voluntary survey, and in comparison to the LAD, there are more issues due to non-response and data quality that we must address before we can generate an estimation sample.[12] Table 1 summarizes the sample restrictions we implement to remove respondents from the data for whom there is insufficient information. Beginning with the full sample of 262,100, we lose 85,100 individuals who refused to complete all questions in the survey, or who provided no income information, leaving 177,000 observations. Following this, we drop individuals who are outside of the target population, minors and adult children living at home, leaving 124,700 observations. Next, after running some data quality checks, we elected to drop individuals who only provided partial income information, as well as those who self-report their tax-filing data.[13] Dropping such observations results in an intermediate sample of 109,500 tax-filers for whom income information is complete and accurate. While a substantial amount of sample has been lost compared to the starting sample, note that over 50,000 of these observations were minors or adult children living at home, which are not part of our target population.

2.3 Trends in data: key variables

Based on the above sample, in Table 2 we present mean time-series values by federal tax bracket grouping for a number of key variables: employment income, total income, taxable income, annual paid labour hours, and the METR. Note that the federal tax bracket in which individuals are grouped is defined by the statutory marginal tax rate (MTR) of the tax-filer's last dollar of income.[14] All nominal income concepts have been converted to real 2004 Canadian dollars. The mean value of total income among the tax-filers in the top two tax-brackets held steady at about $107,000 throughout the period in which the majority of tax cuts took place. This mean value is approximately $20,000 less, or 15% less, than the value for this group that I found in Chapter 1 using the LAD. However, for the tax-filers in the 22% tax bracket group, the mean value reported in this chapter is only about $2,500 less, or 5% less, than the value from the LAD sample. Finally, for the group in the bottom tax bracket, the mean value of total income is about $1000 *higher*, or 5% higher, than in the LAD.

If the LAD captures the "true" distribution of income across these groups, then SLID total income is understated in the upper tail and overstated in the lower tail. This property of the SLID data is thoroughly documented in Frenette et al. (2007). The difference between SLID and LAD is much greater within the upper tail of the income distribution. For example, as shown in Table 3, the cut-off for entry into the top decile in SLID is $80,100; the corresponding value using LAD in Chapter 1 was $89,300. For this reason, elasticities presented in this paper should not be considered to include the responses of very high income individuals. This is not necessarily a major problem. The focus of this paper is on estimating *real economic responses* in labour hours and employment income. Very high income tax-filers are less likely

[12] The LAD is a pure random sample of administrative data and therefore "non-response" issues are less of a concern. Of course, some tax-filers can choose not to file their tax return without consequences in some cases, but this typically applies to low income earners who do not owe tax who are excluded from the sample in Chapter 1 anyway.

[13] About 5,900 tax-filers elected to self-report tax information and did not give Statistics Canada permission to use their SIN number to link with their tax records.

[14] Note the distinction between MTR and METR. The former is simply tax rate applied to the last dollar of income in federal Schedule 1 and can be determined simply by knowing a tax-filer's taxable income (with some minor caveats). The METR, on the other hand, usually requires simulation to calculate as it takes into account clawbacks of means-tested income sources, which are *effectively* taxes. For more on the distinction between the two types of taxes in the Canadian context, see Macnaughton et al. (1998).

to respond to taxes through these real channels, as most of them work full-time hours, and many work well in excess of 2000 hours per year (see Moffitt and Willhelm (2000).

The second panel of Table 2 presents the mean values of *taxable income* over time. For the top tax bracket group, these values are only about $10,000 less than with the LAD sample, a narrower difference than is the case with total income. Recall from the discussion above on METRs, however, that this is likely due to the fact that many high income earners claim deductions that are not provided in SLID, and therefore the computed taxable income using SLID data is biased upward.

In the third panel of the same table, employment income remains relatively stable over the sample period at about $92,000 for the top tax bracket group, and at about $38,000 for the middle tax bracket group. Comparing these values to the LAD sample, they are almost identical. This is encouraging for the validity of the results in this paper as the form of income that we are interested in studying, employment income, may be adequately sampled by the SLID. If this is true, the severe understatement of income in the upper tail is caused by other forms of income such as dividends, and capital gains.

The fourth panel in Table 2 shows mean annual hours paid over time for workers in all jobs. Over the six-year period show, mean annual hours decreased by 4 for the top group, increased by 24 for the middle group, and increased by 63 for the bottom group. For this last group, the increase represents about eight working days, which is substantial. We will address the possibility that this response is due to tax reform when we get to the results on hours elasticities in Section 4.3. The final panel of the table shows the mean values of the METR over the same period. As discussed in Chapter 1, the mean tax cuts were greatest for the top tax bracket group, and lowest for the bottom group. If we expect substitution effects to dominate in models of labour supply and taxes, it is interesting that the while the top group received the most substantial tax cuts, it had the smallest increase in hours. In the raw data, therefore, there is no evidence that the size of the tax cut varies positively with the change in hours worked. The empirical challenge, then, is to account for other possible factors (discussed below) that may have also affected hours over this period, and see if there is any evidence of a conditional response of hours to changes in tax rates.

2.4 Trends in data: other covariates

Apart from the METR, there are a number of other factors that likely affect tax-filer income in any given year. Examples of such factors include, but are not limited to: employment status, working in a full-time job, and the presence of children. Table 4 presents a number of these characteristics for the *adult* tax-filers in our sample. Just over a third of the respondents have children living with them. The presence of children has been shown to increase estimated wage elasticities, especially for women with children. For example, see Blundell et al. (1998). The next two rows of Table 4 provide age characteristics of our sample. On average, a quarter of adult tax-filers is over the age of 59, and about 5% are under the age of 25.[15] About 9% of the sample identifies as being a student (at least part-time) at some point in the year. Given that only 5% of our sample is under the age of 25, this implies that a substantial amount of individuals are still in school beyond this age.

[15] Note that the proportion of this latter group in the sample is so low because we already dropped adult children living at home in Section 2.4 above. If we were to add this group back into our sample, the proportion under the age of 25 in the overall sample would be about 13%.

Approximately four-fifths of the sample was employed at some point during the year over the six years covered by the sample. The next line of the table shows that of those who were employed, 80% were in their current job for at least 24 months at the beginning of the sample period, falling to 75% by the end of the sample period. Given that the employment rate of individuals in our sample remained stable over the same period, this could suggest that there was increased job turnover starting after the year 2000. Approximately 84% of the employed workers in our sample were paid employees, leaving 16% who identified as self-employed in their main job. A slightly higher percentage of workers, about 86%, of the employed workers self-reported as full-time in their main job over the same period, leaving 14% of the sample to be part-time workers.

3 Empirical Methodology

Recall that the empirical specification used in Chapter 1 for estimating an elasticity of income is as follows:

$$\begin{aligned} ln\,(I_{i(t)} / I_{i(t-2)}) = \beta_0 + \beta_1\, ln\,[(1-\tau_{i(t)})/(1-\tau_{i(t-2)})] + \beta_2\, S(ln\, I_{i(t-2)}) + \beta_3\, ln\, K_{i(t-2)} + \textstyle\sum_t \beta_{4t}\, Y_t + \\ \beta_5\, \text{age}_{(t-2)} + \beta_6\, \text{age}^2_{(t-2)} + \beta_7\, \text{numkids}_{(t-2)} + \textstyle\sum_k \beta_{8k(t)}\, IND_k + (\varepsilon_{i(t)} - \varepsilon_{i(t-2)}) \quad , \end{aligned} \qquad [1]$$

where ln $K_{i,t-2}$ is year *t-2* capital income, and $S(I_{i,t-2})$ is a spline function in year *t-2* total income.[16]

Note that the model above is a "quasi-first differences" model. While the dependent variable and some independent variables[17] are first-differenced (or equivalently, use log-ratios), age, industry of employment, and number of children enter the regression as a *levels* variable. This seemingly inconsistent specification from Chapter 1, however, was not entirely by choice. Unfortunately, the industry of employment is only available in the LAD starting in 2000, and therefore missing for the most critical base year of the study, 1999. Therefore, in that paper we used the industry in year *t* as a control variable. In this form, the variable captures average changes in incomes within industry groups between pairs of years.

We also included the number of children as a levels variable in Chapter 1 due to possible measurement error in this variable in the LAD. Specifically, the number of children is not reported on tax forms; it is imputed using other administrative data sources, such as applications for child benefits, linked to the Social Insurance Number (SIN) of the parent. When a new child is born, they are often not captured immediately in the LAD, meaning that a first-differences variable in the number of children will be inaccurate. Second, the age at which the first child in a family enters the LAD is often correlated with each family's propensity to apply for government-administered child benefits. For these reasons, I considered the *level* of the number of children to contain less measurement error than the *change* in the number of children. These issues with the industry and number of children variables in Chapter 1 implies that they serve as second-best proxies for ideal first-differenced forms of these variables.

[16] Note, we maintain the spline assumption for this paper to control for omitted variable bias. The source of the bias is likely due to strong mean reversion at the bottom of the distribution correlated with smaller tax cuts, biasing the elasticity downward.

[17] Although the variables ln $K_{ij(t-2)}$ and $S(\ln I_{ij(t-2)})$ are level variables, recall from the discussion in Chapter 1 that they are proxies for distribution-widening and mean reversion in the error term, $(\varepsilon_{ij(t)} - \varepsilon_{ij(t-2)})$, and in that sense they are capturing first-differenced variation.

The SLID, on the other hand, contains more complete and accurate information for many of the socioeconomic variables missing in the LAD. For this paper, we are able to include both industry of employment and number of children in a first-differences form, consistent with the dependent variable and primary independent variable of interest. Occupation of employment is also available in SLID, so we include first-differenced occupation terms. A potential drawback of including these variables as first-differences, however, is they could now be correlated with the error term ($\varepsilon_{ij(t)} - \varepsilon_{ij(t-2)}$). For the variables just mentioned, however, this seems implausible. The magnitude of the change in tax rates during the TONI reform is unlikely to cause the year *t* values of the demographic variables in the first-differenced terms to be endogenous to shocks in income. Specifically, if having children is endogenous to a cut in marginal tax rates of less than ten percentage points,[18] we are comfortable assuming that the magnitude of this endogeneity is negligible.

We assume industry of employment has a time-invariant fixed effect on the level of income. However, the average wage in an industry can change year-over-year due to market conditions, such as in oil and gas. Therefore, we also include first-differences of the interactions of industry and year dummy variables. For the sake of completeness, we construct similar variables for occupation groupings, although we expect short-term movements in average incomes within broad occupation groupings to be less volatile than within industries.

The new specification, with this new set of demographic variables represented as first-differences, and with the terms interacted with year dummies is:

$$\begin{aligned} \ln (I_{ij(t)} / I_{ij(t-2)}) &= \beta_0 + \beta_1 \ln [(1-\tau_{ij(t)})/(1-\tau_{ij(t-2)})]] + \beta_2 S(\ln I_{i(t-2)}) + \beta_3 \ln K_{i(t-2)} + \beta_{4t} \textstyle\sum_t Y_t \\ &+ \beta_5 \Delta\, \text{age}^2 + \beta_6 \Delta\, \text{numkids} + \textstyle\sum_k \beta_{7k} \Delta\, IND_k + \sum_l \beta_{8l} \Delta\, OCC_l + \sum_m \sum_t \beta_{9mt} \Delta\, (IND_m * Y_t) + \\ &\textstyle\sum_n \sum_t \beta_{10nt} \Delta\, (OCC_n * Y_t) + (\varepsilon_{ij(t)} - \varepsilon_{ij(t-2)}) \quad . \end{aligned} \qquad [2]$$

We conduct a few specification tests on this new model. In Table 6 we start with the case where $\beta_5=\beta_6=\beta_{7k}=\beta_{8l}=\beta_{9mt}=\beta_{10nt}=0$ for all *k, l, m, n, t*. Then we progressively relax these assumptions, culminating with the full estimation of [2] in the final column of that table. The elasticity estimate remains relatively stable across these multiple specifications, with the exception of the inclusion of occupation dummies, after which the estimate drops by almost half. I determined that this drop in the elasticity is due to the large loss of sample that results from adding the occupation dummies (due to missing occupation data), rather than the occupation dummies themselves.[19] Given that the inclusion of occupation result in so much lost sample, we elect to avoid the use of occupation dummies in our baseline regression.

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[18] The province with the greatest tax cut in a two-year period in the sample is BC between 2000 and 2002 at 9.1 points, which is less than 10 percentage points. See Table 5.

[19] Over 4,000 observations out of a starting sample of 21,883 are lost due to adding occupation. After consulting the questionnaire flow, I could not determine any procedural reason for this large number of observations for which industry data are available, but occupation data are not. The drop in elasticity is consistent with a sample selection bias of the responders who are missing occupation. Unfortunately, I could not identify any characteristics of the respondents that varied with the missing data.

3.1 Sample Restrictions

Converting our current sample of 109,500 observations into the two-year differenced structure shown in [2] above, we are left with 76,100 differenced observations. We make a few additional restrictions on this sample of differenced year-pairs so that we can estimate [2]. First, note that the $(1 - \tau_{ij(t)})$ term assumes that the METR will fall between 0 and 1. In practice, however, the structure of tax systems can lead to rare cases where the METR falls outside these bounds; we drop 200 such observations from our sample.

We drop several observations where there are significant changes in the respondent's situation between year *t-2* and year *t*. First, we drop 700 individuals who moved their province of residence between years. Our identification strategy relies on individuals residing in the same province before and after the tax change. With province of residence only reported on December 31st of each year, we have incomplete information on the timing of the tax "treatment" for individuals who move. Of course, these individuals could have moved *because* of the tax change, meaning our sample restriction is endogenous and would bias our estimate of the population elasticity downward. This consideration, however, is based on the theory of tax competition, which is outside the scope of the research question pursued in this paper. In order to model incentives due to *relative* changes between provinces, we would have to modify the estimation strategy entirely.[20] Given the magnitude of relative tax changes between provinces, however, endogeneity of province of residence is implausible. The relative difference in METR between the province with the greatest cut, B.C., and that with the smallest cut, Nova Scotia, was less than five percentage points between 1999 and 2001. It seems unlikely that individuals would move from one side of the country to the other, with associated moving costs, to arbitrage on a relative tax change of this magnitude. The greatest relative changes between *neighbouring* provinces, where moving is less costly, occurred along the border between Manitoba and Saskatchewan; the cuts in the latter province were 3.1 percentage points greater between 1999 and 2001. The number of individuals who moved from Manitoba to Saskatchewan in the raw data is almost zero, providing further evidence that endogeneity of our sample restriction is unlikely to be a concern. With this sample restriction our elasticity estimates represent elasticities among the Canadian population of "non-movers", or "stayers".

Next we drop those who are older than 59 years of age in year *t-2*. These individuals will be 61 in year *t*, and when we experiment with a three-year spacing between observations (as we do in one of our robustness checks in this paper), they will be 62 years of age in year *t*. Statistics Canada defines the working age population as individuals aged 15 to 64, so our threshold of 59 years of age in the base year ensures our sample remains strictly within this population.[21] On the other end of the age distribution, we drop those who are less than 25 years old. The labour supply decisions of people under the age of 25 are likely to be motivated by several factors more important than small tax changes, such as paying down student debt or making a down-payment on a first house. Additionally, this age restriction removes most full-time students from our estimation sample.

[20] We assume and model responses to *own-province* tax changes. We do not assume that the tax-changes of other provinces are in the objective function of the tax-filer. A recent U.S. study, Young et al. (2014) analyzing inter-state migration of high income earners due to increased relative marginal tax rates found very little evidence of migration for tax purposes.

[21] Dostie and Kromann (2013) use a cut-off of 55, a more restrictive upper bound on the retirement age.

As described in Chapter 1, we also drop tax-filers who changed marital status between the two observed periods. Although the unit of taxation in Canada is the individual, there are several calculations that are a function of the net income of the spouse. In 1999, examples of such items included GST/HST credits, social assistance income and repayments, and the spousal amount credit. This implies that the definition of taxable income is a function of marital status, *ceteris paribus*. As argued in Gruber and Saez (2002), ignoring known changes in the definition of taxable income amounts to including measurement error in the dependent variable. Most studies of taxable income elasticities, therefore, maintain a "constant-law" definition of taxable income across the event period, so that any changes in this variable are explained by the model. Rather than "assume" these individuals stay married or stay single (which they do not) to maintain the constant law definition, we choose to drop them from the sample.

We drop all respondents who paid less than $1000 tax in year *t-2* as well as those who earned less than $20,000 in income in either year *t-2* or year *t*. These restrictions remove individuals from our sample who pay no tax or very little tax. Given that we are concerned with estimating the responses to tax reform among those individuals *who pay tax*, this restriction should not significantly bias the population elasticity estimate generated from the remaining sample.[22] Low-income tax-filers are also likely to differ from medium and higher income tax-filers for a number of relevant unobservable characteristics such as accumulated savings. We have judged that the benefit of the additional sample size that comes with including low income individuals is outweighed by inappropriateness of assuming pooled regression parameters for high and low income individuals. Summary statistics for our sample after making the above sample restrictions are shown in Table 7.

3.2 Outliers

Our chosen empirical specification using logarithms, which follows closely that of previous researchers such as Gruber and Saez (2002), is very sensitive to outliers. In Chapter 1, I noted that re-including individuals with taxable income less than $100 in either year (who represented 0.2% of that sample) decreased the elasticity of taxable income for the top decile by over 2.0, an enormous change.[23] In our data, most individuals with taxable income of less than $100 in year *t-2* have taxable income several hundred percent higher in year *t*, and vice versa, representing an extreme form of mean reversion. As in Chapter 1, therefore, we drop all individuals with taxable income less than $100 in either year.[24] Dropping those with taxable incomes below $100 does not remove all extreme forms of mean reversion. As a second filter, we drop all observations where the ratio ($I_{ij(t)} / I_{ij(t-2)}$) is greater than 2 or less than 1/2.

We drop those with *predicted log-changes in METR* (our exclusion restriction) greater than 0.3 and less than -0.1 as no tax changes of this magnitude were legislated.[25] Values of this magnitude are rare and are

[22] Of course, on the extensive margin, a lower tax rate can induce some individuals to enter the workforce and begin to pay tax. In this paper, however, our research question is concerned with the population of individuals who are already employed and pay tax.
[23] This was pointed out in footnote 66 of Chapter 1.
[24] Note that an individual can have total income of $20,000 or more and still have a taxable income less than $100 due to the use of deductions.
[25] When we explored these outliers, they were generated by extreme nonlinearities in the relationship between income and tax payable. Fewer outliers are dropped when we modify the income increment used to calculate the METR in our robustness check in Table 12, i.e. when we use $1,000 instead of $100.

likely caused by extreme non-linearities in the relationship between income and tax payable at some kink points, such as those identified in Figure 3 in Chapter 1. After removing all outliers discussed so far we only lose 1,100 observations, or less than 4% of our sample.

Finally, we remove those with *actual log-changes* in METR greater than 0.3 and less than -0.3. When natural logarithm ratios exceed these values in either direction, they understate the actual percentage change in the METR and therefore our coefficient, β_1, is no longer interpretable as an elasticity. This restriction is costly in terms of sample; we lose 4,900 observations.

4 Results

4.1 Baseline Specification and Comparison to Chapter 1

We select the specification used in column 4 of Table 6 as our preferred baseline specification.[26] In Table 8 we test how the significance of the elasticity estimate responds to using weighted least squares and to clustering of the standard errors. For ease of comparison, the first column of Table 8 repeats the baseline result from Table 6 in which we found an elasticity of 0.066. We estimate the model using weighted least squares in column 2, using *log income* as the weight. Recall from Chapter 1 that the use of real income weights produced much higher elasticities in comparison to log-income weights, as the latter weight dampens somewhat the influence of the very high income earners. Including these log weights in this paper has almost no impact on the estimated elasticity.

In column 3 we cluster standard errors at the province level.[27] We choose the province level as the level of clustering as there may be province-specific movements in year-to-year income changes. The magnitude of the standard errors increases modestly when clustered, suggesting that the original standard errors may not have been biased downward by very much. The original work by Moulton (1990) suggests that downward bias can occur when one of the right-hand side variables is aggregated at some level above the microeconometric units, like province. Our METR variable, however, is only a quasi-aggregate variable; while the tax reforms do create province-specific variation in the METR, the majority of the variation in this variable is observed within provincial units rather than between provincial units.[28]

In the second half of Table 8 we run the same three regressions, except replacing total income with taxable income. Compared to total income, the point estimate is slightly lower in our baseline specification of column 4. Overall, there is very little difference in the pattern of results for taxable

[26] We choose not to use the model with occupation dummies as we would lose over 4,000 observations from missing occupation data. Specifically, in reference to the previous section, we maintain the restriction $\beta_{8lt}= \beta_{9mt} =\beta_{10nt}=0$ for all l,m, n, t.
[27] Ten clusters, one for each province, is considered to be a "small number" of clusters. Unfortunately we have very few alternatives. If we had a fully-balanced panel, it would make sense to cluster errors at the individual-level. For each individual, the term $(\varepsilon_{ij,2001} - \varepsilon_{ij,1999})$ will be correlated with $(\varepsilon_{ij,2002} - \varepsilon_{ij,2000})$ because they are both affected by the same income shocks in the years 2000 and 2001. However, we only have an average of 1.6 observations per individual in our restricted sample, making it unpractical to cluster at the individual level.
[28] I regressed the *predicted* METR (IV) variable on a full set of province dummy variables using the top percentile of the income distribution in the LAD. Only 11% of the variation was explained by province despite all filers being in the same federal tax bracket.

income, even after adding weights and clustered errors. With the elasticities of total and taxable income being almost identical, it suggests that deductions may not have been responsive to the tax changes over this period.[29]

In comparison to the analogous table from Chapter 1, the elasticity estimate for *total* income in this paper is greater by a value of 0.04. Given the range of elasticities in the literature, a difference of this magnitude should not be considered large. In addition, by comparing the estimate in both papers we are not comparing "like with like" for two reasons. First, our regression specification in this paper includes some richer controls, such as first-differenced industry dummies, that were not possible using the LAD data.[30] Second, from the discussion in Section 2.3 above, we know that the SLID sample is less representative of the tails of the income distribution.

Elasticity estimates for *taxable* income are about 0.025 greater than the corresponding estimate in Chapter 1, smaller than the 0.04 difference between the total income estimates. As discussed above, however, the taxable income variable is biased upward in this paper for tax-filers who make use of deductions not captured by the SLID.[31] For the remainder of this paper we focus on elasticities using dependent variables that are accurately captured by the SLID: total income, employment income, and hours of labour supplied.

4.2 Paid Employment Income Elasticity

Two-thirds of total income in Canada is made up of *paid* employment income (e.g. not self-employment income). Unless there are very large elasticities for some of the other types of income in Canada, it is likely that the majority of the total income elasticity is explained by changes in paid employment income. Formally, consider the following simple relationship. Suppose that for Canada, we represent aggregate total income for tax purposes as y, aggregate employment income for tax purposes as y_1 and the aggregate of all other forms of income as y_2. Empirically, if we look at the *T1 Income Statistics Report* published by CRA annually, it reveals that y_1 and y_2 were \$531 billion and \$273 respectively in 2004. We assume both of these income sources are sensitive to the METR, we can write them as $y_1(\tau)$ and $y_2(\tau)$. Writing down this simple relationship, we have:

$$y(\tau) = y_1(\tau) + y_2(\tau)\ . \qquad [3]$$

Taking the derivative with respect to the tax rate, and doing some algebraic manipulation (see the Appendix for all steps), we get:

[29] These results using taxable income should be interpreted cautiously. Recall from the discussion in Section 2.3 above that the definition of taxable income we use in this paper is likely to be biased upward for individuals who use deductions and credits not reported in the SLID.

[30] For example, if income in oil and gas decreased sharply between 2000 and 2002 when oil prices declined nearly 20%, and tax rates fell for earners in Alberta over this same period, this would bias the elasticities downward in the LAD specification because I did not have year-specific industry controls for such cyclical industries .

[31] Given that many of these deductions are primarily used by high income filers who are relatively less present in the SLID sample, bias due to measurement error of taxable income should not be severe.

$$\varepsilon_{y,\tau} = \varepsilon_{y_1,\tau}\frac{y_1}{y} + \varepsilon_{y_2,\tau}\frac{y_2}{y} \qquad [4]$$

From the second expression, the greater the share y_1 is of total income, the more the elasticity of y_1, $\varepsilon_{y_1,\tau}$, influences the overall elasticity of total income. Since y_1/y is less than one, if the elasticity of y_1 was to explain a disproportionate share of $\varepsilon_{y,\tau}$ then we would expect $\varepsilon_{y_1,\tau} > \varepsilon_{y,\tau}$. To see if there is any evidence of this in the data, we estimate the elasticity of paid employment income in Table 9.[32] The first column in this table adopts the same specification as column 3 of Table 8. The estimate of $\varepsilon_{y_1,\tau}$ is only 0.003 less than $\varepsilon_{y,\tau}$ from Table 8, not statistically different. From the discussion above, this suggests employment income is not playing a disproportionate role in the overall total income elasticity.

If we were now to think of [4] as a *microeconomic*, rather than a macroeconomic, relationship we can think of it as representing the income mix of the tax-filer's budget equation. Some filers will have multiple income types, while for others paid employment income will dominate, and represent well over 90% of their budget set. There are a few reasons why the income mix may affect the elasticity of paid employment income. First, it is possible that the elasticity of paid employment income varies positively with the share of paid employment income in a tax-filer's budget, or $\varepsilon_{y_1,\tau} = f\left(\frac{y_1}{y}\right) \, and \, f' > 0$. For example, for a tax-filer whose budget set is dominated by investment income, we may not expect the METR changes during TONI to induce a significant employment income response. Second, the amount of time available for paid employment work is likely a function of the amount of effort put into self-employment work. Elasticities of employment income, therefore, could be different for individuals who engage in *both* paid work and self-employment.

Given the expectation of heterogeneous responses in paid employment income depending on its relative importance in the budget set, in the next three columns of Table 9, we progressively restrict the sample to those tax-filers who rely most on paid employment income as their primary source of income. In column 2, we drop workers who have greater self-employment than paid employment incomes in year *t-2* (less than 1% of the sample). The elasticity increases by 0.04, a substantial jump, but the confidence interval still overlaps with the estimate in the previous column. While this increase is not significant, a 0.04 increase from losing a well-defined (and small) segment of the sample suggests that the original model may have been mis-specified with respect to this segment.[33] Specifically, we could have included a dummy variable for this segment in column 1. Regardless, the elasticity in column 2 can be interpreted as an elasticity of paid employment income for the population of workers who do not have self-employment income as their primary source of income.

In the third column we drop workers who have *any* self-employment income to completely remove workers who face some trade-off between positive amounts of paid work and self-employment work. In

[32] Note, tax-filers with less than \$1000 of employment income in either year *t* or year *t-2* are dropped from the sample. Movements across this boundary (i.e. on the *extensive margin* of labour supply) and are outside the scope of the research question of this paper.

[33] One explanation is those who have an already low income from paid employment were in transition from paid work to starting their own business. When observed in year t, their employment income should be expected to drop substantially and thus the change in the elasticity represents a *compositional* change in income.

the fourth column, we drop those who have investment income greater than employment income to remove any workers who face some trade-off between paid work and this type of income. In both cases, the changes in the elasticity are small and insignificant. Specifically, the changes in the point estimate are less than one-fifth of the magnitude of the standard error.[34]

The specifications in column 2 through 4 explored the impact of *heterogeneity in income sources* on the estimated elasticities of paid employment income. Now, we explore another dimension of heterogeneity within our sample of workers: *heterogeneity in the characteristics of their main job*.[35] To do this, we reset our sample restrictions on income source from above, and return to our starting sample of 20,760 from column 1. In column 5 we restrict the sample to tax-filers who self-identify as paid workers in their main job, where "job" can be a self-employed job. This restriction is very similar to the restriction above where we confined the sample to workers who had paid employment earnings greater than self-employment earnings, but the current restriction is based on a flag variable that identifies the job with the greatest number of hours worked as opposed to the greatest income.[36] Unsurprisingly, the point estimate is very similar in magnitude to that in column 2.

In column 6, we further restrict the sample to those workers who have been in the same job for at least 24 months as of year *t-2*. These workers are more likely to be in "stable" jobs with more certainty about future earnings. We may expect the responses on the margin to changes in METRs to be different between workers with certainty about future income flows compared to those with more uncertainty. We have no prior belief on the sign of this difference. Workers who change jobs often may be doing so because they have bargaining power and are seeking a higher wage. On the other hand, they may have changed employers unwillingly due to loss of their previous job. We would likely need to include data on spells of unemployment to distinguish these two worker types. When we drop the workers with job tenure less than 24 months, the elasticity falls by 0.03 to 0.06, suggesting that the remaining workers in longer-tenure jobs may have lower elasticities.

In the final column of Table 9, we restrict the sample to full-time workers. The theoretical underpinnings of classic labour supply models assume that workers have choice over how much labour to supply on the margin. This assumption is more likely to be true among hourly employees who work less than full-time hours. Full-time workers, many of whom are on salary, may have less opportunity to adjust paid hours of work upward. When we restrict the sample to these full-time workers, the elasticity of paid employment income falls by 0.02 to 0.04 as expected.

Note that our sample restriction strategy above is to progressively drop workers who are more likely to have elastic responses to changes in marginal after-tax income. We are left with a sample of full-time paid workers with relatively long job tenure, and we find the sample elasticity drops relative to the baseline

[34] The sample size in column 4 of Table 9 is only 1,283 observations less than in column 1. This implies that for 95.9% of the sample, paid employment income is the primary source of income.

[35] Summarized in Keane (2011), the extensive literature on the labour supply response to changes in income taxation tells us that there is substantial heterogeneity in the response across different subgroups of the population.

[36] Specifically, the flag variable is "class of worker". This restriction captures many of the same individuals as the income-based restriction. However, we use *class of worker* as our restriction as the subsequent sample restrictions we make are conditional on value of this flag variable in the flow of the survey questionnaire.

estimation. This suggests that the sample of workers who were dropped, just over 3000 observations, have higher elasticities on average.[37]

4.3 Hours of labour supply

In a simple model of labour supply, paid employment income can be thought of as the product of hours of work and an hourly wage. The paid employment income elasticity, therefore, can be written as the sum of the elasticity of hours paid and the elasticity of the hourly wage.[38] Which effect dominates is important when designing policy. For example, increased hours of work reduce the amount of time in the worker's budget set for other activities such as child care and leisure. On the other hand, if the wage effect dominates this could be suggestive evidence of increased worker productivity in response to a greater take-home pay.[39]

To investigate the relative importance of the elasticity of hours of work (versus wages) in the paid employment elasticity, we estimate an elasticity of annual hours of paid work. Given that the dependent variable is now hours of labour supplied, we make a few adjustments to the empirical specification in [2] to align it better with specifications typically used in the literature on the elasticity of hours of labour supply. First, we introduce a term for *after-tax income* to control for income effects. Similar to the discussion on the net-of-tax rate, $ln\ [(1-\tau_{ij(t)})/(1-\tau_{ij(t-2)})]$, this new variable will also be endogenous by design. That is, an increase in hours of work will generate a higher statutory tax rate and higher after-tax income. As with the net-of-tax rate, we instrument the after-tax income term by "counterfactual" after-tax income. Specifically, we take all nominal items reported in year *t-2* of each tax-filer's tax return and inflate them by the provincial CPI. We then run all of these tax return variables through the tax calculator. Essentially, this instrument amounts to assuming that the real value of all lines in a tax-filer's tax return did not change between year *t-2* and year *t*. Described in another way, this counterfactual will generate a change in the after-tax income that is *only a function of the exogenous changes in legislation*, the same as for our net-of-tax-rate (1-τ) instrument.[40]

Next, we drop the control for capital income from the regression. This control was in place in regressions where the dependent variable was a financial variable to control for the observed relative increases in top incomes, or distribution widening in the upper tail, that are unrelated to tax reform. For employment income, this could be due to general trends in executive pay pulling away from the pay of the median worker within firms. For total income, the widening of the distribution in the upper tail could be to

[37] Ideally, then, we would run a regression on these 3000 observations to test this. Unfortunately, when we tried this, we found there was insufficient variation across provinces and across time to be confident in our estimates. Because our identification strategy relies on adequate provincial variation, we require more sample than do estimations that rely on *federal* variation in tax rates.
[38] This is a simply identity in the calculus of elasticities. Namely, the elasticity of a product of functions is the sum of their individual elasticities.
[39] Previous studies have attempted to distinguish hours and wage elasticities. Analyzing the 1986 federal tax reform in the U.S., Moffitt and Willhelm (2000) conclude that for working age males the elasticity of hours paid is zero, and that the hourly wage response accounts entirely for estimated employment income elasticity. They do not suggest a theoretical mechanism behind this result.
[40] To the extent that inflation in an individual's income would not have grown at the rate of the provincial CPI (for example due to a nominal wage freeze) in the absence of tax reform, there will be some measurement error in the counterfactual instrument.

relative increases in capital income over labour income, which occurred in the U.S. in the 1980's and is described in Goolsbee (2000a). For a dependent variable defined as a first-difference in *hours paid*, where relatively few respondents in our sample are high income, there is no theoretical justification to maintain this distribution-widening control.

Finally, we do not use the natural log transformation on the dependent variable. The log-transformation is a reasonable approximation for percentage changes of plus or minus thirty percent. As hours can change by several hundred percent when the value in one of the two years is very small, we simply use the first difference of hours. The new specification is as follows:

$$(h_{ij(t)} - h_{ij(t-2)}) = \beta_0 + \beta_1 \ln [(1 - \tau_{ij(t)})/ (1 - \tau_{ij(t-2)})]] + \beta_2 \ln [(I_{ij(t)} - T(I_{ij(t)}))/ (I_{ij(t-2)} - T(I_{ij(t-2)}))] + \beta_3 S(\ln I_{ij(t-2)}) + \beta_{4t} \sum_t Y_t + \beta_5 \Delta\, \text{age}^2 + \beta_6 \Delta\, \text{numkids} + \sum_k \beta_{7k}\, \Delta\, IND_k + (\varepsilon_{ij(t)} - \varepsilon_{ij(t-2)}) . \quad [5]$$

Annual hours of paid labour for person *i* in year *t* are represented by $h_{ij(t)}$. Correspondingly, after-tax income is represented by ($I_{ij(t)}$ – T($I_{ij(t)}$)). The elasticity for this specification is now computed as $\frac{\beta_1}{(\bar{h}_t + \bar{h}_{t-2})/2}$, which is simply the point estimate divided by the average hours paid in both year *t-2* and year *t*.[41] The estimation results for this new specification are presented in Table 10. As the focus of this paper is on responses on the intensive margin, we drop any tax-filers who have less than 100 hours of paid work in the year or who have no paid employment income. The estimated elasticity of hours reported in column 1 is about 0.15. This implies that for a 10% increase in the net-of-tax rate, the number of hours paid, on average, increases by 1.5%.

As described in Keane (2011), researchers have historically found different labour supply responses for men and women. As women traditionally were second earners, the theory predicts they would have more flexibility to respond to changing tax incentives. To see if there were substantial differences in elasticities between men and women during the TONI reform period, we split the remaining sets of results in Table 10 by gender. Using the same specification as in column 1, we present the results for men in column 2 and for women in column 6. Comparing columns 2 and 6, the hours elasticity for women is higher, although not significantly so as the confidence intervals around the elasticities for men and women overlap. In the second pair of columns (3 and 7) we introduce the income effect control discussed above. In the presence of this new control, the estimate of β_1 represents now the *compensated* elasticity of hours worked. In each case, introducing this term has negligible impacts on the elasticity, suggesting that income effects are small.

In the final two pairs of columns comparing men and women, we repeat the exercise from the final two columns of the previous table, Table 9. Specifically, we restrict the sample to workers who have been in their job for at least 24 months, and then restrict to those who are full-time workers. In both cases, the point estimate for women exceeds that of men, but none of the estimates is significant.

The income effect coefficient, β_2, is positive in all cases for men, although insignificant. It is negative in all cases for women except for women who are full-time with some job tenure; for this case, it is not only

[41] With no log-transformation on the left-hand side and with a log transformation of the key independent variable, the interpretation is analogous to a semi-elasticity, and we have to divide by the mean hours of work to convert β_1 to an elasticity.

positive, but is positive and significant. A positive income effect suggests that for this group of women labour is a normal good, or leisure is an inferior good, which contradicts one of the most basic assumptions in the literature on labour supply (for example, see Ashenfelter and Heckman (1974). The estimate, however, is only significant at the 10% level. Given that our model is not a structural model of labour supply, we do not take this as strong evidence of counterintuitive income effects.

4.4 Robustness Check: Before-after window length

As discussed in Chapter 1, the choice of the appropriate number of years between the base year and the final year (year *t*) in the first-differences specification involves some trade-offs. A shorter time-span reduces the likelihood of there being major non-tax-related changes in a tax-filer's situation; whereas a longer tax span provides more time for a tax-filer to adjust to lower taxes if adjustment frictions are significant. To explore the sensitivity of the results to the year choice, Table 11 presents elasticities for window lengths between years of length one, two and three. The sample restrictions are the same as those in column 1 of Table 9. We make an additional restriction that the *log-ratio of incomes* should be greater than 1/2 and less than 2 to eliminate the role of severe outliers in comparing estimates across years.[42]

Looking at Table 11, we find that the two-year window used in all specifications so far produces the greatest elasticity.[43] If tax-filers take several years to adjust behaviour, we may expect the elasticity on the three-year window to be greatest, like I found in Chapter 1; however, we observe that the elasticity for a three-year spacing is lower than that using two years. It could be that the sample of tax-filers who meet the sample selection criteria in *both* year *t-3* and year *t* in the three-year case are more likely to be in stable employment situations. Thus, the lower elasticity in the three-year case may be driven by sample selection bias. As further evidence of this, moving from left to right in Table 11, the first-stage F statistic is increasing in the number of intervening years. Because our instrumental variables strategy relies on stable incomes for a good first-stage fit, this is consistent with a sample selection bias in which the proportion of workers in stable jobs varies positively with the choice of years between observations.

Given that the two-year gap produces the highest point estimate, there is some evidence that the elasticity estimates in all other regression tables presented so far can be thought of as an upper bound.

4.5 Robustness Check: vary the increment for calculating METR

The METR can be represented as a partial derivative of the change in tax payable for a small change in income. If y is income and $T(y)$ is tax payable as a function of income, the METR is: $\frac{\partial T(y)}{\partial \mathrm{y}}$. The derivative implies we should use the smallest discrete proxy for ∂y possible, namely $0.01. Practically, this would introduce measurement error as CTaCS includes some parameter values and cut-offs that are rounded. To avoid these issues, other authors, such as Milligan and Smart (2015), have used $100 as the increment value. We have also used $100 so far in this paper.

[42] Values outside these bounds imply that employment income has increased by over 100% or been cut in half between years. This restriction drops less than 5% of the original sample.

[43] This is not the same result as in Chapter 1, in which the elasticity was monotonically increasing in the year spacing for both total and taxable income.

Measurement errors aside, in practice the METR can vary substantially over short ranges of income. For example, Figure 3 of Chapter 1 shows that for a low income tax-filer, the METR can change from under 0.1 to 0.3 after adding only a marginal amount of income. Due to claw-backs in the Canadian income tax system, an METR can actually fall as income increases over some ranges of income. The non-monotonicity of the METR as a function of income within the Canadian tax system is in contrast to how the theoretical models of the economic problem facing a tax-filer are typically presented.[44]

Given that we are interested in modeling behaviour, and in particular labour supply behaviour, the relevant METR to model is the one considered by the tax-filer who is optimizing (among other things) over some labour-leisure choice. If an METR were to spike and then crash discontinuously over some small increment of income such as $375 (or a standard work week at a wage of $10/hour), an optimizing worker may tend to "smooth out" the observed METR and consider the take-home wage rate over a period longer than a week. That is, we may not observe the workers bunch at the kink point.[45] The relevant question then is: does it matter for the elasticity estimates if we use a "sharp" or "smooth" definition of METR? The first three columns of Table 12 use increment values of $10, $100, and $1000 to proxy the range from under-smoothing to over-smoothing. The difference between the estimates in the $10 and $100 cases is less than 0.01. The elasticity using the increment of $1000, however, is about 0.04 less than that using $100 and the standard error is smaller.[46] None of the elasticities is significant.

A fourth option to consider, presented in column 4, is taking the average of the METR created by the three possible increments in the first three columns. This generates an elasticity value that falls between that of the two extremes, $10 and $1000. Overall, then there is no significant difference in the elasticity depending on the choice of increment values.[47] Of the four cases considered, the $100 increment produces the greatest elasticity. Given this is the increment used in all previous tables in this paper, this is further suggestive evidence that elasticities estimated in this paper represent the upper bound.

Finally, we replace the METR with the ATR in [2] to consider the possibility that tax-filers in fact respond to their average tax rate rather than their marginal tax rate.[48] In a progressive tax system (i.e. not using a pure flat tax) a given change in the METR results in a smaller change in the ATR.[49] The

[44] In theory, a plot of after-tax income against gross income would simply be represented as a sequence of positive-sloped line segments, with the slopes decreasing as gross income increases.
[45] Saez (2010) finds no evidence of bunching at kink points other than at the extensive margin between zero tax payable and positive tax payable for low income filers.
[46] Low income filers face volatile METRs over short regions of income, which can be thought of as an optimization problem under uncertainty. Filers who are not perfectly informed about their instantaneous METR for each income level, therefore, can be considered to respond to their "expected" METR. The $1000 increment may be a better proxy for expected METR.
[47] For high income filers operating beyond the range of claw-backs and other discontinuities in the tax function, there is in general no difference between the four increment cases presented.
[48] The empirical form of [2] may not be an appropriate representation of an underlying theoretical model of a tax-filer optimizing with respect to changes in ATR. As doing so would require a completely separate analysis, the crude substitution of METR for ATR here should be considered a second-best estimation.
[49] Formally, if income is y and tax is $T(y)$ and the *change in* METR is $\partial T'(y)/\partial y$ and then the change in ATR is $\partial (T(y)/y)/\partial y$, the change in the METR across a kink point (where $T'(y)$ increases) will be greater than the change in ATR. We can also ask, for a given percent change in $(1-\tau)$ (normalized to one), what would be the equivalent change in ATR. If we use the results of the model in Table 12 and use column 4 as our definition of METR, the empirical answer would be the value of (1–ATR) that solves: $\varepsilon_{METR} * 1 = \varepsilon_{ATR} * (\%\Delta(1-ATR))$; $0.0561 =$

expression for the elasticity as a function of a given marginal change in the ATR, therefore, will generate greater elasticity estimates. In column 5, the elasticity is 0.34, implying that a 1% increase in (1–ATR) would result in a 0.34% increase in employment income.

4.6 Other Canadian estimates of the elasticity of labour supply

There have been a number of Canadian studies which have estimated the elasticity of hours of work using SLID. Recently, using the SLID over 1996 to 2005, Dostie and Kromann (2013) find elasticities of labour supply in the range of 0.03 to 0.13 for *married women*. While their estimation strategy is somewhat different, they use the same survey and a similar time period to our paper.[50] We do not have separate estimates for married women in our paper, but our estimates for women in Table 10 range from 0.10 to 0.16.[51] The key difference between the Dostie and Kromann (2013) paper and our paper is they consider variation in the after-tax earnings due to all possible sources, whereas we only consider variation in this variable due to exogenous tax rate changes. Comparability of elasticities from our study with theirs depends on if workers are indifferent between the sources of variation in their after-tax wage. That is, they do not care if it comes from a change in pre-tax wages or from a legislated tax reform.[52]

Another Canadian paper estimating labour supply elasticities using SLID over the period of the TONI reform is by Sand (2005). Using a grouping estimator and repeated cross-section data from the SLID public-use file, he finds elasticities of labour supply not significantly different from zero for both men and women over this period. Although approaching the question using a different identification strategy, the results in that paper are not very different from the results in this paper. Our pooled specifications in Table 10 do include some estimates which are significantly different from zero, but these estimates never exceed 0.16. An advantage of our paper over these other two is we use panel data on individuals rather than repeated cross-section data. Rather than comparing groups of *similar* individuals before and after tax changes, we observe the *same individual* before and after the changes.

5 Conclusion

Estimates of the elasticity of employment income found in this paper are modest in magnitude, ranging from 0.04 to 0.14. With employment income elasticities so low it is not surprising that the estimated hours elasticity, the key determinant of the employment income elasticity, is also low. As has been demonstrated throughout the literature on labour supply, however, while the overall elasticities of labour supply may be low, they may be relatively higher for certain well-defined segments of the labour force. For this reason, many research papers focus entirely on one of these groups where the elasticities are expected to be relatively high, such as unmarried mothers with children (see Blundell et al. (1998).

0.3431*(%Δ(1–ATR)); then %Δ(1–ATR) = 0.164, which implies the average change in (1–ATR) is less than one-sixth the change of a given change in (1– τ).

[50] They use a Heckman two-step procedure to estimate their elasticities and also use a Probit specification to estimate *participation* elasticities (elasticities on the *extensive* margin).

[51] To explore this unexpected result further, we ran a separate regression in which we split the sample from column 9 of Table 10 into married and single women. The income effect for married women is positive and significant, while the income effect for single women is negative and insignificant. Perhaps time-use data could be used to explore the underlying mechanics driving the non-normality of leisure among married women. This is a topic for future research.

[52] Chetty et al. (2009) calls into question this common assumption in microeconomic theory, providing evidence that consumers may respond differently to a given price change if they know it is tax-sourced.

Appreciating the heterogeneity in elasticities, we take advantage of some key labour market variables in the SLID to estimate elasticities for a few identifiable subgroups of the Canadian labour force. We find that dropping the self-employed and those with low job tenure tends to reduce the elasticity of the remaining sample, implying that these dropped workers may in fact have higher elasticities.

The structural literature on tax and labour supply has proceeded largely in isolation of the reduced form, or so-called "new tax responsiveness", literature on total income elasticities.[53] The fact that these literatures have diverged may have more to do with data sources than anything else. Structural labour supply models are often estimated using survey data that is rich in information on hours worked, education, and job characteristics. Papers in the new tax responsiveness literature have tended to use administrative tax data that contains all of the necessary line items necessary to compute an accurate tax liability and METR. The SLID is a unique dataset that contains both of these sets of variables, and in this paper we have attempted to bridge the gap somewhat between these two literatures, by estimating elasticities of *both* hours of work and employment income *for the same set of individuals*. Although the elasticity estimates we found are small for both employment income and hours worked, we found the magnitudes to be internally consistent. For example, when we restricted the sample to full-time workers with long job tenure, the elasticity estimates fell for both employment income and paid hours of work.

Apart from insights into heterogeneity in elasticities among workers, a second-order benefit of using the SLID in this paper is it provides a robustness check on the results from the LAD from Chapter 1. Notwithstanding the fact that the SLID is a survey and therefore subject to issues like attrition bias, the tax-filer records in SLID should in general be representative of the LAD sample because for 80% of the respondents these data are derived from the same database as the LAD.[54] In Chapter 1, I found elasticities of employment income in each decile were either negative or zero. Although not shown, I had estimated a full-sample regression for employment income using LAD (ie. pooling individuals of all income levels) and found the overall elasticity to be near zero and insignificant. Given that we found an insignificant elasticity of 0.067 in this paper, using a different sample of tax-filers but a very similar methodology, this suggests that employment income elasticities were likely small in response to the TONI reform.

In addition to employment income elasticities, we can also compare *total income* elasticities between the two chapters. In Chapter 1, I find an insignificant elasticity of 0.026 for total income in the full-sample regression. In this paper we find an insignificant elasticity of 0.065 using a very similar specification. Although the point estimate in the former paper is about 0.04 lower than in this one, this provides evidence that the response in total income was likewise small in response to the TONI reform.

In the conclusion of Chapter 1, I argued that small observed elasticities estimates do not imply that individuals do not respond to tax changes. There are several reasons for this. First, the estimation strategy in both papers excludes some margins of response. For example, we do not cover individuals who are not participating in the labour force. We do not consider workers who move provinces, or tax-filers who engage in tax evasion. Second, the magnitude of the tax reforms that took place during the TONI reform may have simply been too small to induce an observable response. Third, we selected to observe

[53] Formally, inspection of the bibliography for the most recent survey papers in each literature, Keane (2011) and Meghir and Phillips (2010), reveal almost no common citations.

[54] This database is the T1 Family File (T1FF), provided to Statistics Canada every year by the Canada Revenue Agency. For more on the comparability of SLID with other tax data, see Frenette et al. (2007).

individuals only up to a maximum of three years apart in our estimation strategy. If individuals respond slowly to tax reform, taking longer than three years to fully adjust their behaviour, our elasticity estimates will be understated.

What can we say about the results in this paper? From a policy perspective, low elasticities imply that when the government cuts statutory tax rates, very little of the lost revenue is recaptured. Governments also care about welfare and efficiency. Low labour supply elasticities that reflect *real responses*, however, imply that deadweight loss may not be that large to begin with and that Okun's leaky bucket may not be a major concern. We have provided evidence in this paper that for some well-defined groups in the population, elasticities are likely to be higher. Future research should focus on estimating the responsiveness of these well-defined groups. If elasticities are found to be very significant, this will be useful for the design of targeted policies.

6 Appendix

6.1 Decomposition of total income elasticity

What follows is the full derivation of expression [4] in the main body of the paper. The derivation below is simply an application of a general result in the calculus of elasticities. Namely, that the elasticity of a sum of two functions is the share-weighted average of their individual elasticities.

$$y(\tau) = y_1(\tau) + y_2(\tau)$$

$$\frac{dy}{d\tau} = \frac{dy_1}{d\tau} + \frac{dy_2}{d\tau}$$

$$\frac{dy}{d\tau}\frac{\tau}{y} = \frac{dy_1}{d\tau}\frac{\tau}{y} + \frac{dy_2}{d\tau}\frac{\tau}{y}$$

$$\frac{dy}{d\tau}\frac{\tau}{y} = \frac{dy_1}{d\tau}\frac{\tau}{y}\frac{y_1}{y_1} + \frac{dy_2}{d\tau}\frac{\tau}{y}\frac{y_2}{y_2}$$

$$\frac{dy}{d\tau}\frac{\tau}{y} = \frac{dy_1}{d\tau}\frac{\tau}{y_1}\frac{y_1}{y} + \frac{dy_2}{d\tau}\frac{\tau}{y_2}\frac{y_2}{y} \qquad [6]$$

$$\varepsilon_{y,\tau} = \varepsilon_{y_1,\tau}\frac{y_1}{y} + \varepsilon_{y_2,\tau}\frac{y_2}{y}$$

$$\varepsilon_{y,\tau} = \varepsilon_{y_1,\tau}\frac{y_1}{y} + \varepsilon_{y_2,\tau}\frac{y - y_1}{y}$$

7 Tables and Figures

Table 1. Sample Selection and Record Inclusion

Sample Description	Observations	Row ID
Starting Sample	262,100	1
Less: out of scope (mostly deceased or hard refusals)	226,400	2
Less: missing income information	177,000	3
Less: minors (age less than 18)	134,500	4
Less: adult children living at home*	124,700	5
Less: missing full labour and income variables	115,400	6
Less: did not permit access to tax records	109,500	7
Change Unit of Analysis to First Differences	76,100	8
Less: METR not in [0,1]	75,900	9
Less: Moved provinces between years	75,200	10
Less: age in base year less than 25	72,200	11
Less: age in base year greater than 59	48,400	12
Less: change in marital status between year *t-2* and *t*	46,000	13
Less: paid less than $1000 in tax in year *t-2*	34,600	14
Less: total income less than $20,000 in year *t-2*	30,800	15
Less: total income less than $20,000 in year *t*	29,200	16
Additional Regression Restrictions	-	17
Less: total income greater than $250,000 in year *t-2*	29,100	18
Less: $ln\ [(1-\tau_{ij(predicted)})/(1-\tau_{ij(t-2)})]$ not in [-0.1,0.3]	28,700	19
Less: $ln\ [(1-\tau_{ij(t)})/(1-\tau_{ij(t-2)})]$ not in [-0.3,0.3]	23,800	20
Less: taxable income less than $100 in year 1 or year 2	23,800	21
Less: $\ln(taxinc_t/taxinc_{t-2})$ not in [0.5,2.0]	23,200	22

Notes: The starting sample is from Panel 3 of the SLID. All values have been rounded to nearest 100. There are exactly 43,683 observations per year over six years from 1999 to 2004, representing about 17,000 households (see 2007 SLID Overview.pdf in SLID Documentation files). The above sample restrictions are for our baseline regression in Table 8 only – see notes in other tables for any additional restrictions. Where the unit of analysis above is in first-differences, we use a year gap of two years between observations for the purposes of generating the lost sample counts; ie. the base year is *t-2*. *This group includes 100 observations for which we are missing marital status.

Table 2. Time series of key variables by federal statutory tax rate on the last dollar of income

		Federal Tax Bracket					
		MTR 29 and 26		**MTR 22**		**MTR 15**	
Variable	**year**						
total income	1999	$	107,100	$	47,900	$	16,700
	2000	$	110,400	$	47,500	$	16,300
	2001	$	110,400	$	47,500	$	16,700
	2002	$	107,600	$	48,000	$	16,800
	2003	$	107,500	$	47,700	$	16,700
	2004	$	117,100	$	50,500	$	17,600
taxable income	1999	$	105,200	$	46,500	$	15,100
	2000	$	108,700	$	46,100	$	14,800
	2001	$	108,700	$	46,100	$	15,200
	2002	$	105,700	$	46,600	$	15,300
	2003	$	105,500	$	46,300	$	15,200
	2004	$	114,900	$	48,900	$	16,100
employment income	1999	$	92,700	$	38,600	$	9,300
	2000	$	94,100	$	38,100	$	9,100
	2001	$	94,200	$	37,900	$	9,400
	2002	$	91,400	$	38,500	$	9,400
	2003	$	92,200	$	38,200	$	9,300
	2004	$	100,300	$	41,000	$	10,000
annual hours paid	1999		2,082		1,845		1,070
	2000		2,038		1,835		1,079
	2001		2,083		1,841		1,092
	2002		2,079		1,848		1,074
	2003		2,099		1,846		1,086
	2004		2,078		1,869		1,133
METR	1999		48.9%		42.5%		23.4%
	2000		47.6%		40.5%		23.3%
	2001		43.3%		36.8%		22.0%
	2002		42.9%		36.2%		21.5%
	2003		42.9%		36.2%		21.4%
	2004		43.3%		36.0%		22.0%

Notes: The mean values in the table are drawn from the full sample of about 109,500 shown in row 7 of Table 1. Thus, the category MTR15 includes individuals who paid no tax. The 29% MTR did not exist in 1999 and 2000; it is imputed by back-casting and deflating the 2001 cut-off. All income values have been converted into 2004 dollars using a CPI deflator. Tax brackets used are the federal statutory brackets, and are used as an indicator of place within the taxable income distribution. Both total and taxable income values shown are those that are produced by the tax calculator, minus taxable capital gains. The METR shown is the actual METR in each cell, not the predicted value using the instrument. All means calculated using panel weights (ilgwt).

Table 3. Threshold values for total income deciles used in regression results, overall and by gender

Decile	All	Male	Female
1	$ 20,000	$ 20,000	$ 20,000
2	$ 25,700	$ 27,700	$ 24,100
3	$ 30,100	$ 33,200	$ 27,400
4	$ 34,400	$ 38,500	$ 30,600
5	$ 38,900	$ 43,800	$ 34,000
6	$ 43,900	$ 49,500	$ 37,500
7	$ 49,900	$ 55,400	$ 41,900
8	$ 56,700	$ 63,100	$ 47,300
9	$ 66,000	$ 72,600	$ 55,200
10	$ 80,100	$ 88,200	$ 66,800

Notes: Cut-off values are generated from the baseline sample in the final row of Table 1; the lower bound of the first decile is $20,000. For regression results in this paper, I use the "All" values as the threshold values, even in tables where regressions are estimated separated by gender. Gender values are shown for comparison. The deciles in this table are different from familiar national definitions to divide the population such as those found in CANSIM Table 204-0001 which include low-income observations. All values have been rounded to the nearest $100 in accordance with the confidentiality rules of the RDC. All dollars values are in 2004 Canadian dollars. The sample is based on year *t-2* values over our entire sample period.

Table 4. Mean time-series values of binary variables in sample

	Values						Frequencies						
Variable	**1999**	**2000**	**2001**	**2002**	**2003**	**2004**	**1999**	**2000**	**2001**	**2002**	**2003**	**2004**	**Total**
Any children	0.36	0.36	0.35	0.34	0.33	0.33	16,500	17,000	19,000	18,500	19,000	19,000	109,000
Age > 59	0.24	0.24	0.25	0.25	0.26	0.25	16,500	17,000	19,000	18,500	19,000	19,000	109,000
Age < 25	0.05	0.04	0.04	0.04	0.04	0.04	16,500	17,000	19,000	18,500	19,000	19,000	109,000
Student	0.09	0.09	0.09	0.08	0.09	0.08	14,000	14,500	16,000	16,000	16,000	16,000	92,500
Employed in year*	0.79	0.79	0.80	0.79	0.80	0.80	14,000	14,500	16,000	16,000	16,000	16,000	92,500
Same job for 24 months	0.80	0.80	0.78	0.76	0.75	0.74	11,500	12,500	14,000	14,000	14,000	14,000	80,000
Employee (paid worker)**	0.84	0.83	0.84	0.85	0.84	0.85	11,000	11,500	13,000	12,500	12,500	12,500	73,000
Full time worker***	0.85	0.86	0.85	0.85	0.86	0.86	11,000	11,000	12,500	12,000	12,000	12,000	70,500

Notes: Mean values are based on row 7 of Table 1, starting with a total sample size in all years of 109,000. All frequencies are rounded to the nearest 500 and indicate the number of valid (non-missing) values for each cell. Student refers to student of any kind. Full and part time workers are conditional on employment. * Individuals who are not employed were unemployed all year or not in the labour force all year. ** Those who are not paid workers were self-employed in their main job. *** Those who are not full-time were part-time workers in their main job. All means calculated using panel weights (ilgwt).

Table 5. Mean values of percentage point changes in *predicted* METR by tax bracket and province for multiple sets of two-year pairs

Federal Statutory Rate	Year Pair	NL	PE	NS	NB	QC	ON	MB	SK	AB	BC
MTR 29 and 26	1999-2001	-6.1%	-3.9%	-3.5%	-5.2%	-4.7%	-4.2%	-4.8%	-7.9%	-8.1%	-8.2%
	2000-2002	-5.0%	-3.0%	-2.9%	-3.6%	-3.5%	-3.4%	-3.6%	-6.9%	-6.1%	-9.1%
	2001-2003	0.1%	0.0%	0.0%	0.1%	-0.5%	-0.1%	-0.1%	-2.6%	0.1%	-2.0%
	2002-2004	-1.0%	-1.0%	-0.4%	-0.8%	-0.5%	-0.4%	-0.4%	-3.1%	-0.5%	-0.8%
MTR 22	1999-2001	-6.2%	-5.6%	-4.1%	-5.1%	-5.3%	-5.5%	-4.7%	-7.4%	-6.7%	-6.7%
	2000-2002	-2.9%	-3.2%	-3.0%	-2.9%	-4.5%	-3.6%	-3.8%	-4.8%	-4.5%	-6.3%
	2001-2003	0.2%	0.2%	-0.1%	0.3%	-0.3%	-0.2%	-1.4%	-0.7%	-0.1%	-1.3%
	2002-2004	0.1%	-0.3%	-0.3%	-0.6%	-0.8%	-0.2%	-1.9%	-1.4%	-0.7%	-0.5%
MTR 15	1999-2001	-1.3%	-0.2%	0.6%	-1.0%	-2.0%	-0.6%	-0.2%	0.4%	0.3%	-1.8%
	2000-2002	-0.4%	-0.5%	0.3%	-1.0%	-2.1%	-0.8%	0.4%	0.9%	1.2%	-2.6%
	2001-2003	1.0%	1.1%	1.0%	1.1%	-0.8%	0.3%	0.5%	-0.4%	2.0%	-0.7%
	2002-2004	0.3%	0.7%	0.2%	0.4%	-0.3%	1.0%	0.0%	-0.6%	-0.2%	-0.1%

Notes: Values represent the *mean* percentage point change in the *predicted* METRs between various pairs of years for each province. 'Predicted' refers to the variation in METRs generated by the instrument described in Chapter 1. The predicted METR is the METR that would result if the tax-filer had no change in real income. The statistics are based on the same set of sample restrictions as row 16 in Table 1 (N=29,200). Federal statutory MTR is determined by taxable income calculated by CTaCS in year *t-2*. The 29% MTR did not exist in 1999 and 2000; it is imputed by back-casting and deflating the 2001 cut-off. All means calculated using panel weights (ilgwt).

Table 6. Testing covariates: elasticity of total income with various covariates

	(1)	(2)	(3)	(4)	(5)	(6)
change in log (1-τ)	0.0717	0.0718	0.0700	0.0656	0.0369	0.0449
	(0.0514)	(0.0510)	(0.0510)	(0.0513)	(0.0524)	(0.0527)
Spline Variables						
decile 1	-0.6094***	-0.5983***	-0.5970***	-0.5896***	-0.6022***	-0.6016***
	(0.0471)	(0.0468)	(0.0468)	(0.0479)	(0.0540)	(0.0541)
decile 2	-0.0737	-0.0826	-0.0802	-0.0852	-0.0696	-0.0715
	(0.0557)	(0.0553)	(0.0553)	(0.0563)	(0.0611)	(0.0612)
decile 3	-0.3436***	-0.3485***	-0.3485***	-0.3437***	-0.3344***	-0.3366***
	(0.0751)	(0.0746)	(0.0746)	(0.0756)	(0.0799)	(0.0800)
decile 4	0.0622	0.0643	0.0655	0.0819	0.1097	0.1043
	(0.0752)	(0.0746)	(0.0746)	(0.0755)	(0.0799)	(0.0801)
decile 5	-0.0987	-0.0865	-0.0875	-0.0825	-0.0435	-0.0403
	(0.0775)	(0.0770)	(0.0770)	(0.0779)	(0.0821)	(0.0823)
decile 6	-0.0285	-0.0446	-0.0439	-0.0613	-0.0684	-0.0639
	(0.0702)	(0.0698)	(0.0697)	(0.0700)	(0.0736)	(0.0737)
decile 7	-0.0671	-0.0269	-0.0259	0.0001	-0.0437	-0.0541
	(0.0670)	(0.0666)	(0.0665)	(0.0665)	(0.0690)	(0.0691)
decile 8	-0.0149	-0.0295	-0.0327	-0.0288	0.0335	0.0395
	(0.0571)	(0.0567)	(0.0567)	(0.0565)	(0.0580)	(0.0581)
decile 9	-0.0922**	-0.0919**	-0.0893**	-0.0778*	-0.0853*	-0.0885**
	(0.0443)	(0.0440)	(0.0440)	(0.0436)	(0.0449)	(0.0450)
decile 10	-0.0013	0.0057	0.0051	-0.0031	0.0029	0.0038
	(0.0140)	(0.0139)	(0.0139)	(0.0137)	(0.0139)	(0.0140)
year 1 capital income	-0.0014***	-0.0004	-0.0004	-0.0004	-0.0006**	-0.0006**
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
base year 1999	0.0012	-0.0006	-0.0006	-0.0011	0.0013	-0.0265
	(0.0051)	(0.0050)	(0.0050)	(0.0051)	(0.0053)	(0.0215)
base year 2000	-0.0056	-0.0073	-0.0073	-0.0066	-0.0059	-0.0182
	(0.0045)	(0.0045)	(0.0045)	(0.0046)	(0.0048)	(0.0204)
base year 2001	-0.0035	-0.0044	-0.0044	-0.0036	-0.0051	-0.0067
	(0.0035)	(0.0035)	(0.0035)	(0.0035)	(0.0037)	(0.0195)
change in age squared		-0.0007***	-0.0007***	-0.0006***	-0.0005***	-0.0005***
		(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
change in num. kids			-0.0097***	-0.0086***	-0.0108***	-0.0105***
			(0.0025)	(0.0025)	(0.0026)	(0.0026)
Industry						
primary				0.0434***	0.0312*	0.0385
				(0.0138)	(0.0181)	(0.0372)
private goods				0.0365***	0.0677***	0.0776***
				(0.0071)	(0.0099)	(0.0191)
public				0.0140	0.0261*	0.0065
				(0.0111)	(0.0134)	(0.0309)

	(1)	(2)	(3)	(4)	(5)	(6)
Occupation						
mgmt and fin					-0.0082	-0.0082
					(0.0097)	(0.0098)
health and science					-0.0105	-0.0100
					(0.0116)	(0.0117)
govt					-0.0254*	-0.0253*
					(0.0147)	(0.0147)
Culture					-0.0329*	-0.0318*
					(0.0174)	(0.0175)
sales and service					-0.0423***	-0.0423***
					(0.0110)	(0.0111)
Restrictions						
β_5=0	Yes					
β_6=0	Yes	Yes				
β_{7k}=0 for all k	Yes	Yes	Yes			
β_{8l}=0 for all l	Yes	Yes	Yes	Yes		
B_{9m}=0 for all m	Yes	Yes	Yes	Yes	Yes	
B_{10n}=0 for all n	Yes	Yes	Yes	Yes	Yes	
Observations	23,183	23,183	23,183	21,883	17,765	17,765

Notes: The exclusion restriction is the predicted change in log (1-τ) as described in Chapter 1. The definition of *year t-2* income, represented as a spline, is the same as the dependent variable. In this table the dependent variable is defined in terms of total income. Deciles used to form the spline function are calculated by dividing the sample into ten equal groups according to the year *t-2* value of total income. All estimates are based on the sample in row 22 (last row) of Table 1. All year *t-2* values of taxable income less than \$100 have been dropped. Such small values are not appropriate to use in a log-ratio operator to represent approximations in percent change. All regressions have been weighted using the panel weight (ilwgt). Weights are not multiplied by income and standard errors are not clustered in this table. Standard errors in parentheses: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 7. Means and standard deviations for key variables

Variable	N	Mean	Std. Deviation
income and METR			
year 1 taxable income	29,000	$ 53,700	$ 56,600
year 1 total income	29,000	$ 55,200	$ 56,800
year 1 wage & salary income	29,000	$ 46,500	$ 50,900
percentage point change in METR	25,000	-1.8%	0.064
percentage point change in METR (IV)	29,000	-1.9%	0.034
Personal	-		
married dummy	29,000	0.78	0.415
number of kids	29,000	0.96	1.164
Age	29,000	42	9
labour force	-		
annual hours paid in year *t-2*	29,000	1,949	690
self-employment dummy	29,000	0.06	0.234
in job for at least 24 months in year *t-2*	29,000	0.89	0.318
in full-time job in year *t-2*	29,000	0.88	0.326
Occupation	-		
mgmt and fin	24,000	0.31	0.464
health and science	24,000	0.16	0.368
Govt	24,000	0.09	0.288
Culture	24,000	0.02	0.145
sales and service	24,000	0.15	0.352
blue collar	24,000	0.27	0.442
Industry	-		
Primary	28,000	0.04	0.195
private goods	28,000	0.25	0.434
private services	28,000	0.63	0.483
Public	28,000	0.08	0.272

Notes: Statistics are based on the sample restrictions applied up to row 16 of Table 1. Sample sizes rounded to nearest 1,000. Dollar values greater than $1000 rounded to nearest $100. All means and standard deviations calculated using panel weights (ilgwt). The mean tax cut is around 2% because the sample includes pairs of years in which there were few significant tax cuts such as the period between 2002 and 2004. Frequency values reflect first difference-year units of analysis, not individual-year units of analysis. All dollar values are in 2004 Canadian dollars.

Table 8. Baseline Regression: Elasticity of income (taxable and total) by choice of base year income control, and by weighting and clustering assumptions

	Total Income			Taxable Income		
	(1)	(2)	(3)	(4)	(5)	(6)
change in log (1-τ)	0.0656	0.0652	0.0652	0.0616	0.0597	0.0597
	(0.0513)	(0.0516)	(0.0698)	(0.0539)	(0.0542)	(0.0512)
Spline Variables						
decile 1	-0.5896***	-0.5898***	-0.5898***	-0.6136***	-0.6135***	-0.6135***
	(0.0479)	(0.0496)	(0.0480)	(0.0456)	(0.0472)	(0.0429)
decile 2	-0.0852	-0.0853	-0.0853***	-0.1477***	-0.1482**	-0.1482***
	(0.0563)	(0.0578)	(0.0331)	(0.0571)	(0.0585)	(0.0400)
decile 3	-0.3437***	-0.3430***	-0.3430***	-0.2459***	-0.2440***	-0.2440***
	(0.0756)	(0.0768)	(0.0664)	(0.0791)	(0.0804)	(0.0514)
decile 4	0.0819	0.0813	0.0813	-0.0413	-0.0420	-0.0420
	(0.0755)	(0.0764)	(0.1469)	(0.0773)	(0.0782)	(0.1158)
decile 5	-0.0825	-0.0824	-0.0824	0.0059	0.0058	0.0058
	(0.0779)	(0.0784)	(0.1094)	(0.0797)	(0.0803)	(0.0621)
decile 6	-0.0613	-0.0612	-0.0612	-0.1833**	-0.1837**	-0.1837**
	(0.0700)	(0.0701)	(0.1431)	(0.0731)	(0.0732)	(0.0784)
decile 7	0.0001	-0.0004	-0.0004	0.1382**	0.1377**	0.1377***
	(0.0665)	(0.0662)	(0.0755)	(0.0664)	(0.0661)	(0.0469)
decile 8	-0.0288	-0.0281	-0.0281	-0.1119*	-0.1115*	-0.1115
	(0.0565)	(0.0559)	(0.0799)	(0.0591)	(0.0585)	(0.0929)
decile 9	-0.0778*	-0.0784*	-0.0784	-0.0633	-0.0634	-0.0634
	(0.0436)	(0.0428)	(0.0517)	(0.0435)	(0.0428)	(0.0419)
decile 10	-0.0031	-0.0029	-0.0029	-0.0001	0.0001	0.0001
	(0.0137)	(0.0131)	(0.0273)	(0.0136)	(0.0130)	(0.0269)
year 1 capital income	-0.0004	-0.0004	-0.0004	-0.0003	-0.0003	-0.0003
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
base year 1999	-0.0011	-0.0007	-0.0007	0.0040	0.0045	0.0045
	(0.0051)	(0.0051)	(0.0057)	(0.0052)	(0.0053)	(0.0058)
base year 2000	-0.0066	-0.0066	-0.0066	-0.0042	-0.0041	-0.0041
	(0.0046)	(0.0046)	(0.0045)	(0.0047)	(0.0047)	(0.0042)
base year 2001	-0.0036	-0.0035	-0.0035	-0.0037	-0.0035	-0.0035
	(0.0035)	(0.0035)	(0.0045)	(0.0036)	(0.0036)	(0.0042)
change in age squared	-0.0006***	-0.0006***	-0.0006***	-0.0005***	-0.0005***	-0.0005***
	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0000)	(0.0001)
change in num. kids	-0.0086***	-0.0086***	-0.0086**	-0.0096***	-0.0096***	-0.0096**
	(0.0025)	(0.0025)	(0.0040)	(0.0025)	(0.0025)	(0.0045)
primary	0.0434***	0.0443***	0.0443**	0.0482***	0.0493***	0.0493***
	(0.0138)	(0.0139)	(0.0192)	(0.0141)	(0.0142)	(0.0186)
private goods	0.0365***	0.0363***	0.0363***	0.0331***	0.0328***	0.0328***
	(0.0071)	(0.0071)	(0.0108)	(0.0072)	(0.0073)	(0.0111)
public	0.0140	0.0134	0.0134	0.0036	0.0030	0.0030
	(0.0111)	(0.0111)	(0.0099)	(0.0114)	(0.0114)	(0.0094)
Spline function	Yes	Yes	Yes	Yes	Yes	Yes
WLS using income	No	Yes	Yes	No	Yes	Yes
Clust std err. by prov	No	No	Yes	No	No	Yes

	Total Income			Taxable Income		
	(1)	(2)	(3)	(4)	(5)	(6)
Observations	21,883	21,883	21,883	21,883	21,883	21,883

Notes: The exclusion restriction is the predicted change in log (1-τ) as described in Chapter 1. The definition of *year t-2 income*, represented as a spline, is the same as the dependent variable. Deciles used to form the spline function are calculated by dividing the sample into ten equal groups according to the year *t-2* value of the income definition used in the regression (ie. either total income or taxable income). In all cases, the sample restrictions applied to the sample are the same as in row 22 of Table 1. All year *t-2* values of taxable income less than $100 have been dropped. Such small values are not appropriate to use in a log-ratio operator to represent approximations in percent change. In the second-to-last column for each income type, estimates are weighted by a product of the sample weight and log of total income. In the final column for each income type standard errors clustered at the province level. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9. Elasticity of employment income: by degree of dominance of employment income and by attachment to the labour force

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
change in log (1-τ)	0.0677	0.1187	0.1371	0.1262	0.0940	0.0627	0.0413
	(0.1317)	(0.1144)	(0.1255)	(0.1218)	(0.0756)	(0.0765)	(0.0792)
Spline Variables							
decile 1	-0.5413***	-0.6464***	-0.6290***	-0.6079***	-0.5930***	-0.6210***	-0.8607***
	(0.0452)	(0.1022)	(0.1180)	(0.1073)	(0.0430)	(0.0492)	(0.0629)
decile 2	-0.3443***	-0.2372*	-0.3201**	-0.3578**	-0.2965***	-0.2900***	-0.2306**
	(0.0934)	(0.1344)	(0.1473)	(0.1492)	(0.0851)	(0.0915)	(0.1003)
decile 3	-0.1270*	-0.1768**	-0.1494*	-0.1331**	-0.1456	-0.2025*	-0.2207*
	(0.0765)	(0.0725)	(0.0830)	(0.0630)	(0.1137)	(0.1202)	(0.1271)
decile 4	-0.2729**	-0.2853**	-0.3070**	-0.3047***	-0.2946**	-0.1654	-0.1632
	(0.1282)	(0.1110)	(0.1199)	(0.1113)	(0.1176)	(0.1233)	(0.1285)
decile 5	0.0084	0.0232	-0.0170	0.0567	0.0865	0.0181	0.1217
	(0.0907)	(0.0924)	(0.1019)	(0.0758)	(0.1147)	(0.1185)	(0.1225)
decile 6	0.0504	0.0541	0.1157	0.0344	-0.0156	0.0133	-0.0725
	(0.1310)	(0.1272)	(0.1207)	(0.0761)	(0.1045)	(0.1067)	(0.1102)
decile 7	0.0295	0.0325	0.0913	0.0962*	0.0636	0.0350	0.0632
	(0.0978)	(0.1010)	(0.0620)	(0.0582)	(0.0921)	(0.0935)	(0.0958)
decile 8	0.0841	0.0856	0.0209	0.0110	0.0675	0.0687	0.0459
	(0.1245)	(0.1259)	(0.1201)	(0.1138)	(0.0763)	(0.0772)	(0.0788)
decile 9	-0.1597	-0.1732	-0.1612**	-0.1484*	-0.1549***	-0.1476**	-0.1309**
	(0.1164)	(0.1070)	(0.0787)	(0.0791)	(0.0595)	(0.0599)	(0.0614)
decile 10	-0.0130	-0.0114	-0.0037	0.0299	0.0100	0.0125	0.0084
	(0.0474)	(0.0463)	(0.0411)	(0.0586)	(0.0147)	(0.0146)	(0.0149)
Year 1 capital income	-0.0013***	-0.0014***	-0.0012***	-0.0008**	-0.0010**	-0.0011***	-0.0010**
	(0.0004)	(0.0004)	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
base year 1999	0.0077	0.0011	-0.0005	0.0007	0.0059	0.0050	0.0065
	(0.0085)	(0.0079)	(0.0067)	(0.0052)	(0.0082)	(0.0084)	(0.0086)
base year 2000	-0.0087	-0.0106	-0.0097	-0.0072	-0.0073	-0.0060	-0.0053
	(0.0114)	(0.0096)	(0.0074)	(0.0062)	(0.0073)	(0.0075)	(0.0077)
base year 2001	-0.0031	-0.0044	-0.0036	-0.0006	0.0023	0.0023	0.0013
	(0.0092)	(0.0077)	(0.0059)	(0.0058)	(0.0053)	(0.0055)	(0.0056)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
change in age squared	-0.0010***	-0.0009***	-0.0010***	-0.0010***	-0.0009***	-0.0009***	-0.0008***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
change in num. kids	-0.0291***	-0.0309***	-0.0281***	-0.0288***	-0.0297***	-0.0271***	-0.0254***
	(0.0048)	(0.0047)	(0.0072)	(0.0069)	(0.0038)	(0.0039)	(0.0040)
primary	0.0556	0.0530**	0.0691***	0.0629***	0.0388	0.0457*	0.0595**
	(0.0357)	(0.0254)	(0.0212)	(0.0201)	(0.0236)	(0.0263)	(0.0278)
private goods	0.0696***	0.0718***	0.0759***	0.0723***	0.0565***	0.0608***	0.0650***
	(0.0209)	(0.0189)	(0.0195)	(0.0198)	(0.0109)	(0.0120)	(0.0123)
public	0.0962***	0.0993***	0.0645***	0.0592***	0.1260***	0.1376***	0.1535***
	(0.0251)	(0.0268)	(0.0172)	(0.0162)	(0.0173)	(0.0182)	(0.0189)
Income mix restrictions; year *t-2*:							
employment inc. > self-employment inc.	-	Yes	Yes	Yes	-	-	-
self-employment inc. = 0	-	No	Yes	Yes	-	-	-
employment inc. > investment inc.	-	No	No	Yes	-	-	-
Worker type restrictions; year *t-2*:							
are paid workers	-	-	-	-	Yes	Yes	Yes
have been in job for 24 months	-	-	-	-	No	Yes	Yes
have FT main job	-	-	-	-	No	No	Yes
Observations	20,760	20,607	19,624	19,477	19,726	18,022	16,661

Notes: The specification used in this table is the same as in columns 3 and 6 of Table 8. The definition of *year t-2 income*, represented as a spline, is the same as the dependent variable: employment income. Deciles used to form the spline function are calculated by dividing the sample into ten equal groups according to the year *t-2* value of employment income. In all cases, the sample restrictions applied to the sample are the same as in row 22 of Table 1. All year *t-2* values of taxable income less than $100 have been dropped. Such small values are not appropriate to use in a log-ratio operator to represent approximations in percent change. We drop those with wage and salary income less than $1,000 in either year *t* or year *t-2*. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10. Elasticity of hours on intensive margin: overall, by gender, with and without inclusion of an income effect control

	All	Male				Female			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Elasticity (compensated)	0.1497***	0.1104**	0.1002*	0.0145	0.0447	0.1587**	0.1609**	0.1076	0.1002
	(0.0395)	(0.0512)	(0.0514)	(0.0591)	(0.0533)	(0.0708)	(0.0721)	(0.0795)	(0.0878)
change in log (1-τ)	296.3637***	229.3949**	208.1173*	30.0348	92.9430	292.6748**	296.8446*	198.5396	184.8948
	(78.1903)	(106.3690)	(106.7925)	(122.8683)	(110.8091)	(130.6647)	(133.0085)	(146.6043)	(161.9810)
change in log (*I-T(I)*)			156.9945	140.3691	138.7205		-84.0941	-54.1734	861.6807*
			(153.6188)	(157.2771)	(156.6813)		(471.6920)	(395.6427)	(399.0372)
base year paid hours	-847.9422***	-1034.7818***	-1025.3672***	-1053.6127***	-1126.6235***	-691.5468***	-700.6454***	-678.2799***	-964.4518***
	(9.7435)	(49.0959)	(60.1769)	(63.7224)	(84.5070)	(32.0765)	(34.6271)	(34.0375)	(91.4787)
base year 1999	0.7015	12.2748	8.3373	20.5225	11.8201	-5.7023	-3.8255	-7.4407	-20.1649
	(7.3154)	(19.0284)	(23.8123)	(29.6886)	(30.4280)	(17.0254)	(19.4239)	(17.3631)	(16.6444)
base year 2000	-28.0761***	-34.4618***	-36.3153**	-15.0069	-20.8050	-11.7495	-11.3633	-14.0076	-17.9355
	(7.1936)	(12.9387)	(15.6295)	(15.8692)	(16.5069)	(12.4557)	(14.0679)	(15.5414)	(15.7273)
base year 2001	-1.4771	-4.4364	-3.0574	-6.4543	-11.8518	5.1997	6.2756	1.0434	-7.2911
	(15.6005)	(20.3648)	(20.2127)	(18.6643)	(17.7255)	(14.8888)	(15.0590)	(13.6188)	(8.2363)
change in age squared	-0.6399***	-0.7679***	-0.6645***	-0.8237***	-0.7723***	-0.5173***	-0.5671	-0.4514	0.0729
	(0.1270)	(0.1708)	(0.1086)	(0.1441)	(0.1610)	(0.1321)	(0.3657)	(0.3297)	(0.3208)
change in num. kids	-23.7923***	-4.9417	-5.1359	-7.7889*	-8.4866*	-54.6894***	-57.3116***	-44.8034***	-25.8328
	(6.7273)	(5.6434)	(6.1001)	(3.9569)	(3.9045)	(10.8575)	(15.9774)	(11.6740)	(15.3542)
Primary	163.1856**	143.5893	138.8248	204.8399	188.2230	172.0792***	177.6974***	253.1868***	202.6335**
	(76.8090)	(95.4613)	(103.8018)	(155.3794)	(159.3478)	(52.3195)	(44.1278)	(69.3820)	(72.2389)
private goods	43.2912***	4.4354	-0.3981	4.0517	2.2375	173.3871***	176.7673**	140.5900**	101.2885
	(9.6823)	(14.2415)	(12.1637)	(12.3087)	(13.4020)	(41.6333)	(55.2164)	(61.5427)	(62.8259)
Public	38.5906	87.4144**	80.9051	82.3051	105.7798**	-28.0953	-31.6127	-29.8398	9.6178
	(24.7432)	(43.0909)	(49.6419)	(59.7687)	(42.4222)	(32.0252)	(25.3365)	(20.6335)	(24.7043)
Restrict to workers who:									
are paid workers	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
have been in job for 24 months	No	No	No	Yes	Yes	No	No	Yes	Yes
have FT main job	No	No	No	No	Yes	No	No	No	Yes
Observations	18,573	10,581	10,579	9,669	9,567	7,992	7,990	7,351	6,500

Notes: The dependent variable is the first-difference of hours paid. The elasticity and standard error are calculated using the nlcom command by dividing the point estimate by the average number of hours worked in the regressed sample. In all regressions, we drop tax-filers with hours paid or hours worked not in (100, 5800) inclusive and with wage and salary income less than $1,000. Because the dependent variable is now measured in terms of hours, we only include year *t-2* paid workers (based on clwkr1) and year *t-2* tax-filers with some employment income in the year. We lose 4,500 observations from the baseline sample by making these restrictions. Where income effects are included, we run two separate first-stage OLS regressions and use the predicted values in the main regression. We *do not* use the Stata command reg3 for the two first-stage equations. All standard errors clustered at the province level. Capital income is excluded from this regression as it was a control for income-distribution-widening in dollar incomes, not for discrete measures such as hours. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11. Elasticity of employment income: robustness of year spacing assumption

	t-1	t-2	t-3
change in log (1-τ)	0.0001	0.0976*	0.0352
	(0.0819)	(0.0587)	(0.0412)
Spline Variables			
decile 1	-0.0513**	-0.0757***	-0.0334
	(0.0224)	(0.0292)	(0.0307)
decile 2	-0.2923***	-0.3938***	-0.3785***
	(0.0440)	(0.0594)	(0.1111)
decile 3	-0.1413***	-0.0671**	-0.2276**
	(0.0471)	(0.0342)	(0.0937)
decile 4	0.0406	-0.0843*	0.0588
	(0.0707)	(0.0504)	(0.1239)
decile 5	-0.0846	-0.0186	-0.2793
	(0.0699)	(0.0556)	(0.1834)
decile 6	-0.0255	-0.0879***	0.1522
	(0.0788)	(0.0336)	(0.1404)
decile 7	0.0236	0.0598	0.0236
	(0.0702)	(0.0800)	(0.0490)
decile 8	0.0434	-0.0436	-0.1265
	(0.0421)	(0.0962)	(0.0864)
decile 9	-0.1119***	-0.0741	0.0472
	(0.0357)	(0.0967)	(0.1210)
decile 10	0.0034	0.0110	-0.0076
	(0.0087)	(0.0322)	(0.0273)
year 1 capital income	-0.0000	-0.0002	-0.0006
	(0.0001)	(0.0003)	(0.0005)
base year 1999	0.0006	-0.0055	-0.0039
	(0.0076)	(0.0098)	(0.0085)
base year 2000	-0.0072	-0.0068	-0.0105*
	(0.0048)	(0.0082)	(0.0057)
base year 2001	-0.0075**	-0.0008	
	(0.0031)	(0.0061)	

	t-1	t-2	t-3
base year 2002	-0.0102***		
	(0.0021)		
change in age squared	-0.0009***	-0.0007***	-0.0006***
	(0.0000)	(0.0001)	(0.0000)
change in num. kids	-0.0053	-0.0095**	-0.0108***
	(0.0033)	(0.0042)	(0.0023)
primary	0.0010	0.0654***	0.0671*
	(0.0220)	(0.0196)	(0.0404)
private goods	0.0097	0.0219***	0.0271***
	(0.0181)	(0.0081)	(0.0083)
public	-0.0068	-0.0059	0.0048
	(0.0188)	(0.0117)	(0.0177)
	209.1324	608.4845	1259.6376
Observations	28,246	19,880	13,192
First-stage F statistic	209.1324	608.4845	1259.6376

Notes: The specification used in this table is the same as in column 1 of Table 9. We drop those with wage and salary income less than $1,000.The number of year dummies decreases with the spacing between years; in all cases it is the latest (more recent) year that is the omitted dummy variable. All years 1999 to 2004 are included; the longer the number of years between observations, the less differenced observations we can construct. In addition, just for this regression we restrict those who have a log-change in earnings not in (ln(0.5), ln(2)) so that outliers do not affect the comparison. For this reason, the second column of this table is not comparable to the first column of Table 9. All standard errors are clustered at the province level. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12. Elasticity of employment income; robustness of tax variable to: METR increment, alternative tax measures (ATR)

	(1)	(2)	(3)	(4)	(5)
change in log (1-τ)	0.0587	0.0677	0.0280	0.0561	
	(0.1256)	(0.1317)	(0.1030)	(0.1244)	
change in log (1-ATR)					0.3431
					(0.3574)
Spline Variables					
decile 1	-0.5411***	-0.5413***	-0.5416***	-0.5412***	-0.5430***
	(0.0452)	(0.0452)	(0.0457)	(0.0453)	(0.0455)
decile 2	-0.3454***	-0.3443***	-0.3435***	-0.3453***	-0.3648***
	(0.0936)	(0.0934)	(0.0954)	(0.0935)	(0.1058)
decile 3	-0.1255	-0.1270*	-0.1243	-0.1264	-0.1166
	(0.0770)	(0.0765)	(0.0848)	(0.0784)	(0.0832)
decile 4	-0.2685**	-0.2729**	-0.2511***	-0.2661**	-0.2563***
	(0.1277)	(0.1282)	(0.0969)	(0.1199)	(0.0817)
decile 5	0.0050	0.0084	-0.0044	0.0051	-0.0372
	(0.0960)	(0.0907)	(0.1049)	(0.0963)	(0.0955)
decile 6	0.0499	0.0504	0.0458	0.0485	0.0384
	(0.1312)	(0.1310)	(0.1243)	(0.1283)	(0.1251)
decile 7	0.0291	0.0295	0.0285	0.0296	0.0349
	(0.0966)	(0.0978)	(0.0981)	(0.0976)	(0.0951)
decile 8	0.0840	0.0841	0.0818	0.0832	0.0820
	(0.1248)	(0.1245)	(0.1247)	(0.1246)	(0.1305)
decile 9	-0.1574	-0.1597	-0.1493	-0.1566	-0.1555
	(0.1187)	(0.1164)	(0.1021)	(0.1130)	(0.1119)
decile 10	-0.0134	-0.0130	-0.0145	-0.0134	-0.0195
	(0.0470)	(0.0474)	(0.0451)	(0.0467)	(0.0459)
year 1 capital income	-0.0013***	-0.0013***	-0.0013***	-0.0013***	-0.0014***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
base year 1999	0.0084	0.0077	0.0105	0.0086	0.0018
	(0.0099)	(0.0085)	(0.0109)	(0.0092)	(0.0220)
base year 2000	-0.0082	-0.0087	-0.0065	-0.0081	-0.0132
	(0.0122)	(0.0114)	(0.0098)	(0.0110)	(0.0194)

	(1)	(2)	(3)	(4)	(5)
base year 2001	-0.0031	-0.0031	-0.0031	-0.0031	-0.0030
	(0.0092)	(0.0092)	(0.0091)	(0.0091)	(0.0086)
change in age squared	-0.0010***	-0.0010***	-0.0009***	-0.0010***	-0.0010***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
change in num. kids	-0.0291***	-0.0291***	-0.0291***	-0.0291***	-0.0313***
	(0.0048)	(0.0048)	(0.0048)	(0.0048)	(0.0049)
primary	0.0556	0.0556	0.0554	0.0555	0.0583
	(0.0356)	(0.0357)	(0.0360)	(0.0357)	(0.0382)
private goods	0.0695***	0.0696***	0.0694***	0.0695***	0.0715***
	(0.0209)	(0.0209)	(0.0211)	(0.0211)	(0.0218)
public	0.0962***	0.0962***	0.0964***	0.0962***	0.0971***
	(0.0250)	(0.0251)	(0.0253)	(0.0252)	(0.0251)
“Marginal” increment value	$10	$100	$1000	METR avg.	ATR
Observations	20,759	20,760	20,760	20,759	20,760
First-stage F statistic	875.9791	699.3570	270.6540	998.8561	788.4902

Notes: The specification used in this table is the same as in column 1 of Table 9. This table compares the results arising from alternative specifications of the key independent variable of interest: the change in the “tax rate”. The second column, with a $100 increment is the method used in all other tables in this paper. $10 and $1000 increments are tested here for comparison. The tax rate in the fourth column, “METR Average” is simply the average value of the METR calculated using the methods in the previous three columns. Using an average will attenuate any outlier effects among any one of the options. Finally, in the fifth column, we use the average tax rate (ATR). The ATR is calculated as the ratio of total tax payable (output from CTaCS) to total income. We drop those with wage and salary income less than $1,000. All standard errors clustered at the province level. Standard errors in parentheses: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 13. Mapping of SLID variables into CTaCS variables

CTaCS Variable	Description	2012 Line	PR var	CF var
addded	Additional deductions before Taxable Income	256		
adoptex	Adoption expenses	313		
age	Age	301	age26	
caregiver	Caregiver claim. Reported line 236 income.	315		
cginc	Capital gains income	127	capgn42	
chartex	Qualifying children art and culture expenses	370		
chfitex	Qualifying children sport expenses	365		
cqpinc	CPP/QPP income	114	cpqpp42	
dcexp	daycare expenses	214	ccar42	
disabled	disability status	316, 215	disabs26	
dmedexp	dependent medical expenses.	331		
dongift	charitable donations and gifts	349		
dues	Union dues or professional association fees	212	udpd42	
dvdinc	Dividend income (Eligible Dividend Income from 2006 on)	120	inva42	
dvdincne	Not Eligible Dividend income (Matters 2006 on)	180		
earn	Earned income	101	wgsal42	
equivsp	Spousal equivalent dependant. Reported line 236 income	303	fslsp26	
fullstu	Number of months full time student	322	fllprt20	
gisspainc	GIS and SPA income	146, 235, 250	gi42	
id	identification variable			
infdep	Infirm dependant age 18+. Reported line 236 income	306, 5820		
intinc	interest income	121	inva42	
kidage1	Age of the youngest child	306		fmcomp46, fmsz46
kidage2	Age of the 2nd youngest child	306		fmcomp46, fmsz46
kidage3	Age of the 3rd youngest child	306		fmcomp46, fmsz46
kidage4	Age of the 4th youngest child	306		fmcomp46, fmsz46
kidage5	Age of the 5th youngest child	306		fmcomp46, fmsz46
kidage6	Age of the 6th youngest child	306		fmcomp46, fmsz46
kidcred	Credits transferred from child's return	327		
male	Reference person is male		sex99	
mard	marital status		marst26, fmcomp46	

CTaCS Variable	Description	2012 Line	PR var	CF var
medexp	medical expenses	330	medx42	
north	Proportion of the year resided in area eligible for Northern Deduction	255	eir25, postcd25, cmaca25	
northadd	Proportion of the year eligible for additional residency amount of Northern Deduction.	256	eir25, postcd25, cmaca25	
oasinc	OAS income	113	oas42	
othcredf	Other non-refundable credits federal	313		
othcredp	Other non-refundable credits provincial	5833		
othded	Other deductions before Net Income	256		
othinc	all other sources of income	130	othinc42	
partstu	Number of months part time student	321	fllprt20	
peninc	Pension / RPP income	115	pen42	
political	political contributions	410		
politicalp	political contributions - provincial	6310		
proptax	Property tax payments for provincial credit		prtxm25	
province	province of residence		pvreg25	
pubtrex	Qualifying public transit expenses	364		
qmisded	Quebec miscellaneous deductions before Taxable Income	[ ]		
qothded	Quebec other deductions before Net Income	[ ]		
rent	Rent payments for property tax credits	6110	rentm25	
rppcon	RPP contributions	207	rppc42	
rrspcon	RRSP contributions	208		
rrspinc	RRSP income	129	rspwi42	
sainc	social assistance income	145, 250	sapis42	
schinc	Scholarship income	130		
self	self-employment income	135	semp42, incfsee, incnfse	
spaddded	Additional deductions before Taxable Income	256		
spage	age	301	age26	
spcginc	Capital gains income	127	capgn42	
spcqpinc	CPP/QPP income	114	cpqpp42	
spdisabled	disability status	316, 215	disabs26	
spdues	Union dues or professional association fees	212	udpd42	
spdvdinc	Dividend income (post 2006: eligible only)	120	inva42	
spdvdincne	Dividend income - not eligible	180		
spearn	Earned income	101	wgsal42	

CTaCS Variable	Description	2012 Line	PR var	CF var
spfullstu	Number of months full time student	322	fllprt20	
spgisspainc	GIS and SPA income	146, 235, 250	gi42	
spintinc	interest income	121	inva42	
spmale	spouse person is female		sex99	
spoasinc	OAS income	113	oas42	
spothcredf	Other non-refundable credits federal	313		
spothcredp	Other non-refundable credits provincial	5833		
spothded	Other deductions before Net Income	256		
spothinc	all other sources of income	130	othinc42	
sppartstu	Number of months part time student	321	fllprt20	
sppeninc	RPP / other pension income	115	pen42	
sppolitical	political contributions	410		
sppoliticalp	political contributions - provincial	6310		
spqmisded	Quebec miscellaneous deductions before Taxable Income	[ ]		
spqothded	Quebec other deductions before Net Income	[ ]		
sprppcon	RPP contributions	207	rppc42	
sprrspcon	RRSP contributions	208		
sprrspinc	RRSP income	129	rspwi42	
spsainc	social assistance income	145, 250	sapis42	
spschinc	Scholarship income	130		
spself	self-employment income	135	semp42, incfsee, incnfse	
spstuloan	Interest on student loan	319		
spteachex	Teaching supply expenditures (for PEI credit)	0		
sptuition	Tuition paid	320		
spuiinc	Unemployment insurance income	119	uiben42	
spvolfire	Volunteer firefighter etc.	362		
spwcinc	Workers' compensation income	144, 250	wkrcp42	
stuloan	Interest on student loan	319		
teachex	Teaching supply expenditures (for PEI credit)			
tuition	Tuition paid	320		
uiinc	Unemployment insurance income	119	uiben42	
volfire	Volunteer firefighter etc.	362		
wcinc	Workers' compensation income	144, 250	wkrcp42	

Notes: Not all variables provided for in CTaCS could be computed using the available information in SLID. In general, the LAD is far more comprehensive than the SLID. The detailed Stata code file in which all SLID variables were converted into CTaCS variables, with assumptions, is available upon request. We thank Kevin Milligan for providing Stata code files that identified many of the above mappings. Composite variables refer to "catch-all" or subtotaled CTaCS variables into which more detailed administrative variables can be included. The headings in the above table are as follows:
2012 line: as a frame of reference, refers to the line number of the item within the 2012 T1 General forms.
PR / CF variable: administrative name of SLID variable. PR refers to person file. CF refers to census family file.
CTaCS variable: administrative name of tax calculator variable. See Milligan (2012) for tax calculator documentation.

Chapter 3. Can Labour Relations Reform Reduce Wage Inequality?

1 Introduction

According to data from the OECD, union membership as a proportion of the workforce declined in all but five OECD countries between 1980 and 2010.[1] In Australia, New Zealand, the U.K., and the U.S. the declines were particularly dramatic. While there are sharply diverging views on whether a smaller role for unions in labour markets is desirable, there is little disagreement that it matters. On the one hand, unions have been shown to reduce corporate profits, investments and dampen employment growth. On the other hand, unions have clear beneficial impacts on the wages, fringe benefits and working conditions of unionized workers.[2] Consistent with this evidence, the set of Anglo-Saxon countries that have experienced the largest declines in unionization internationally, have also experienced the largest increases in inequality. These developments are resulting in heightened interest in the potential for policies aimed at reversing deunionization trends to mitigate growing labour market inequality.[3]

How might greater unionization affect the distribution of earnings? As Fortin et al. (2012) explain, unions tend to compress the wage distribution by raising wages most among low-wage workers and less among high-wage workers, which reduces inequality. At the same time, however, if they raise the wages of unionized workers more than the wage gains obtained by nonunionized workers, unions can actually increase inequality. Thus, greater unionization would reduce wage inequality only if the equalizing effect of unions were to dominate. The literature on income inequality shows that an important part of rising wage inequality in Canada is due to declining union density rates, suggesting that the equalizing effect dominates. For example, Card, Lemieux and Riddell (2004) attribute about 15 percent of the growth in Canadian male wage inequality during the 1980s and 1990s to declining union density, with the proportion of Canadian men who were unionized falling from 47 percent in 1984 to 33 percent in 2001.[4] The decline in union density in the United States — from 24 percent in 1984 to 15 percent in 2001 — is similarly associated with increasing US wage inequality. If one takes into account the broader spillover effects of unions on nonunionized workers' wages, the impact of declining union density is potentially much larger in both countries (Beaudry, Green and Sand 2012; Western and Rosenfeld 2011).

Whether unionization can provide a policy lever to affect inequality depends critically on the extent to which deunionization has been a consequence of government policies (and can therefore potentially be reversed through policy), as opposed to an inevitable development driven by broad globalization and deindustrialization trends.[5] The relative stability of union density rates in Canada,

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[1] Exceptions are Belgium, Chile, Iceland, Norway and Spain. The data are from: http://stats.oecd.org/ and measure the proportion of the workforce that are union members.
[2] For reviews of the evidence on the economic effects of trade unions, see Addison and Hirsch (1989), Kuhn (1998) and Hirsch (2004a, 2004b).
[3] For a formal analysis of the link between deunionization and inequality trends across OECD countries see Jaumotte and Buitron (2015).
[4] The sample in Card, Lemieux and Riddell (2004) includes paid workers ages 15 to 64 earning wages between $2.50 and $44 per hour in 2001 dollars.
[5] Riddell and Riddell (2004) examine changes over time in the probability of given types of workers being unionized, and suggest that these changes are consistent with the effects of legal changes (as well as with a decline

despite its legal, political and cultural similarities and close economic ties to the U.S., suggests that the phenomenon was not inevitable. Comparing survey and opinion poll data, Riddell (1993) finds that the vast majority of the Canada-U.S. gap in union density rates cannot be accounted for by structural economic differences or social attitudes and infers that the gap is most consistent with differences in legal regimes. Following on this evidence, there now exists a substantial Canadian empirical literature linking changes in provincial labour relations laws to administrative data on certification success rates (Martinello 1996; Martinello 2000; Johnson 2002; Riddell 2004, Bartkiw 2008), applications for certification (Johnson 2004), as well as successful negotiations of first contracts (Riddell 2013).[6] This research consistently finds a significant effect of the labour relations regime on the ability of unions to organize new bargaining units. Of particular importance appears to be rules for certification and for insuring that a first contract is successfully negotiated.[7] Supported by this body of research, a frequently mentioned policy option for reversing the deunionization trend in Canada is enacting labour relations legislation that is more supportive of unions.[8]

In establishing that labour relations laws matter for union formation, the current literature is both extensive and highly compelling. However, in informing the potential for legal reforms to not only reverse deunionization trends, but also mitigate inequality trends, it falls short in two key respects. First, changes in union density rates at the aggregate level depend not only on the rate of organizing new union members, but also on relative changes in employment levels within the union and nonunion sectors, including those resulting from expansions and contractions of existing bargaining units, the creation of new firms, and firm closures (Farber and Western 2001). For example, if firms shift production to less union-friendly jurisdictions in response to a more union-friendly legal environment, union density and, consequently, wage rates are affected, but the loss of unionized jobs is not captured in the administrative data on certification and decertification. The current literature has, however, largely overlooked the effect that labour relations laws have on employment levels. For example, in examining the impact of mandatory certification votes on the Canada-U.S. union density gap, Johnson (2004) explicitly assumes that the law has no impact on employment. One would, however, expect such effects to be important as a

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in the demand for unionization as governments improve employment protection and nonwage benefits and employers introduce mechanisms to manage grievances).

[6] Directly relating labour relations laws to unionization is more difficult in the U.S. and U.K. where labour law largely falls under the federal jurisdiction and, therefore, provides little or no cross-sectional variation. For example, in the U.S., collective bargaining for all private sector workers is regulated federally by the National Labour Relations Act (NLRA) and subsequent modifications and interpretative decisions of this Act. Consequently, one has to rely on time-series variation to identify the effects of laws. This is the approach of Freeman and Pelletier (1990) and Farber and Western (2002). An exception is for public sector workers at the local and state government levels within the U.S., where laws vary across occupation groups (e.g., firefighters, police and teachers). This variation is exploited by Freeman and Valletta (1988) and Farber (2005). Also, the 1947 Taft-Hartley amendment of the NLRA allows states to pass right-to-work laws affecting all private sector workers (and sometimes public employees) within the state. Moore (1993) provides a review of the right-to-work laws. For a review of the broader literature, see Godard (2003).

[7] For evidence of the alternative view that deunionization trends in Canada and the U.S. are primarily driven by broader economic factors beyond the influence of public policy and therefore unlikely to be reversed through labour relations reforms, see Troy (2000, 2001).

[8] Some examples are Fortin et al. (2012), Stiglitz (2012) and a number of recent publications from the Canadian Centre for Policy Alternatives, such as Black and Silver (2012). Interestingly, a June 2012 White Paper from the Ontario Progressive Conservative Caucus calls for right-to-work laws in Ontario, which almost certainly would have a dramatic effect on decertification rates in the province, although its implications for wage inequality are less obvious.

more union-friendly legal environment, for example, affects employers' perceived threats of unionization or their relative bargaining power and, in turn, investment, capital utilization, scale, and locational decisions. To identify the general equilibrium effects of labour relations reforms, including employment effects, one has to relate the cross-sectional and/or time-series variation in laws directly to union density rates. To do this, one needs to look beyond the available administrative data. Changes in certification rules might alter not only the outcomes of certification applications, but also the initial decision to begin a union drive. Administrative labour relations data do not capture the latter decision, but the overall effect can be captured by union density rates more generally. We are aware of four studies that relate labour relations to union density rates: one using Canadian data (Martinello and Meng 1992); one British (Freeman and Pelletier 1990); and two from the U.S. (Freeman and Valletta 1988; Farber 2005).

The second key respect in which the current literature falls short is its assumptions regarding the impact of legislation on different worker types. By restricting the effect of legal reforms to be identical across workers within the labour force, the literature tell us nothing about where in the earnings distribution union density rates are expected to increase most.[9] However, from a standard model of rational union organizing activity, we expect that legal reforms will primarily affect workplaces where the net marginal benefit of organizing a new bargaining unit is close to zero. The reason is that where the net benefits of unionization are large, workers will already have incentive to unionize regardless of small changes in legislation. Where unionization is very costly, on the other hand, small reductions in the marginal cost of unionization resulting from legal reforms will be insufficient to alter unionization decisions. It is where the net benefit of unionization is close to zero and becomes more positive as the result of legal reforms that changes in unionization will occur. The question is where are these workplaces? To begin to understand the potential for legal reforms and unionization to address inequality, we need to understand what types of workers are most affected by legislative reform.[10]

In this study we provide evidence of the distributional effects of labour relations reforms by relating an index of the favorableness to unions of each Canadian province's labour relations regime to its union density rates estimated within a number of well-defined groups of worker types over the 1981-2012 period. To estimate these rates we rely on nationally-representative survey data, as opposed to the administrative data that currently predominates the literature. The advantage of the Canadian setting in doing this analysis is that the legislative jurisdiction primarily lies at the provincial level, rather than the national level, as it does in the U.K. and U.S., thereby allowing us to disentangle policy effects from the effects of broader unobserved economic fluctuations correlated with the timing of legal changes. Moreover, given the contentiousness of these laws, changes in governing provincial parties has resulted in

[9] There is, of course, evidence on how rates of deunionization have varied across worker types. For example, we know that deunionization has been particularly dramatic among men employed in manufacturing. But, this does not necessarily tell us anything about how legal reforms affect workers differentially. There is also evidence that the existence of unions serves to reduce earnings inequality among men, but have little impact on, and may even raise, inequality among women (Lemieux 1993; Card 1996; Card, Lemieux, Riddell 2004). But again, this does not tell us anything about the effects of legal reform, which are likely to affect the union density rates of some types of workers more than others.

[10] The only evidence we have found on distributional effects in the existing literature is from Farber and Western (2002), who examine the effects of the U.S. air-traffic controllers' strike in 1981 and the Reagan NLRB appointment of 1983 on the number of certification applications (but not union density rates more generally) separately by industry and occupation groups.

significant historical swings across Canadian provinces and over time in the favorableness of provincial laws to unions, thereby providing substantial policy variation to identify effects.

To identify the distributional effects of legal reforms, we use a dynamic feasible generalized least squares (FGLS) estimator that conditions on a full set of province and year fixed effects, as well as provincial-level measures of unemployment, inflation, the manufacturing share of employment, and public opinion of unions. The aggregate results suggest that shifting every Canadian province's current legal regime to the most union-favorable possible (within the set of laws considered) would raise the national union density rate in the long-run by no more than 8 percentage points, from its current value of 30%. More specifically, we find that legislative changes would have the greatest effect on the union density rate of more highly educated men — particularly those with postsecondary education working in the public and parapublic sector — while the effect would be felt more widely among women, but slightly more among those in the public and parapublic sector.

Using our estimates of the effect of legislation on union density, we derive the wage distributions that might exist under a more union-friendly regime. Among men, we expect reduced wage inequality in a more union-friendly regime, for two reasons. First, higher union density in the public sector would raise wages in the lower and middle parts of the men's wage distribution. Second, we expect some wage compression at the top of the wage distribution, as more men in the private sector with a university degree would be unionized. Among women, we find that the wage distribution would be largely unchanged, since, although a more union-friendly regime would increase union density among women, most women likely to become unionized already have fairly high wages and thus would gain only a very small wage premium from unionization. Overall, a more union-friendly regime would have only a modest effect on reducing wage inequality.

The remainder of the paper is organized as follows. In the following section we describe our empirical methodology for estimating the effects of legal reforms on provincial-level union density rates. In the third section, we describe the data we use to estimate the model and in the fourth section we discuss our findings. In the fifth section we discuss the potential for the changes in union density for different worker types to influence labour market inequality in Canada. The paper concludes with a discussion about the practical policy relevance of our findings.

2 Methodology

Modelling the decision of a union to invest the resources necessary to organize a new bargaining unit involves an optimization problem in which unions compare the relative marginal costs and benefits of additional membership. By influencing these costs and benefits, small changes in the legal environment can potentially alter optimal behaviour, thereby initiating organizing activities in a particular workplace and, in turn, the per-period flow of workers transitioning from the nonunion to union sector.[11] Ideally, we

[11] Similarly, legal changes could influence the marginal cost of decertifying an existing bargaining unit, which would instead increase union-to-nonunion transitions. However, since decertifications are relatively rare, we focus our discussion on certifications. Farber (2015) and Dinlersoz, Greenwood and Hyatt (2014) are two recent papers examining how union determine which establishments to target for organizing drives. Also related to our approach is

could estimate the effect of legal changes directly on these worker-level flows across different types of workers. However, this requires large samples of longitudinal microdata with information on workers' union status and either demographic characteristics or earnings going back to at least the early 1990s, when the key historical variation in laws began. Such data for Canada do not exist.[12] We can, however, estimate provincial union density rates for particular types of workers using repeated cross-sections of nationally-representative household survey data. But this requires that we think carefully about how changes in the per-period flows of workers in and out of the union sector resulting from changes in labour relations laws affect union density rates in the long-run.

Assuming for simplicity a two-state national labour market in which all workers are employed in either the union or nonunion sector, the union density rate in any year t can be expressed as:

$$U_t = (1 - p_{un})U_{t-1} + p_{nu}(1 - U_{t-1}) \qquad [1]$$

where p_{un} and p_{nu} are the union-to-nonunion and nonunion-to-union transition probabilities, respectively. That is, in a world with no possibility of non-employment, the union density rate is equal to the proportion of the previous year's union members that maintain their union status into the next year plus the proportion of the previous year's nonunion members that switch to the union sector. Rearranging terms, equation [1] can be rewritten as the first-order Markov process:

$$U_t = (1 - p_{un} - p_{nu})U_{t-1} + p_{nu}\,. \qquad [2]$$

Assuming the per-period flows p_{un} and p_{nu} are constant over time and sufficiently small so that $1-p_{un} - p_{nu} > 0$, this process implies a steady-state union density rate given by:

$$U^* = \frac{p_{nu}}{p_{un} + p_{nu}}, \qquad [3]$$

which is strictly increasing in the nonunion-to-union transition rate p_{nu} and strictly decreasing in the union-to-nonunion transition rate p_{un}.[13]

Equation [2] implies that one can recover the underlying transition probabilities by regressing aggregate union density rates on their own lagged values. The intercept in the model identifies the numerator in equation [3]; the coefficient on the lagged dependent variable identifies the denominator; and together this provides two equations to solve for p_{un} and p_{nu}. Moreover, assuming that legal reforms favorable to unions raise union density rates by permanently increasing the nonunion-to-union transition rate p_{nu}, one could identify this effect on the long-run union density rate by allowing the legal reform variable to interact with both the overall intercept and the lagged dependent variable (since p_{nu} appears in both the intercept and the lagged dependent variable terms in equation [2]).

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the accounting model of union density by Dickens and Leonard (1985), which provides a framework for determining future union density given current organizing activity.

[12] A possible exception is the Longitudinal Administrative Databank (LAD), which links T1 income tax returns of individuals going back to the early 1980s. However, unlike the survey data we employ, the LAD do not provide any information on workers' education levels or occupations.

[13] This can be derived by either solving the infinite geometric series obtained by substituting in for U_{t-1} or from simply equating $U_t = U_{t-1}$.

Of course, changes in union density rates over time are driven by numerous factors, some of which may be correlated with the timing of provincial changes to labour relations laws. The key empirical challenge is, therefore, to separately identify the effects of the laws from other factors. To do so, we extend the model implied by equation [2] by controlling for province and year fixed effects, as well as a set of province-level covariates intended to capture province-specific trends in union density rates that may be correlated with legislative changes. Specifically, we estimate the linear model:

$$U_{pt} = \alpha U_{p,t-1} + \delta R_{pt} + \theta(U_{p,t-1} \cdot R_{pt}) + x'_{pt}\beta + c_p + y_t + \varepsilon_{pt}\,. \qquad [4]$$

where R_{pt} is an index of the favorableness to unions of the provincial labour relations regime that exists in province p in year t; x_{pt} is a vector of control variables intended to capture underlying province-specific trends in unionization, which includes the inflation rate (based on the all-items CPI), the unemployment rate (age 25 and over), the manufacturing share of employment, and an estimate of public opinion of trade unions based on opinion poll data; c_p and y_t are province and year fixed effects, respectively; and ε_{pt} is an error term with an expected value of 0, but potentially non-spherical variance-covariance matrix.[14] Given variation over time in R_{pt} within at least one province, all the parameters of equation [4] are identified. Equating U_{pt} and $U_{p,t-1}$, the estimates of equation [4] imply an expected steady-state union density rate U_p^*, which depends on all the parameters of the model.[15] Moreover, using union density rates estimated for different subgroups of the labour force, such as more or less educated workers, we obtain evidence of the distributional effects of legal reforms.

It turns out that the term containing the interaction of the lagged dependent variable and the legal index ($U_{p,t-1} \cdot R_{pt}$) is poorly identified in our data. To address this problem, we compare our estimates of the long-run policy effect at the provincial level to those obtained when we impose the restriction $\theta = 0$, so that legislation only affects the intercept through δ.[16] Having shown that the implied steady-state effects are similar whether the interaction term effect θ is estimated or not, we estimate the effects for subgroups of the population using the restricted model.

It is well known that a consequence of including the lagged union density rate in equation [4] is that the ordinary least squares (OLS) estimates are biased. They are, however, consistent if the error term ε_{pt} contains no serial correlation. Using a Breusch-Godfrey test of autocorrelation based on the OLS fitted errors from estimating equation [4] we are unable to reject the null hypothesis of no serial correlation.[17] However, efficiency gains can be made using a feasible generalized least squares (FGLS) estimator that

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[14] See Section 3.4 for detailed descriptions of each of the control variables.

[15] Equating U_{pt} and $U_{p,t-1}$ in equation (1.4), we obtain the expected steady-state union density rate:

$$U_p^* = \frac{\delta R + W}{(1-\alpha-\theta R)}$$

where $W = x'_{pt}\beta + c_p + y_t$. Taking the derivative of this term with respect to the legal index R implies an effect on the steady-state union density rate given by:

$$\frac{\partial U^*}{\partial R} = \frac{\delta(1-\alpha)+\theta W}{(1-\alpha-\theta R)^2}\,.$$

[16] In this case, the effect of a marginal change in the legal index on the steady-state union density rate is simply $\partial U^* / \partial R = \delta / (1-\alpha)$.

[17] We also performed tests of: (i) the poolability of the parameters across provinces; (ii) heteroskedasticity; and (iii) stationarity. The results are discussed in the notes of Table 5.

estimates the structure of the variance-covariance matrix of the error term. We therefore begin by comparing the estimates across four estimators: OLS; FGLS with province-specific heteroskedasticty; FGLS with province-specific heteroskedasticity and spatial correlation; and FGLS with province-specific heteroskedasticity, spatial correlation, and province-specific autocorrelation.[18] Reporting separate results for the models with and without the θ interaction term discussed above, we obtain eight sets of estimates. As it turns out, the estimated steady-state effects of policy reform are remarkably robust across specifications. Given the statistical challenge of identifying these effects for particular subgroups of the population, we take as our preferred specification the estimator with a smallest variance and then examine the robustness of the estimates to: (i) including province-specific linear time trends to capture any possible remaining latent provincial trends correlated with legal reforms; (ii) sample weights based on the underlying number of observations used to estimate the provincial union density rates; and (iii) an alternative source of data on union density rates based on administrative data on union membership. We conclude our analysis by estimating the distributional effects of legal reform by comparing the magnitude of the long-run estimated effects for 12 groups defined by educational attainment (high school completion or less, completion of a postsecondary certificate or diploma, and completion of a university degree[19]), gender and whether they work in the private or public/parapublic sector.

3 Data and Trends

To examine the effect of changes in provincial labour relations legislation on union density and on the distribution of workers' wages, we rely on a number of household surveys conducted by Statistics Canada to construct union density rates and wages since 1981. Specifically, we use the Survey of Work History for 1981; the Survey of Union Membership for 1984; the Labour Market Activities Survey for the period from 1986 through 1990; the Survey of Work Arrangements for 1991 and 1995; the Survey of Labour and Income Dynamics for 1993, 1994 and 1996; and the Labour Force Survey for 1997 through 2012. Our approach to constructing union density rates using these data is described below in Section 3.2. Unless otherwise stated, we use samples of paid workers for whom we have complete information on

[18] If the variance-covariance matrix of the error term ε_{pt} is given by Ω, then in the most flexible case we estimate:

$$\Omega = \begin{bmatrix} \sigma_1^2\Omega_1 & \sigma_{1,2}I & \cdots & \sigma_{1,10}I \\ \sigma_{2,1}I & \sigma_2^2\Omega_2 & \cdots & \sigma_{2,10}I \\ \vdots & \vdots & \ddots & \vdots \\ \sigma_{10,1}I & \sigma_{10,2}I & \dots & \sigma_{10}^2\Omega_{10} \end{bmatrix}.$$

Not allowing province-specific serial correlation imposes that the diagonal matrices Ω_j are all equal to a $T \times T$ identity matrix; not allowing spatial correlation imposes that all the off-diagonal elements $\sigma_{i,j}$ are zero; and not allowing for heteroskedasticity imposes that σ_j^2 is a constant equal to σ^2. This model is similar those in Freeman and Pelletier (1990) and Nickell et al. (2005).

[19] Education categories are not entirely consistent across surveys and they change over time. Statistics Canada (2012) offers some guidance with respect to the LFS question design adopted by many surveys. In 1989 or earlier, post- secondary certificates and diplomas referred to education that normally requires high school graduation and resulted in a certificate or diploma, but less than a university degree such as a bachelor's degree. In 1990 and later, the high school requirement was removed to allow more persons into the post-secondary education category. Postsecondary certificates and diplomas include trades certificates or diplomas from vocational or apprenticeship training; non-university certificates or diplomas from a community college, CEGEP, school of nursing, etc.; and university certificates below bachelor's degrees. The university degree category normally includes those with a bachelor's degree, or degrees and certificates above a bachelor's degree.

gender, education, province of residence, industry and union status. We should note that all employees who are covered by a collective agreement are considered unionized, not just those who are union members.[20]

The rules governing the formation, operation and destruction of union bargaining units in Canada are normally specified by the labour relations code of the province in which an employee works. However, not all workplaces within a province are governed by these provincial statutes. For example, labour relations for employees of the federal government are governed by the *Public Service Labour Relations Act (PSLRA)*, while employees in federally-regulated industries, such as air transportation, banking and uranium mining, are regulated by the *Canada Labour Code*. While workers in the banking sector are governed by federal labour relations legislation, most individuals working in finance or insurance are governed by provincial legislation. Provincial civil servants, police, firefighters, teachers, and hospital workers on the other hand are, in some cases, but not all, governed by separate statutes. For the most part, provincial exceptions in labour relations legislation affect the management of disputes and the right to strike, and differ from one province to another. In Ontario, for example, hospital workers' certification procedures are governed by the Ontario Labour Relations Act, while dispute resolution in that sector is governed by the Hospital Labour Disputes Arbitration Act. The proportion of workers governed by such special legislation is small but important for our measurement of union density. Ideally, one could separately identify each of these exceptional cases in the data in order to relate the relevant legislation to union density rates of each employee group. However, with the exception of the federal government employees, the level of industry and occupation detail provided in the data is inadequate.

However, as we have emphasized, our primary objective is to identify the effect of legal environment broadly defined. When governments change provincial statutes, the effects are likely to not only have spillover effects on workers falling under separate statutes, but are also likely to be correlated with other legal decisions that affect the broad legal environment and, in turn, the union density rates of excluded groups. For example, special statutes typically exist primarily to regulate the right to strike where employees are providing services deemed essential. Consequently, key regulations affecting union density rates, such as rules for certifying new bargaining units, are taken from the overriding provincial statutes on which are index is based. Moreover, in some cases amendments to provincial statues coincide with comparable changes in the special statutes. As well, it may be that political swings that result in legislative changes lead to broad changes in the labour relations environment within a province. To take a particular example, a change in government to a relatively labour-friendly administration, may lead to both a more union-friendly legal regime and an increasing reluctance of the government to force, through legislation, public sector workers who are in a legal strike back to work, which could influence subsequent employment growth and thereby membership. The key point is that in not excluding public-sector employees (with the exception of federal civil servants) from our analysis, we potentially capture the effect of broader changes in the labour relations climate within a province. Given that we are primarily interested in the distributional effects of the labour relations reforms, and changes in labour relations laws rarely happen in isolation, we think that this broad scope is most relevant.

[20] The difference between union membership and coverage varies by province and over time. The 1981 Survey of Work History identifies only membership. We impute the coverage rate for the 1981 Survey of Work History using the percentage of covered workers by province from the 1984 Survey of Union Membership. See Table 13 for more detail on treatment of inconsistencies across surveys.

Using the industry information available in the surveys, we chose to analyze the private and public/parapublic sectors separately. The public and parapublic sector includes all individuals working at the provincial and municipal levels in utilities, educational services, health care, social assistance and public administration. We exclude federal employees as they are clearly governed by federal legislation. All other workers are defined as in the private sector. In distinguishing between workers employed in the public and parapublic sector and those employed in the private sector, we do not use the surveys' standard "class of worker" classification, because the Labour Market Activities Survey, on which we rely for five years of our data, does not provide it. Judging by the Labour Force Survey's class-of-worker data, however, we have found that our categorization based on industry classification captures well industries that unambiguously fall within the private sector. In addition, using industry classification to identify public sector employees also appears to capture well employers that operate privately but are either publicly funded or heavily regulated and, therefore, are often thought of as falling within the public sector.[21]

3.1 Wage inequality

In determining how changes to provincial labour relations legislation might influence the distribution of wages and income inequality, we first present changes over time in the distribution of hourly wages (stated in constant 2013 dollars) within groups of workers. Specifically, we look at the log hourly wages of unionized and nonunionized men and women in 1984 and 2012.[22]

The density of log wages presented in Figure 1 shows the relative frequency of unionized and nonunionized women with particular (log) hourly wage rates in the two years. In 1984, the density of wages of nonunionized women peaked just above the average provincial minimum wage that year of $7.76 (in 2013 dollars), indicated by the grey vertical line at ln(7.76) = 2.05. In other words, in 1984 it was most common for nonunionized women to be earning just above the minimum wage. (In the figure, the density values on the vertical axis are defined so that the area under the curve sums to 1. In this case, for nonunionized women in 1984, the percentage of women earning wages at or below 2.09, or $8.10 per hour in 2013 dollars, was 25 percent.) In 2012, the distribution of wages of nonunionized women was quite similar in shape, also peaking just above the average minimum wage that year of $10.15, indicated by the black vertical line at ln(10.15) = 2.23. Over time, therefore, there was a clear rightward shift in the distribution of — in other words, a general increase in — hourly wages among nonunionized women.

Figure 1 also shows a clear difference in the wage distribution of unionized and nonunionized women in 1984 and 2012. In both years, few unionized women worked for wages close to the minimum wage; instead, they were likely to earn wages near the middle and top of the wage distribution. In 2012,

[21] For example, in the 2012 Labour Force Survey sample, more than 99 percent of workers in manufacturing and wholesale/retail trade are classified as private sector employees using the class of worker variable. Transportation/ warehousing is the only industry we classify as private sector that has a significant public sector component (23 percent). Among those classified as in the public/parapublic sector, the likelihood of being classified as in the private sector is typically low: 18 percent in utilities, 8 percent in education and 0 percent in public administration. The exception is health care and social assistance, where 47 percent of employees are classified as in the private sector.

[22] It would be preferable to use 1981, but the Survey of Work History does not identify individuals' union coverage.

the median log wage of nonunionized women was 2.78 ($16 per hour), while the median log wage of unionized women was 3.18 ($24 per hour).

The wage distribution of unionized women was also narrower than that of nonunionized women in both years, as reflected in the lower inequality measures summarized in Table 1 (panel a). For example, the 90-10 differential in log wages shown in the table describes the difference between the wages of the highest-earning 10 percent (the 90th percentile) and the lowest-earning 10 percent (the 10th percentile) of workers. In 1984, this differential was 0.981 for unionized women and 1.099 for nonunionized women, indicating greater inequality in wages among nonunionized women. By 2012, these inequality measures had increased for both unionized and nonunionized women; they are reflected in Figure 1 in the general widening of the distribution of wages of both groups of women.

The wage distribution of the nonunionized men, represented by Figure 2 and Table 1 (panel b), takes a very different shape than that of nonunionized women. In particular, in both 1984 and 2012, men were much less likely than women to be working for wages near the minimum wage (indicated by the vertical lines in Figure 2). As well, more of the mass of the wage densities of both unionized and nonunionized men overlapped in both years than was the case for women. In other words, there were fewer differences between unionized and nonunionized men's wage distributions, as more unionized men fell in the middle of the wage distribution than was the case for women.

What is also distinct about men's wages is the way in which their distribution changed between 1984 and 2012. For nonunionized men, wages increased the most for those in the lowest part of the wage distribution (Figure 2), resulting in a slight decrease in most measures of wage inequality among this group (Table 1, panel b). For example, the 90-10 log differential for nonunionized men fell from 1.447 in 1984 to 1.416 in 2012. In contrast, the distribution of wages of unionized men widened between the two years, reflecting relatively stagnant wages in the lower half of the distribution and large increases at the top end. As a result, measures of wage inequality increased among unionized men — much more so than among women, whether the women were unionized or not.

3.2 Union Density

These wage distributions do not show, however, the extent to which the composition or size of each group changed over time. In fact, there was a substantial decline in union density over the period from 1981 to 2012, which varied in magnitude across different types of workers. From the household surveys referred to earlier, we measured union density as the share of employees covered by a collective agreement within each province, sector and demographic group. For years in which a household survey was not available, we used a simple linear interpolation of neighbouring years' group-specific union density rates.[23]

[23] The only survey year for which we could not clearly identify all workers covered by a collective agreement is 1981 — in that year, the Survey of Work History identifies only union membership. To adjust for this, we estimated a union coverage rate by first calculating union membership in the 1981 Survey of Work History for each demographic group considered, and then added to it a within-group difference between the membership and coverage rates estimated from the Survey of Union Membership for 1984.

In Table 2 we consider long-term declines in union density rates across provinces and worker types by comparing rates in 1981 and 2012. The estimates point to relatively large declines in New Brunswick, British Columbia and Alberta; in manufacturing and private services; and among men. In most cases, the three-decade decline in unionization is more than twice as large for men as women, whether measured in terms of the change in the level of the rate or the proportionate change. There appears relatively little difference in deunionization trends across broad occupation groups, although in the two western-most provinces – Alberta and British Columbia – the overall declines have clearly been much larger among blue-collar workers.

As Figure 3 shows, all provinces experienced a decline in union density rates from 1981 to 2012, especially among men. In most provinces, the bulk of the decline occurred from the 1980s to the mid-1990s. In British Columbia, however, the decline continued well into the 2000s, and by 2012 the rate had fallen to only 28 percent among men, from 55 percent in 1981. At 20 percent, Alberta's union density rate among men in 2012 was the lowest of any province, while Quebec, at 40 percent among men, had the highest rate.

The decline in union density over this period is largely a reflection of falling union coverage in the private sector, as shown in Figure 4. At the national level, private sector union density declined by 16 percentage points over the period, with the largest decline occurring in British Columbia and the smallest declines in Alberta and Saskatchewan. Union density also declined — by 13 percentage points nationally — in the public and parapublic sector, but this change was relatively small considering public sector union density rates ranging from 56 to 70 percent in 2012. It is important to note that the decline in private sector union density does not reflect merely structural changes in provincial economies; we show in Section 4 (and Table 3) below that the downward trend in union density also exists at the industry and occupation level.

It is also worth emphasizing that the decline in union density occurred chiefly among men, as Figure 5 shows. Nationally, men's union density rates declined by 20 percentage points between 1981 and 2012, while women's union density rates declined by only 5 points, and in some provinces they barely changed. Looking again at Figure 3, union density among women actually has trended upward in several provinces in more recent years. Saskatchewan is especially noteworthy, with union coverage among women reaching 40 percent in 2012.

Finally, in all provinces, there was a decline in union density rates among all education groups between 1981 and 2012, as shown in Figure 6. In some provinces, such as Ontario and British Columbia, the most-educated appear to have experienced the smallest decline in union density, but in Quebec, Nova Scotia, Manitoba and Prince Edward Island union density declined the most among university graduates. Nationally, however, no particular education category is more heavily unionized than others (not shown). The ubiquity of these trends across provinces, as well as the large gender difference, emphasizes that an important part of the deunionization trends are driven by factors beyond labour relations laws. The empirical challenge is to determine to what extent the declines in Table 2 reflect changes in provincial labour relations laws.

There are two significant limitations of the household survey data that we employ: (i) missing years (specifically 1982, 1983, 1985, and 1992); and (ii) substantial sampling biases in the estimation of union density rates arising from the limited sample sizes, particularly prior to 1997 when the Canada's monthly Labour Force Survey (LFS) first introduced a question identifying union status. To provide ourselves with some confidence in the accuracy of our estimated provincial time-series prior to 1997, we compare our estimates to those obtained using comparable provincial time-series data based on mandatory union filings under the *Corporations and Labour Unions Returns Act* (CALURA). Specifically, prior to 1996 all unions with members in Canada were required to file an annual return in December of each year reporting the total number of union members within each union local. These counts were then aggregated at the provincial level and published annually by Statistics Canada. To obtain provincial union density rates we divide these membership levels by estimates of provincial employment from the LFS. This provides us with union density rates from 1976 to 1995, which can be combined with the 1997 to 2012 LFS data to produce a complete series. However, to make the LFS series consistent with the CALURA, for this comparison series we exclude from the LFS data employees who are covered by union contracts, but not union members.[24]

The resulting provincial time-series of union density rates using both the household survey data (labeled HS-LFS) and CALURA (labeled CALURA-LFS) are plotted in Figure 7.[25] Consistent with Table 2, both data sources point to larger declines in New Brunswick, Alberta and British Columbia. However, in all provinces the long-term declines are smaller in the CALURA-LFS series. In fact, in Prince Edward Island, Nova Scotia, Quebec, Manitoba and Saskatchewan there is little or no evidence of a long-term secular decline in unionization in the administrative data. One possible explanation is that deunionization has occurred primarily through a decline in workers covered by union contracts, as opposed to union membership. Indeed, to some extent, this has been the experience in Australia, the United Kingdom and New Zealand, where declines in union coverage rates since the early 1980s have exceeded declines in union membership rates (Schmitt and Mitukiewicz 2011).[26]

The key advantage of the survey data is that it allows us to estimate union density rates for particular subgroups of the population. Before considering the role of labour relations laws, we examine to what extent Canadian deunionization trends can be accounted for by compositional shifts in employment across provinces, industries, occupations, education groups and gender. For example, union density rates have always been higher in the manufacturing sector than in private services. Consequently, employment shifts away from manufacturing towards services, will push aggregate union density rates downwards for reasons unrelated to labour relations laws.

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[24] There are two significant complications in comparing the LFS and CALURA rates. First, unions with less than 100 members did not have to provide information in the CALURA. This will tend to underestimate union density rates in the CALURA relative to the LFS. On the other hand, CALURA membership counts include union members who are not currently employed, such as workers on temporary layoff, and are recorded as of December 31 of each year, when seasonal layoffs are typically highest. Consequently, dividing by December employment levels tends to overestimate union density rates, particularly for the Atlantic Provinces where seasonal layoffs are most prevalent. To limit this measurement error, we instead use employment levels estimated using the July LFS files. For detailed information on the comparability of the CALURA and LFS data, see Table 14.

[25] Note that we are missing some years in both time series. The CALURA are missing 1996 and with the series based on survey data are missing 1982, 1983, 1985, and 1992. To fill in these gaps we use a simple linear interpolation of the neighbouring years. For 1985, 1992, and 1996, this is simply an average of the values for the years on either side of the missing year. For 1982 and 1983 we use a weighted average (e.g. 1982 is two-thirds of the 1981 value and one-third of the 1984 value).

[26] Another difference with the CALURA data series is that professional organizations certified as unions, such as teachers federations and nurses associations, were not included prior to 1983 (Mainville and Olinek 1999). This will tend to understate union density rates in the early 1980s, resulting in flatter profiles over time.

To quantify the role of these compositional shifts more generally, we compare the estimates from two different regressions, the results of which are reported in Table 3. In the first, we pool the aggregate provincial-level HS-LFS union density rates plotted in Figure 7 and regress them on linear (specification 1) or quadratic (specification 2) time trends. In the second, we do the same thing using union density rates estimated at the level of a particular province-industry-occupation-education-gender group. With 32 years of data this gives us 320 observations in the first case (32 x 10 provinces) and 23,040 in the second (32 x 10 provinces x 4 industries x 3 occupations x 3 education groups x 2 genders).[27] Estimating the union density rates at this detailed level compromises the precision of the estimates significantly. However, since there is no reason to believe that the expected value of this measurement error is correlated with a trend (although its variance is decreasing due to larger sample sizes beginning with the LFS in 1997), it should not bias our estimates.

The first two columns of Table 3 point to a downward trend in unionization when the rates from all provinces are pooled. The linear specification points to an annual decrease of 0.37 percentage points, while the quadratic specification suggests that the rate of decline is decreasing such that by the end of our sample period, rates have stabilized (the slope of the time trend is -0.0065 x 0.0002**time*, where *time* is equal to 32 in 2012). To the extent that this declining trend reflects employment shifts across groups, it should not be evident within groups. However, the third and fourth columns of Table 3 suggest only slightly smaller rates of decline when we use the group-specific union density rates. The linear specification now suggests an annual decline of 0.31 percentage points, while the quadratic specification suggests rates stabilized by 2009. These results imply that something more than structural economic shifts are responsible for decreasing Canadian union density rates over the past three decades.[28]

3.3 The Labour Relations Index

The current literature has taken one of three approaches to empirically identifying the effects of labour relations laws on union density rates. The first is to focus on the effects of particular types of regulations, such as automatic certification or first-contract arbitration. While focusing on a particular regulation makes interpreting estimates relatively straightforward, new regulations are seldom introduced in isolation, so that the estimates potentially capture the effects of concomitant legal changes. To identify the independent effect of particular regulations, other features of the legal regime need to be controlled for, but knowing what these features should be is unclear. Moreover, because the legal changes are highly collinear, disentangling their independent effects with meaningful statistical precision becomes a challenge. An alternative strategy is to focus on the effects of political regime changes where there has been a clear and significant shift in the favorableness of legal regime to unions. Martinello (2000), using data from the Canadian province of Ontario, and Farber and Western (2002), for the U.S., provide examples of this strategy. Unfortunately, these types of regime switches are rare. A third approach, which we follow in this paper, is to exploit variation across a broad set of regulations, but combine the variation into an overall index capturing the favorableness to unions of the law. This is the approach of Freeman

[27] The way in which we mapped the detailed survey variables on industry, occupation and education to these aggregated categories is available upon request.

[28] Hirsch (2008) does a similar compositional analysis by directly decomposing changes in union density into: (i) within-sector changes in union density; and (ii) changes in the sector-specific employment shares. Using this approach we find that the entire change in the national union density rate between 1981 and 2012 can be accounted for by changes in union density rates within either four major industry or three occupation groups. These results are available upon request.

and Valletta (1988) and Farber (2005), who examine union density rates of U.S. public sector workers, and Freeman and Pelletier (1990), who examine long-term changes in the U.K. national union density rate.

The advantage for us in employing an index is twofold. First, the primary objective of our analysis is to identify the potential for broad shifts in provincial labour relations regime, as opposed to specific types of regulations, to differentially affect the union density rates of different groups of workers. By using an index, we obtain estimates of a *single* coefficient, the magnitude of which can be compared in a straightforward way across different samples of workers to obtain evidence on where legal changes are likely to have their biggest impact. Second, by pooling all the variation in a single variable, we estimate these effects with greater statistical precision, so that differences in the magnitudes of the estimates across groups are less likely to reflect random sampling error. This efficiency gain, however, comes at a cost. In constructing the index, one has to arbitrarily set weights on the relative contributions of the individual regulations to the index. To the extent that the weights chosen are incorrect, the resulting index will provide an inaccurate measure of the favorableness to unions of a province's legal regime. However, as Freeman and Pelletier (1990) emphasize, the effect of this measurement error should be to attenuate the estimated effects. Since we are primarily concerned with the relative differences in the magnitude of the estimated effects, as opposed to their overall levels, this bias is of secondary importance in our analysis.

In constructing our index, we restricted our attention to 12 particular aspects of labour relations addressed in provincial statutes governing labour relations in the private sector as well as municipal government workers (the timing of these laws in each province is summarized in Table 4). Closely following the description of legislation in Johnson (2010), the laws we consider are:

- *the secret ballot certification vote*, whereby certification of new bargaining units requires majority support in a mandatory secret ballot vote;
- *first-contract arbitration*, whereby the union or employer can request that a third-party arbitrator be assigned to impose the terms and conditions of the collective agreement;
- *anti-temporary-replacement laws* that prohibit employers from hiring temporary replacement workers during a work stoppage and that limit the use of existing employees;
- *a ban on permanent replacements*, whereby employers are prohibited from hiring permanent replacement workers during a work stoppage;
- *a ban on strikebreakers*, whereby employers are prohibited from hiring individuals not involved in a dispute primarily to "interfere with, obstruct, prevent, restrain or disrupt" a legal strike;
- *reinstatement rights*, whereby striking workers are granted the right to reinstatement at the conclusion of the strike, with priority over temporary replacement workers;
- *compulsory dues checkoff*, whereby a union may request that a clause be included in the collective agreement that requires employers to deduct union dues automatically from employees' pay and remit them to the union;
- *a mandatory strike vote*, whereby the union must demonstrate, through a secret ballot vote, that it has the majority support of the bargaining unit before it can legally strike;
- *an employer-initiated strike vote*, whereby the employer may request that a secret ballot vote be held to determine if the bargaining unit is willing to accept the employer's last offer;

- *compulsory conciliation*, which requires some form of third-party intervention to encourage a contract settlement before a legal work stoppage can occur;
- *a cooling-off period*, which mandates that a number of days must pass after other legal requirements have been fulfilled before a legal work stoppage can begin; and
- *a technology "reopener,"* which permits, at the union's request, that a clause be included in the collective agreement that allows the contract to be reopened before its expiry in the event that the union is concerned about the consequences of technological change.

With respect to the laws governing these 12 aspects of labour relations, we assigned a value of 0 if the law is relatively unsupportive of unions and 1 if it is relatively union friendly. In the year a law was introduced, we assigned a fraction representing the portion of the year the law was in place. Our final labour relations index is then simply the unweighted average of the [0,1] values in each province in each year. Changes to labour legislation are rarely enacted in isolation; accordingly, changes in the labour relations index capture instances where several legislative changes are made simultaneously.

Again looking back at Figure 3, the labour relations index is plotted alongside union density rates for each province and, important for our analysis, displays variation both across provinces and over time within provinces. Some provinces, such as Manitoba, generally have had labour relations legislation that is more supportive of unions, while legislation in others, such as Alberta, has been generally less supportive.

Figure 3 also reveals important differences in union density rates across provinces that do not necessarily align with differences in their labour relations environment. For example, British Columbia's 1981 union density rate among men, at 55 percent, was among the highest in the country, while Alberta's, at 38 percent, was among the lowest, clearly reflecting the more supportive labour relations environment in British Columbia than in Alberta. In contrast, Manitoba and Saskatchewan had similar union density rates from 1981 to 2012 despite substantial differences in their labour relations environments.

Overall, there were large declines in union density, particularly among men, and most prominently in the private sector. There is, however, no clear pattern across education groups, and no evidence to suggest that positive changes in the legislative environment had clearly positive effects on union density. Moreover, the descriptive evidence provides no indication of which workers would be most affected by legislative changes or the affected workers' likely placement in the wage distribution. Our strategy, then, is to estimate the changes in gender- and education-specific union density rates that might result from changes in labour relations legislation, while controlling for general differences across provinces, national differences across years and provincial trends in various other factors that could affect union density in a province.[29] We then use this information to link legislative changes to potential changes in the distribution of wages.

3.4 Control Variables

[29] In Section 4.2 below we estimate these effects for further disaggregated groups where the sample sizes from the household surveys are large enough to generate precise time series estimates of the union density rate in all provinces.

To control for the broader trends that are common across provinces we include a full set of year fixed effects. However, as is evident in Table 2 and Figure 7, deunionization has clearly been stronger in some provinces – New Brunswick, Alberta and British Columbia – than in others – Newfoundland, Manitoba and Saskatchewan. We, therefore, also include a set of control variables that employ province-specific data, as well as examine the robustness of the estimates to including province-specific linear trends. Below we justify our choice of controls and describe the data we employ.

Inflation rate:

In periods of high inflation workers' real wages are often eroded. An important benefit of unionization is that unions typically negotiate clauses in collective agreements providing members with automatic cost of living wage adjustments. Since the demand for these COLA clauses, and therefore unionization, is expected to be higher in situations where inflation is high and the legal regime itself may be influenced by levels of inflation, we control for provincial-level inflation throughout our analysis. To do this, we use the all-items Consumer Price Index (Basket 2009, Year=2002). Note, that we use the inflation rate (year-over-year change in CPI), and not the *level* of the CPI.[30]

Unemployment rate:

Another key benefit of unionization is that it provides its members with increased job security, through seniority rules and restrictions on employers' use of technology to replace workers. Therefore, we would expect the demand for unionization to be increasing in provincial unemployment rates. In addition, job destruction during a recession may occur differentially in unionized workplaces, due primarily to higher fixed labour costs and therefore greater incentives for labour hoarding. Since provincial government initiatives to augment the labour relations environment may itself be influenced by business cycle fluctuations, it is important to condition on the unemployment rate. To do this we include the provincial unemployment rate among individuals aged 25 and over in all the estimated regressions.

Manufacturing share of employment:

There is considerable evidence that an important component of the long-term secular decline of unions in Canada and other OECD countries has been driven by structural economic shifts, in particular the shift from manufacturing to service-producing employment beginning in the 1980s. Since these trends are likely to have occurred differentially across provinces, and may be themselves correlated with changes in labour laws, we follow Bartkiw (2008) and Freeman and Pelletier (1990) and control for the manufacturing share of paid employment. These annual shares are estimated using the industry codes in the 1976 through 2012 Labour Force Survey (LFS) microdata files.

Popular preferences for unions:

Changes in union density rates are driven by individual preferences for unionization in the population, but these preferences are, in turn, likely to be correlated with political preferences and the decisions of politicians to augment labour relations laws. To capture changes in preferences that may be correlated with both union density rates and our legal index, we exploit two sources of public opinion poll data – the

[30] Provincial CPI series begin in 1979, so for the regressions using the CALURA-LFS data series, which begins in 1976, we use the national CPI for 1976-1978.

Canadian Gallup Poll and the Canadian Election Study. The Canadian Gallup Poll surveyed individuals about their perceptions of unions between 1976 and 1989, and again between 1991 and 2000, while the Canadian Election Study contained questions about perceptions of unions between 1993 and 2008. Given the changes in the exact wording of poll questions over time and missing years, a separate model is estimated to obtain consistent provincial time-series measuring popular tastes for unions.[31]

4 The Effect of Labour Relations Reform on Union Density

We begin by examining the results from estimating the lagged dependent variable (LDV) model defined in equation [4] of Section 2.[32] In Table 5, we compare the results with and without the interaction of the LDV and legal index and across 4 alternative specifications of the error variance-covariance matrix. We then choose our preferred estimator and in Table 6 examine the sensitivity of the estimates to: (i) using the administrative CALURA-LFS data based on union membership counts; (ii) including province-specific quadratic trends[33]; and (iii) weighting observations by the underlying sample sizes used to estimate the union density rates.

In the absence of the LDV-labour relations index interaction (columns "a"), the coefficients on the LDV vary between 0.64 and 0.71. In terms of the underlying dynamics defined by equation [2], this implies considerable annual job flows in and out of the union sector and a gradual adjustment of union density rates following legal reforms. The interaction terms (columns "b") are generally not well identified, although the point estimates are negative in all cases. This is consistent with our expectation that a shift towards a legal environment more favourable to unions will serve to increase the nonunion-to-union transition rate p_{nu}. Similarly, the positive and significant coefficients on the legal index itself across all specifications are, in terms of the structure given by equation [2], consistent with more favourable laws increasing nonunion-to-union transitions. To obtain an estimate of the long-run effect of legal reform, we predict the effect of increasing the legal index from average provincial value observed in 2012 (weighted by the population of each province) to one. Given the dynamic structure implied by equation [3], the estimates in Table 5 imply a long-run increase in the national union density rate ranging from 5.5 to 7.6 percentage points. Given an actual national rate of 30.6% in 2012, this represents roughly a 20 percent increase.

[31] Specifically, we map the categorical responses in each poll regarding support for unions into a binary variable: one for a favorable perception of unions and zero for a neutral or negative opinion. We then estimate a probit regression of this variable on a quadratic time trend; a set of province dummies; a set of province dummies interacted with both time and time-squared; and survey indicators to control for survey effects (in particular, changes in exact wording of questions). We then use the parameters from the probit to fit the model between 1976 and 2012 by province, thereby generating the "tastes" variable used to estimate equation [4].

[32] Note, in Legree, Schirle and Skuterud (forthcoming) we use a re-defined *weighted definition* of our legal index that puts relatively greater weight on, for example, card check legislation. In addition, following the work of Budd (2000), we take into account the interactions among varies forms of strike legislation. In the version of our paper presented within this thesis chapter, the twelve laws we consider are not weighted (or are weighted equally) within our legal index.

[33] We restrict the quadratic term across provinces, but allow the linear term in the polynomial to vary across provinces.

With regard to the control variables, the unemployment rate effect estimates imply a countercyclical relationship with union density rates, which is consistent with evidence elsewhere (Freeman and Pelletier 1990) and the idea that the demand for unionization and the job protection unions provide increases in recessions. All the point estimates also suggest that union density rates are increasing in inflation, consistent with the demand for unionization and COLA clauses rising with inflation, although this effect is estimated much less precisely. As for the manufacturing share of employment, all the estimates are positive and in six of the eight cases not statistically different from zero at the 5% level. However, to some extent deindustrialization trends have been common across provinces, in which case their influence on unionization will be captured by the year fixed effects. Finally, and most surprisingly, we find no evidence that popular perceptions of unions captured in opinion poll data have a direct impact on unionization rates; all the estimates are insignificant at the 5% level. One interpretation is that public opinion impacts unionization rates both directly, through demand for unionization, but also indirectly through the political process and in turn the legal environment that elected governments impose.

Given the similarity of the estimated long-run effects in Table 5, we subsequently restrict our attention to the estimator with the lowest variance – the FGLS estimator allowing for province-specific heteroskedasticity and autocorrelation, as well as contemporaneous spatial correlation. In addition, we restrict the interaction effect θ to be zero. The results from this case are reported in column (4a) of Table 5. The first column of Table 6 reports these results again to enable comparison with the results using the same estimator and specification, but with the CALURA-LFS union density rates (see fifth column of Table 6). The additional specifications in Table 6 add province-specific trends (2); or sample weights (3); or both (4).

The estimated long-run effects of legal reform are remarkably similar using the CALURA-LFS data based on union membership. In three of the four cases the CALURA-LFS point estimates are slightly larger, but the differences are never statistically distinguishable. What is more different is the adjustment process. The coefficient on the LDV in the CALURA-LFS is substantially larger in all cases. The structural interpretation of this result, based on equation [2], is that transition rates in and out of union coverage exceed the transitions in and out of union membership; as one would expect. However, it is likely also the case that the difference reflects greater measurement (sampling) error in the HS-LFS data. The greater noise in the union density rates estimated using survey data is evident in Figure 7. Given that this measurement error is random, we know it will serve to attenuate the estimated LDV effect, which in turn will bias (or "smear") all the estimates in the model. Fortunately, the similarity of the long-run effects provides us with some assurance that the bias using the HS-LFS is modest, and if anything tends underestimate the true effects.

Including province-specific trends and sample weights produces larger differences, particularly using the HS-LFS data. In both cases, the estimates of the long-run legal reform effect are diminished, although including province-specific trends seems to matter more than sampling weights; the long-run estimate declines from 7.6 percentage points to 4.5 in the former case, but to 6.6 percentage points in the latter case. The difference appears to primarily reflect a decrease in the coefficient on the LDV, which is now less than 0.49 suggesting that the sum of the union-to-nonunion and nonunion-to-union annual transition rates is about one-half, which is clearly implausibly large. A possible explanation is that including province trends means that more of the remaining variation in the data to be explained is noise, which once again attenuates the estimated coefficient on the LDV. When we include the province trends and the sampling weights in specification (4), the long-run estimate is 3.1 percentage points; less than half the magnitude of the original estimate, but still statistically different from zero.

4.1 Results; cutting the sample into 12 groups

Our new specification, with $\theta = 0$, becomes:

$$U_{pt} = \alpha U_{p,t-1} + \delta R_{pt} + x'_{pt}\beta + c_p + y_t + \varepsilon_{pt}. \qquad [5]$$

We estimated [5] separately for 12 groups defined by educational attainment (high school completion or less, completion of a postsecondary certificate or diploma, and completion of a university degree), gender and whether they work in the private or public/parapublic sector.[34]

Equating U_{pt} and $U_{p,t-1}$, these estimates imply an expected steady-state union density rate, which depends on all the parameters of the model. From this, we can describe a long-run policy effect on union density associated with a change in the labour relations environment. Using the union density rates estimated for different subgroups of the labour force, we obtained evidence of the differential effects of legal changes as an indication of the potential for labour laws to reduce wage inequality.

Table 7 and Table 8 present our results of the effect of labour relations reform on men and women, respectively, by educational attainment and by sector of employment. For these estimations we use the preferred specification from Table 5 (column 4(a)) and do not include provincial trends or sampling weights. We found in Table 5 and Table 6 that this specification produced the greatest long-run effect. These results, therefore, should be thought of as upper bound estimates; although of primary interest are the relative magnitudes of the estimates across groups in the labour force. Before considering the effects of legislation, we consider the coefficients on other covariates.

For men, the results in the first row clearly demonstrate that current union density rates are dependent on their prior values (see Table 7). For example, for men in the private sector with high school completion or less, a 1 percentage point increase in a province's union density rate at a particular time is associated with a 0.63 percentage point increase in the province's union density rate in the following period. This persistence in union density over time is similar across education groups for both men and women (Table 8, first row), although it is smaller for those with a university degree working in the private sector.

Union density appears to be positively correlated with the unemployment rate, but the relationship is not always statistically significant. The relationship with the inflation rate is less clear. Among men with high school or less education, there appears to be a statistically significant and positive relationship between union density and the share of the province's employment in manufacturing in both the private and public/parapublic sectors (Table 7, columns 1 and 2). For women, this relationship is significant only for those in the private sector (Table 8, column 1). We find very little evidence that population perceptions of unions captured in opinion poll data have any influence on union density rates for women; in only one of the six cases is the coefficient significantly different from zero at the 5% level. For men this variable is more important; in three of the six cases it is negative and significant at the 1% level, reflecting an inverse relationship between public opinion of unions and union density rates. It may

[34] See Section 4 below for results using alternative estimators.

be that the public opinion variable is itself partially determined by unionization rates, in the sense that more union-friendly laws that lead to a greater union presence and power result in a more negative view of unions among the general public.

Our results show that changes in labour relations legislation have significant effects on union density among men and women in most education groups and in both the private and public/parapublic sectors. For example, the results in the last column of Table 7 suggest that a 1-unit increase (from 0 to 1) in the labour relations index is associated with a 5 percentage point increase in the union density rate of men with a university degree employed in the public/parapublic sector. In the long run, the estimates imply that increasing the labour relations index from the current national average to a value of 1 (fully supportive of unions) would increase union density among university-educated men employed in the public/parapublic sector by almost 6.7 percentage points (Table 7, column 6, last row).

The effects of legislative changes vary, however, across groups. The effects do not appear to be statistically significant for men with high school completion or less or for women with a college or trade diploma. They are largest for men in the public/parapublic sector with a college or trades diploma, suggesting that moving to a fully supportive labour relations environment would increase union density among this group of men by 15.8 percentage points (Table 7, column 4, last row).

Why are such effects larger in some sectors than others? One possible explanation is that legal changes would primarily affect workplaces where the difference between the benefits of unionization, in terms of improved wages and working conditions, and the costs, such as the salary costs of union organizers, is small and even close to zero. The logic is that, where the difference between the benefits and costs of unionization is large, workers are already unionized in workplaces where benefits exceed costs and nonunionized in workplaces where costs exceed benefits. Thus, small changes in the costs of unionization that result from legislative reform are unlikely to alter the decision about whether or not to be unionized. It is where the net benefits of unionization become positive as a result of legal reforms that changes in union status will occur. In the nonunionized private sector, where the risks associated with efforts to unionize a workplace can be quite large, a small reduction in the costs of unionization through legal changes will not be enough to seriously alter union density. In the public sector, however, where profit incentives are weaker, small changes in the costs of union organizing brought about by legislative reforms are more likely to be sufficient to alter the decision to initiate a union drive.

The extent to which a change in policy might change union density in each province, relative to density rates in 2013, is presented in Figure 8 and Figure 9.[35] Here, the long-run effect of a switch to legislation that is fully supportive of unions takes into account that legislation in some provinces is already more supportive of unions than in others. For example, Alberta had a labour relations index value of 0.083 in 2012 (see Figure 3). According to our estimates, if the value of the index were increased to 1, to be fully supportive of unions, union density among men in Alberta would increase by 6 percentage points (Figure 8). In contrast, in Manitoba, which had a labour relations index of 0.83 in 2012, increasing the index value to 1 would increase union density among men by only 1 percentage point. Nationwide, increasing the labour relations index to 1 would increase union density among men by 4 percentage

[35] We used the reweighing methods described in Section 7 (Appendix A) to derive the counterfactual union density rates that would exist if legislation were made fully supportive of unions, accounting for differential effects across education, gender and sector.

points. The results for women are quite similar (Figure 9): increasing the labour relations index to 1 would increase union density in Alberta and Nova Scotia by 6 percentage points and nationwide, as for men, by 4 percentage points.

Overall, the results imply that changes in labour relations legislation would not affect all workers equally. Those most likely to become unionized as a result of legislative changes are men with post-secondary certificates or diplomas working in the public/parapublic sector, while those least likely to become unionized are men with a high school diploma or less working in the private sector.

4.2 Robustness Check: Disaggregated worker types

The results discussed above are based on twelve broadly-defined groups of workers; six for men, and six for women. These six groups for each gender arise from all possible permutations of our industry (2 groups) and highest education (3 groups) defined in Section 3 above. The survey data, however, allow us to cut the data into more finely-specified groups of workers, which reduces the heterogeneity within each group. In this section, therefore, we redefine our worker types in a couple of ways. First, we further divide the private sector into three sub-groups: primary industry, manufacturing, and private services. Combined with the public sector, this now gives us a total of four industry groups. Second, we introduce an occupation dimension to our analysis. Specifically, using the occupation variable from each survey we classify each of our workers as one of: blue collar, white collar, or administrative. With these finer cuts of our sample, we can construct 72 permutations (or 72 cells) of worker types (4 industries x 3 occupations x 3 education groups x 2 genders).

Richer insight into the types of workplaces where legal reforms are expected to be most influential could be obtained by estimating the effects within the 72 industry-occupation-education-gender cells. For example, the long-run effect of legal reforms could be estimated separately for university-educated women employed in professional (white collar) public-sector jobs. Unfortunately, in the vast majority of cases the sample sizes in the survey data are too small to estimate provincial union density rates at this level of detail with sufficient precision.[36] Alternatively, in Table 9 we report the results from the largest 10 of these 72 cells, in terms of the total provincial sample sizes provided in the HS-LFS data.

The point estimates point to the largest long-run gains in unionization among unskilled (high-school and blue-collar) women and men employed in private services and manufacturing, respectively (columns 3 and 4). However, neither estimate is statistically distinguishable from the long-run effect for university-educated men or women employed as professionals in public services (columns 6 and 10). Moreover, both estimates are almost identical in magnitude to that of college-educated women employed as professionals in public services (column 5). The results also continue to suggest small gains among other unskilled groups, such as high-school educated men employed in private services in either blue-collar (column 1) or administrative (column (9)) jobs, as well as high-school educated women employed as administrators in private services (column 2). Given the rising importance of private services in overall

[36] Specifically, the most common worker type in our microdata across all years is male, blue-collar, high-school educated, working in the private service sector. The third-most common is the same as the last worker type, except working in manufacturing. On the other end of the spectrum, the least common worker type in our sample is male, university-educated, doing a clerical/administrative job in the primary sector.

employment, these results suggest a limited potential for reforms in labour relations laws to mitigate rising inequality trends.

5 Implications for the Wage Distribution

The results of our analysis in Section 4.1 suggest that making labour relations legislation more supportive of unions would have a positive and fairly substantial effect on union density, but that the effect would be larger for some groups in the population than for others. What would be the implications for the distribution of wages?

To answer this question, we first looked at the wage distribution and union density that prevailed in 2013. We then constructed a counterfactual wage distribution that might exist if legislation were made fully supportive of unions in each province. With higher union density, we expect wages to be slightly higher given the wage premium generally associated with unionization. However, we do not expect that legal changes would raise all groups' union density rates equally — the methods we used, which are described in Section 7 (Appendix A), allowed us to construct a counterfactual scenario in which we raise the 2013 union density rates more for those most affected by changes in labour relations legislation and less for those least affected by such changes. The extent to which we raise union density rates is based on the results presented in Table 7 and Table 8 (based on data from the 1981-2012 period) and the extent to which each province's legislation is already supportive of unions.

The share of the population that becomes unionized enjoys the wage gains associated with being unionized in a particular group as defined by education, gender and sector of employment. Note that due to the greater precision of the union density rates, for this counterfactual exercise we use the 12 groups of worker types from Section 4.1 above, and not the 72 groups from Section 4.2. The resulting counterfactual wage distribution then reflects what the wage distribution would look like if labour legislation in each province were made fully supportive of unions and if union density rates increased as expected in each demographic group. We emphasize that our analytical framework is not able to account for spillover effects such as the potential positive effect of increasing union density on the wages of nonunionized workers.

In what follows, we estimate the density of the distribution of both log hourly wages and log weekly wages of men and women in the private and public/parapublic sectors.[37] The reason for looking at the distributions of both hourly and weekly wages is that, in unionized work environments, wages, work schedules and fringe benefits are negotiated, and we expect unionization to result in more stable work schedules, particularly for workers with less than full-time hours. This could imply a greater number of regular hours and higher earnings for those with relatively low wages. Furthermore, many fringe benefits, such as life insurance, pensions and sick leave, are more prevalent in unionized environments, and represent fixed costs of hiring an employee. Employers of unionized workers thus have an incentive to increase the hours of existing employees (including overtime), rather than increasing the number of employees when there is an increase in labour demand. Overall, then, unionization should result in higher earnings due to both higher wages and more work hours.

[37] We estimated weekly wages by multiplying the hourly earnings reported in the Labour Force Survey by the actual total hours reported for the reference week.

5.1 Results

We provide our density estimates and statistics describing the distribution of log hourly wages for men and women in 2013 and under our counterfactual scenario in Table 10 and Figure 10. In Table 10, we also report separately the results for the private and public/parapublic sectors. For reference, we present the 2013 mean log hourly wages of unionized and nonunionized workers in each of the demographic groups shown in Table 11. We should note that the difference in log wages between groups is a good approximation of the percentage difference in wages between groups.

Consider, first, the observed 2013 distribution of log hourly wages of men in the private sector (Table 10, panel a). In 2013, 10 percent of men in the private sector earned log hourly wages at or below 2.398 ($11 per hour), just slightly more than every provincial minimum wage.[38] This helps to explain the large mass of workers observed around this wage rate in the 2013 wage density distribution presented in Figure 10. The median log wage of men in the private sector was 3.069 ($22 per hour), and 10 percent of men in the private sector had log wages of 3.732 ($42 per hour) or more, represented by the 90th percentile.

The counterfactual distribution — that is, the distribution that would exist if labour relations legislation were fully supportive of unions — of log hourly wages of men in the private sector is shown in the second column of Table 10 (panel a). Here, higher union density results in a modest increase in the median hourly wage, reflecting the small wage premium that unionized men in the private sector with a college or trade diploma would enjoy — the estimates we show in Table 11 (panel a) indicate that these men would earn wages 15 log points higher (3.259 – 3.113) than those of their nonunionized counterparts.

This wage premium from unionization for college-educated workers is modest, however, compared with the 22 log point premium men with high school education or less would be expected to receive. Yet our results in Table 10 show that wages at the lower part of the distribution for men in the private sector would be largely unaffected by unionization, with the 10th percentile unchanged. This is consistent with our estimates in Table 7 that indicate that legislative changes would have no significant effects on union density among men with high school education or less working in the private sector. Interestingly, wages at the 90th percentile would decline even though union-friendly legislation would increase union density among men in the private sector with a university degree. A closer look at the 2013 wage data tells us why. In 2013, the average log wage of unionized men in this sector with a university degree was actually 7.4 log points lower than that of nonunionized men (see Table 11). As a result, inequality could be reduced in the private sector since wage compression at the top end of the distribution would reduce the 90-10 log wage differential and result in a lower standard deviation (Table 10). However, the differential effects of union-friendly legislation also imply that wage disparities between lower- and middle-wage workers would increase, as reflected in the higher 50-10 and 75-25 differential in this group's counterfactual wage distribution.

In Table 10 (panel b), the first two columns describe the distribution of hourly wages for 2013 and our counterfactual among men in the public/parapublic sector. The 2013 data in Table 10 and Table 11 reveal that wages are generally higher in this sector than in the private sector and are slightly less

[38] For the minimum wage in each province, see Canada (2015).

dispersed, particularly in the upper half of the wage distribution. Considering the counterfactual distribution, the greatest effect of legislative changes would be on the 10th percentile of men's wages in the public/parapublic sector. The wage compression that would result from greater unionization would also reduce measures of inequality — in particular, the 90-10 log wage differential for men in the public/parapublic sector would be 5.4 percent (or 6.5 log points) lower than that observed in 2013.

Looking at the results for both sectors of employment and all education groups combined, we see that union-friendly legislative changes would reduce wage inequality among men (Table 10, panel c). This is largely because increased union density would raise the wages of the lowest-paid men in the public/parapublic sector and compress the wages of men in the private sector near the very top of the wage distribution. Making legislation fully supportive of unions would reduce the 90-10 log wage differential and the 75-25 log differential by about 2 percent (or by 2.2 and 1.4 log points, respectively), which would be a fairly substantial reduction in inequality considering that the 90-10 log wage differential for men increased by 6.2 percent over the 1984-2012 period.[39]

It is worth emphasizing the importance of accounting for the heterogeneous effects of legislative changes across sectors and education groups. To illustrate this, we also estimated a counterfactual wage distribution for men if union density simply increased by the average effect of legislation in Canada — namely, by 4 percentage points, thus disregarding heterogeneous effects. We then found that the 75-25 log differential would be reduced by 3.2 percent,[40] compared with our estimate of a 1.8 percent (1.4 log points) reduction when we account for heterogeneous effects (Table 10, panel c). As such, although union-friendly legislative changes could reduce wage inequality among men, other mechanisms that increased union density more broadly would be required to reduce wage inequality further.

The results for the wage distribution of women are quite different from those of men. For women in the private sector (Table 10, panel a, column 3), wages tend to be lower than those of men. Perhaps surprisingly, our counterfactual wage distribution (Table 10, panel a, column 4) suggests that higher union density resulting from changes to labour legislation would have only minor effects on the distribution of women's wages. Union density among women in the private sector with a university degree might rise by 4 percentage points, but, similar to men in the private sector, such women would have little to gain from unionization in terms of wages — the average log wage of unionized women in the private sector with a university degree is 1 percent more than that of nonunionized women (or 3 log points, see Table 11, panel a). Although there would also be a modest increase in union density among less-educated women in the private sector, as well as a modest wage premium (16 log points for those with high school education or less), very few unionized women are found in the lowest part of the wage distribution (recall Figure 1). There would be some changes in the middle of the wage distribution for women, as the 75-25 log differential would be reduced, reflecting an increase in the 25th percentile of wages but no change in the 75th percentile (Table 10, panel a). Overall, any increase in union density

[39] Authors' tabulations based on the Survey of Union Membership, the Labour Force Survey and the same sample as represented in Table 1.

[40] Note that this larger increase aligns well with estimates presented in Card, Lemieux and Riddell (2004). They consider increasing union density rates among men from 0 to 33 percent, which results in a 7 to 9 percent reduction in the variance of wages. Using our methods, a broad increase in union density by 33 percentage points, disregarding heterogeneous effects, would reduce the standard deviation of men's wages by 8 percent.

among women that might result from changes to labour relations legislation would not be enough to alter the wage distribution of women in the private sector.

Little change would also be expected in their wage distribution as a result of legislative changes for women in the public/parapublic sector. Such changes as did occur likely would have the largest effect on the median wage (Table 10, panel b) and the 75th percentile.[41] As a result, the increase in unionization might help to close the gap between highest- and middle-wage women in this sector, but might increase the gap between middle- and lowest-wage women. Overall, the standard deviation of log wages is slightly smaller when union density rates are higher as a result of legislative changes.

For women, then, changes to legislation that increased union density rates would not alter the wage distribution substantially (Table 10, panel c). Over the period from 1984 to 2012, the 90-10 log differential in women's wages increased by 9 percent, but our estimates in Table 10 suggest that legislative changes might reduce the 90-10 log differential by less than 0.1 percent (or less than 0.05 log points).

In Table 12, we consider the effects of higher union density on the distribution of log hourly wages of all individuals. The compression of wages that would occur among men would close the gap between the middle of the wage distribution and the top earners, as indicated by a substantial 2 percent (or 2.1 log points) reduction in the 90-50 log wage differential. The 75-25 log differential would be similarly reduced. At the same time, however, the gap between the lowest-wage and middle-wage workers would increase, as indicated by the increase in the 50-10 log wage differential. Why would the gap between the lowest-wage and middle-wage workers increase? Despite raising the wages of the lowest-wage men in the public/parapublic sector, an increase in union density would raise the wages of men more than the wages of women (see Table 10, panel c), and it is women who are more likely to have the lowest wages. The increase in the 50-10 log wage differential is due to the increase in the gap between men's and women's wages that is predicted to result from changes to labour relations legislation.

Thus far, we have considered only how increased unionization would affect wage rates. However, we expect unionization also to affect individuals' work hours. In columns 3 and 4 of Table 12 we account for this by considering the effects of higher union density rates on the distribution of log weekly wages — the product of hourly wages and hours worked. The increase in union density would raise weekly earnings in the middle of the distribution the most, largely reflecting the effects on men's wages discussed above. However, increased unionization would also result in a modest increase in the 10th percentile of log weekly wages of both men and women and in both the private and public/parapublic sectors. Overall, increased unionization would reduce the gap between the richest and poorest workers' weekly wages more than it would reduce the gap for hourly wages, as represented by the reduction in the 90-10 log differential for weekly wages.

In short, the evidence suggests that changes that made provincial labour relations legislation more supportive of unionization would have only a modest effect on reducing wage inequality. As illustrated in Figure 10, any changes to the overall distribution of wages would not be striking. Within certain groups, however, the benefits of unionization would be more noticeable, in particular for middle-wage men in the

[41] The 2013 log hourly wage for women in the public/parapublic sector at the 75th percentile was 3.544; the counterfactual's 75th percentile was 3.553.

private sector and lower-wage men in the public/parapublic sector. Broader benefits for lower-wage individuals might come through union negotiation of work schedules.

6 Conclusion

In this chapter, we constructed a historical dataset of provincial union density rates and labour relations legislation, and we used a dynamic generalized least-squares estimator to estimate the effect of changes in labour relations legislation on union density over the period from 1981 to 2012. The results are significant and substantial: the introduction of a fully supportive labour relations regime could increase union density by as much as 6 percentage points in some provinces for both women and men in the long run. For women, such an increase would represent a return to the level of unionization that prevailed in the early 1980s. For men, a 6 percentage point change in union density is equal to a third of the decline in union density that occurred between 1981 and 2012.

Should we rely on changes to labour relations legislation to reduce income inequality? Previous studies have shown that the decline in unionization in the 1980s and 1990s explains a sizable portion of the increases in wage inequality that occurred during that period. Card, Lemieux and Riddell (2004) show that unionization tends to reduce wage inequality among men and has no effect on wage inequality among women. Our results are similar: higher union density resulting from union-friendly legislative changes is expected to reduce wage inequality among men, but to have only a modest effect on wage inequality among women. For men and women combined, the effect would still be modest. Moreover, higher union density rates likely would increase the gap between the lowest-wage and middle-wage workers, mainly by increasing the wage gap between men and women.

In light of these results, we conclude that reform to labour relations legislation should not be pursued in isolation from other policy levers in an attempt to alter income inequality. Fortin and Lemieux (forthcoming) have found that increases in the minimum wage since 2005 are the main reason why wages at the very bottom of the wage distribution have increased faster than wages in the rest of the distribution. However, this effect is concentrated among teenage workers, and the impact of the minimum wage is smaller when teenage workers are excluded from the sample. We think this suggests minimum wage policy may be less effective in reducing income inequality across households than it is in reducing wage inequality across all workers. Frenette, Green and Milligan (2009) have shown that the tax-and-transfer system can directly affect the incomes of lower-wage workers. Heisz and Murphy (forthcoming) also demonstrate the importance of taxes and government transfers (in terms of their size and progressivity) for redistribution. They find that since 1976, changes in average benefit rates have been the main factor affecting redistribution trends. Indeed, the progressivity of transfers has been quite stable over time, while the potential negative impact on inequality of income tax rate reductions since the early 2000s has been offset by increases in the progressivity of tax rates. It is our sense, therefore, that the tax-and-transfer system would be a much more effective avenue for tackling overall income inequality than changes in labour relations legislation.

7 Methodology for Constructing the Counterfactual Wage Distribution (Appendix A)

The procedure for constructing a counterfactual wage distribution follows from the decomposition procedures presented in Dinardo, Fortin and Lemieux (1996).[42] Each individual observation can be viewed as a vector (*w*, *U*, *E*, *G*, *S*, *P*) made up of the individual's wages (w) and a set of individual attributes, including union status (*U*), education level (*E*), gender (*G*), sector (*S*) and province of residence (*P*). Each individual observation belongs to a joint distribution *F*(*w*, *U*, *E*, *G*, *S*, *P*), and might depend on characteristics such as the labour relations legislation in place in the province (*R*). The density of wages at time *t*, $f_t(w)$, can be written as the integral of the density of wages conditional on the set of individual attributes, given the labour relations legislation in place in the province:

$$f_t(w) = \int f_t(w|U,E,G,P;R)\, dF(U|E,G,S,P;R_t) \qquad [6]$$

The counterfactual density of wages that might exist if labour relations legislation were made fully supportive of unions can be written as

$$f_c(w) = \int f_t(w|U,E,G,P;R)\, dF(U|E,G,S,P;R_c)\,, \qquad [7]$$

which can be obtained by multiplying the observed density at time *t* (equation [6]) by the function

$$\psi_U = \frac{dF(U|E,G,S,P;R_c)}{dF(U|E,G,S,P;R_t)} \qquad [8]$$

As union status takes on values of either 1 or 0, we can restate this function as

$$\psi_U = U\ \frac{Pr(U=1|E,G,S,P;R_c)}{Pr(U=1|E,G,S,P;R_t)} + (1-U)\ \frac{Pr(U=0|E,G,S,P;R_c)}{Pr(U=0|E,G,S,P;R_t)} \qquad [9]$$

We estimated the probabilities represented by the denominator in equation [9] based on observed cell-specific union density rates (for example, university-educated females in the private sector in Ontario) in 2013. The probabilities represented by the numerator are the cell-specific union density rates that would exist in each province if labour relations legislation were made fully supportive of unions. To obtain the latter, we estimated the effect of changing labour relations legislation using a feasible generalized least-squares estimator within each of the 12 education, gender and sector groups presented in Table 7 and Table 8. From this, for each province, we estimated the extent to which union density rates in each education and gender group would increase in the long run if the province took the legislative regime that existed in 2012 and made it fully

[42] Notation in this section closely follows that in Fortin and Schirle (2006).

supportive of unions (an index value R of 1). The result is added to the prevailing union density rate represented by the denominator in equation [9].

We then multiplied the function represented by equation [9] by the survey weights of each observation in the 2013 Labour Force Survey data to create a revised weight. When estimating the prevailing 2013 wage density and the statistics describing the distribution, we used the original survey weights provided by Statistics Canada. When estimating the counterfactual density and associated statistics, we used the revised weights. In practice, this procedure will increase the sample weights for unionized individuals, resulting in the union density rates we would expect under a new, fully supportive labour relations regime.

8 Tables and Figures

Table 1. Distribution of Men's and Women's log hourly wages, 1984 and 2012

	(a) Women			
	1984		2012	
	Union	Non-union	Union	Non-union
90-10	0.981	1.099	1.087	1.234
90-50	0.470	0.693	0.542	0.764
50-10	0.511	0.405	0.545	0.470
75-25	0.486	0.693	0.588	0.723
Std. Dev.	0.385	0.462	0.418	0.475
	(b) Men			
	1984		2012	
	Union	Non-union	Union	Non-union
90-10	0.811	1.447	1.089	1.416
90-50	0.325	0.754	0.48	0.772
50-10	0.486	0.693	0.610	0.644
75-25	0.405	0.875	0.570	0.767
Std. Dev.	0.361	0.555	0.421	0.524

Source: Authors' tabulations based on Statistics Canada, Survey of Union Membership, 1984, and Labour Force Survey, 2012. "Unionized" refers to all employees covered by a collective agreement, not just union members.

Table 2. Provincial union density rates, 1981 and 2012

		NL	PE	NS	NB	QC	ON	MB	SK	AB	BC
All Workers	1981	0.45	0.40	0.36	0.41	0.49	0.35	0.40	0.40	0.32	0.44
	2012	0.38	0.30	0.29	0.28	0.39	0.27	0.35	0.35	0.23	0.30
Industry											
primary	1981	0.51	0.06	0.35	0.37	0.48	0.31	0.34	0.31	0.16	0.60
	2012	0.38	0.06	0.19	0.21	0.23	0.17	0.20	0.27	0.11	0.29
manufacturing	1981	0.69	0.39	0.46	0.43	0.57	0.47	0.45	0.42	0.40	0.63
	2012	0.43	0.26	0.17	0.24	0.36	0.21	0.31	0.25	0.17	0.25
private services	1981	0.25	0.25	0.22	0.28	0.38	0.22	0.27	0.27	0.23	0.30
	2012	0.19	0.10	0.12	0.10	0.26	0.14	0.18	0.18	0.12	0.18
public services[a]	1981	0.73	0.82	0.72	0.78	0.89	0.67	0.77	0.79	0.73	0.78
	2012	0.67	0.69	0.64	0.62	0.70	0.59	0.68	0.68	0.56	0.63
Occupation											
blue collar	1981	0.50	0.35	0.41	0.44	0.60	0.46	0.45	0.42	0.38	0.58
	2012	0.37	0.23	0.26	0.25	0.44	0.30	0.33	0.31	0.20	0.31
administrative	1981	0.26	0.28	0.25	0.35	0.40	0.26	0.33	0.32	0.26	0.29
	2012	0.25	0.20	0.17	0.17	0.26	0.15	0.23	0.24	0.16	0.20
professionals	1981	0.62	0.73	0.58	0.57	0.64	0.41	0.53	0.63	0.44	0.51
	2012	0.47	0.46	0.41	0.41	0.44	0.31	0.46	0.48	0.31	0.38
Education											
high school or less	1981	0.46	0.35	0.36	0.4	0.53	0.38	0.4	0.4	0.32	0.46
	2012	0.25	0.17	0.18	0.18	0.33	0.22	0.27	0.26	0.17	0.23
post-secondary degree	1981	0.46	0.6	0.5	0.56	0.59	0.44	0.52	0.59	0.46	0.55
	2012	0.43	0.36	0.34	0.31	0.43	0.3	0.39	0.4	0.25	0.36
university degree	1981	0.63	0.79	0.58	0.61	0.68	0.41	0.61	0.58	0.42	0.52
	2012	0.48	0.46	0.37	0.43	0.41	0.28	0.45	0.45	0.31	0.34
Gender											
male	1981	0.51	0.40	0.43	0.46	0.59	0.45	0.47	0.46	0.38	0.55
	2012	0.37	0.24	0.25	0.26	0.40	0.26	0.32	0.29	0.20	0.28
female	1981	0.43	0.46	0.37	0.43	0.50	0.32	0.39	0.42	0.34	0.38
	2012	0.38	0.36	0.32	0.30	0.38	0.27	0.38	0.40	0.26	0.32

Notes: Union density rates are from the HS-LFS series and therefore exclude federal government employees. All other relevant sample restrictions are described in Table 13. The definition of unionization includes those who are covered by a collective agreement, but who are not a member of the union. Sources: SWH (1981), LFS(2012).

[a] Public services is broadly defined including provincial and municipal government employees, education and related services, health and welfare services and utilities.

Table 3. Union density rates regressed on linear and quadratic time trends

	Union density rates:			
	Provincial-level		Province-industry-occupation-education-gender-level	
Independent variables	(1)	(2)	(1)	(2)
Time	-0.0037***	-0.0065***	-0.0031***	-0.0056***
	(0.0003)	(0.0006)	(0.0003)	(0.0005)
time squared		0.0001***		0.0001***
		(0.0000)		(0.0000)
Constant	0.4011***	0.4150***	0.3924***	0.4052***
	(0.0220)	(0.0236)	(0.0188)	(0.0186)
Observations	320	320	23040	23040
R^2	0.284	0.296	0.014	0.014

Note: All linear regressions are weighted by sample sizes of underlying survey data. Standard errors are clustered; (1) and (2) at province level, (3) and (4) at unit level. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. Timing of Laws

Law	NL	PE	NS	NB	QC	ON	MB	SK	AB	BC	Index
First Contract Arbitration[i]	85:06		11:12[g]		77:12	86:05	82:02	94:10		73:11	=1
Anti-Temporary Replacement Laws					78:02	93:01-95:11				93:01	=1
Ban on Permanent Replacements		87:05					85:01				=1
Re-instatement Rights		87:05			78:02	70:11-92:12	85:01	94:10	88:11		=1
Ban on Strike-breakers						83:06	85:01			73:11	=1
Mandatory Dues Check-off	85:07				78:04	80:07	72:11	72:05		77:09	=1
Mandatory Strike Vote		67	67	72:04	78:04	95:11	85:01	67	67	67	=0
Employer-Initiated Strike Vote			94:05		02:11	80:07	97:02-00:10	83:07	88:12	87:08	=0
Compulsory Conciliation	67	67	67	67	67-78:01	67:86:12			68:01-81:02, 88:12		=0
Cool off period[h]	67	67	67	67	77:12	67		83:07	67-88:11	67	=0
Technology Re-opener				89:04			72:11			74:03	=1
Secret Ballot Certification Vote[a]	94:02-12:06[e]		77:05			95:11[f]	97:02-00:09[c]	08:05[d]	88:11	84:06-93:01, 01:08[b]	=0

Notes: All dates are from Johnson (2010) unless otherwise noted by a reference. Date specifies when law comes into effect (may be different from royal assent date).
a: Dates are from Johnson (2002) unless otherwise noted by a reference in this row. Changes between 1967 and 1975, inclusive, not provided.
b: Highlights of Major Developments in Labour Legislation, HRSDC (2001)
c: Highlights of Major Developments in Labour Legislation, HRSDC (2000)
d: Bill 6: An Act to amend The Trade Union Act, Chapter 26; Royal Assent: May 14, 2008.
e: Bill 37: An Act to amend The Labour Relations Act, Chapter 30; Royal Assent: June 27, 2012.
f: Bill 144: An Act to amend certain statutes relating to Labour Relations; Royal Assent June 13, 2005. Remove mandatory vote below 55% support for construction workers only. Note: we do not exclude construction workers in HS-LFS series.
g: Bill 102: An Act to Prevent Unnecessary Labour Disruptions and Protect the Economy by Amending Chapter 475 of the Revised Statutes, 1989, the Trade Union Act, Chapter 71; Royal Assent: December 15, 2011.
h: We do not specify the number of days of cool-off period in this table – see Johnson (2010) for more detail.
i: Update since Johnson (2002). PEI did not implement first contract arbitration in 95:05; never received Royal Assent.

Table 5. Estimates of the effect of provincial labour relations index on union density rates

	Dependent variable: HS-LFS union density rates							
Independent var.	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
lagged density rate	0.6422***	0.6593***	0.6873***	0.7101***	0.7057***	0.7297***	0.6735***	0.7055***
	(0.0450)	(0.0514)	(0.0407)	(0.0469)	(0.0408)	(0.0436)	(0.0383)	(0.0395)
labour relations index	0.0427***	0.0636*	0.0301***	0.0568**	0.0308***	0.0565***	0.0422***	0.0815***
	(0.0124)	(0.0326)	(0.0101)	(0.0287)	(0.0085)	(0.0215)	(0.0060)	(0.0198)
interaction term		-0.0610		-0.0764		-0.0743		-0.1164**
		(0.0883)		(0.0769)		(0.0569)		(0.0559)
unemployment rate	0.1709**	0.1752**	0.1563**	0.1632**	0.1036*	0.1102*	0.0499	0.0443
	(0.0742)	(0.0745)	(0.0629)	(0.0634)	(0.0574)	(0.0573)	(0.0526)	(0.0525)
inflation rate	0.1355	0.1527	0.0472	0.0628	0.0260	0.0347	0.0382	0.0425
	(0.1281)	(0.1306)	(0.1078)	(0.1100)	(0.0373)	(0.0388)	(0.0792)	(0.0801)
manufacturing share	0.0975	0.1032*	0.0934*	0.1035**	0.0753	0.0781	0.0752*	0.0797**
	(0.0615)	(0.0621)	(0.0501)	(0.0508)	(0.0491)	(0.0487)	(0.0390)	(0.0385)
tastes	-0.0368	-0.0356	-0.0312*	-0.0276	-0.0166	-0.0120	-0.0218	-0.0192
	(0.0242)	(0.0243)	(0.0188)	(0.0191)	(0.0172)	(0.0178)	(0.0226)	(0.0227)
constant	0.1307***	0.1232***	0.1193***	0.1072***	0.1096***	0.0982***	0.1271***	0.1171***
	(0.0274)	(0.0294)	(0.0253)	(0.0284)	(0.0266)	(0.0279)	(0.0269)	(0.0271)
Error Terms:								
Var[$\epsilon_{p,t}$]=	σ^2	σ^2	σ_p^2	σ_p^2	σ_p^2	σ_p^2	σ_p^2	σ_p^2
Cov[$\epsilon_{p,t}$, $\epsilon_{q,s}$]=	0	0	0	0	$\sigma_{p,q}$	$\sigma_{p,q}$	$\sigma_{p,q}$	$\sigma_{p,q}$
Cov[$\epsilon_{p,t}$, $\epsilon_{p,t-1}$]=	0	0	0	0	0	0	ρ_p	ρ_p
observations	310	310	310	310	310	310	310	310
R^2	0.969	0.969	-	-	-	-	-	-
long run effect	0.0707	0.0671	0.0571	0.0545	0.0619	0.0591	0.0764	0.0689
	(0.0212)	(0.0193)	(0.0197)	(0.0171)	(0.0176)	(0.0151)	(0.0109)	(0.0103)

Notes: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Year dummies and province dummies are included in all regressions. The variable *tastes* is between (0,1) with 1 being most supportive of unions. The following tests are performed on specification (1): (a) Poolability: Using the Baltagi (2008, p.57) for full poolability (we need to exclude year dummies to do the test), we reject the null of poolability of all parameters. Using the Beck (2001) test for poolability of a single parameter of interest, we fail to reject the null of poolability of the legal index parameter. (b) Heteroskedasticity: Using the Wald Test proposed in Greene (2003, p.323) we reject the null of no groupwise (panel) heteroskedasticity. (c) Serial Correlation: Using the Lagrange multiplier test for serial correlation in time-series-cross-section data as described in Beck and Katz (1996), we do not reject the null of no serial correlation. (d) Stationarity: Using the Levin, Lin, Chu (2002) test for stationarity of time-series-cross-section data, we reject the null that the panels contain unit roots (cross-sectionally-demeaned stationary). The "long run effect" is the difference between the long run value of $U_{p,t}$ evaluated at R_t=1 and evaluated at R_t=R_{2012} where R_{2012} is the average of all provincial values of R in 2012, weighted by population of the province.

Table 6. Robustness analysis of effect of legislative index on union density rates

	Dependent Variable: union density rates:							
	HS-LFS				CALURA-LFS			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
lagged density rate	0.6735***	0.6963***	0.4917***	0.4552***	0.8459***	0.7900***	0.6210***	0.5719***
	(0.0383)	(0.0350)	(0.0484)	(0.0461)	(0.0233)	(0.0279)	(0.0388)	(0.0412)
labour relations index	0.0422***	0.0339***	0.0389***	0.0288***	0.0220***	0.0198***	0.0366***	0.0342***
	(0.0060)	(0.0066)	(0.0076)	(0.0079)	(0.0046)	(0.0060)	(0.0053)	(0.0071)
unemployment rate	0.0499	0.0510	-0.0348	-0.0470	0.0231	-0.0154	0.0217	0.0578
	(0.0526)	(0.0486)	(0.0601)	(0.0610)	(0.0345)	(0.0376)	(0.0412)	(0.0456)
inflation rate	0.0382	-0.0161	0.0076	-0.0797	0.0116	-0.0018	-0.0497	-0.0189
	(0.0792)	(0.0753)	(0.0825)	(0.0805)	(0.0618)	(0.0472)	(0.0603)	(0.0498)
manufacturing share	0.0752*	0.0892**	-0.1117	-0.0832	0.0907***	0.0569**	-0.0819	0.0453
	(0.0390)	(0.0375)	(0.0780)	(0.0642)	(0.0284)	(0.0264)	(0.0519)	(0.0459)
tastes	-0.0218	-0.0464***	0.0447	0.0154	0.0050	0.0211*	-0.0036	0.0611**
	(0.0226)	(0.0165)	(0.0522)	(0.0457)	(0.0108)	(0.0127)	(0.0190)	(0.0256)
constant	0.1271***	0.1375***	0.2235***	0.2680***	0.0182**	0.0439***	0.1374***	0.0800***
	(0.0269)	(0.0218)	(0.0499)	(0.0445)	(0.0075)	(0.0104)	(0.0234)	(0.0252)
province trends	No	No	Yes	Yes	No	No	Yes	Yes
sample size weights	No	Yes	No	Yes	No	Yes	No	Yes
observations	310	310	310	310	360	360	360	360
long run effect	0.0764	0.0660	0.0453	0.0313	0.0869	0.0572	0.0588	0.0486
	(0.0109)	(0.0128)	(0.0091)	(0.0088)	(0.0185)	(0.0168)	(0.0088)	(0.0102)

Notes: Year dummies and province dummies are included in all regressions. The variable *tastes* is between [0,1] with 1 being most supportive of unions. All specifications use the same form of GLS as columns 7 and 8 in Table 5, Var[$\epsilon_{p,t}$]=σ_p^2, Cov[$\epsilon_{p,t}$, $\epsilon_{q,s}$]=$\sigma_{p,q}$, Cov[$\epsilon_{p,t}$, $\epsilon_{p,t-1}$]=ρ_p. Sample size weights refer to total cell counts of micro data underlying the data. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7. Effect of labour legislation on union density rates among men, by educational attainment and employment sector, Canada

	(1)	(2)	(3)	(4)	(5)	(6)
	High School		College		University	
	Private	Public	Private	Public	Private	Public
Lagged density rate	0.6304***	0.4396***	0.5342***	0.5023***	0.2238***	0.5504***
	(0.0457)	(0.0478)	(0.0447)	(0.0451)	(0.0571)	(0.0373)
Labour relations index	0.0085	0.0314	0.0328*	0.1329***	0.0631***	0.0506**
	(0.0113)	(0.0288)	(0.0176)	(0.0340)	(0.0222)	(0.0249)
Unemployment rate	0.1867**	1.1159***	0.2375	0.4038*	0.2451	0.5522***
	(0.0920)	(0.1867)	(0.1533)	(0.2068)	(0.1579)	(0.1546)
Inflation rate	0.2064	0.8359**	0.0367	0.3106	-0.7620***	0.2290
	(0.1540)	(0.3333)	(0.1943)	(0.3481)	(0.2450)	(0.2793)
Manufacturing share	0.2091***	0.2754*	0.1357	-0.1170	0.1970*	-0.0068
	(0.0702)	(0.1478)	(0.1136)	(0.1659)	(0.1184)	(0.1370)
Public opinion	0.0077	-0.1085	-0.1574***	-0.0654	-0.1716***	-0.0975***
	(0.0262)	(0.0803)	(0.0561)	(0.0724)	(0.0602)	(0.0363)
Constant	0.1113***	0.3079***	0.2413***	0.3443***	0.2199***	0.3336***
	(0.0327)	(0.0628)	(0.0530)	(0.0670)	(0.0472)	(0.0614)
Observations	310	310	310	310	310	310
Long run effect	0.0137	0.0332	0.0417	0.1581	0.0482	0.0666
	(0.0179)	(0.0304)	(0.0220)	(0.0369)	(0.0168)	(0.0327)

Note: Province-fixed and year-fixed effects are included in all regressions. The "long-run effect" is defined as the increase in the steady-state density rate that would result if the weighted average provincial labour relations index (0.41 in 2012) was assigned a value of 1.0 (in other words, if all provinces had a labour relations index value of 1.0). The public opinion measure varies between 0 and 1, with 1 being most supportive of unions (see section 3.4). *$p < .10$ **$p < .05$ ***$p < .01$

Table 8. Effect of labour legislation on union density rates among women, by educational attainment and employment sector, Canada

	(1)	(2)	(3)	(4)	(5)	(6)
	High School		College		University	
	Private	Public	Private	Public	Private	Public
Lagged density rate	0.5422***	0.4961***	0.6143***	0.5461***	0.3842***	0.4071***
	(0.0457)	(0.0501)	(0.0417)	(0.0485)	(0.0492)	(0.0498)
Labour relations index	0.0333***	0.0568**	0.0187	0.0188	0.0459*	0.0541***
	(0.0112)	(0.0284)	(0.0119)	(0.0284)	(0.0238)	(0.0207)
Unemployment rate	0.0396	-0.0132	-0.0581	0.2680	0.2029	0.2671*
	(0.0732)	(0.1502)	(0.1105)	(0.1649)	(0.1521)	(0.1455)
Inflation rate	-0.0336	0.3301	-0.4019**	0.1243	0.3095	0.3394
	(0.1119)	(0.2620)	(0.1747)	(0.2794)	(0.2338)	(0.2320)
Manufacturing share	0.1185**	0.2000	0.0442	-0.0090	0.0398	-0.0933
	(0.0551)	(0.1370)	(0.0768)	(0.1272)	(0.1729)	(0.0907)
Public opinion	-0.0078	-0.1047*	-0.0620	-0.1718**	-0.0053	-0.0700*
	(0.0190)	(0.0567)	(0.0430)	(0.0691)	(0.0388)	(0.0388)
Constant	0.0733***	0.3508***	0.1285***	0.4592***	0.0429	0.4796***
	(0.0204)	(0.0630)	(0.0313)	(0.0670)	(0.0548)	(0.0554)
Observations	310	310	310	310	310	310
Long run effect	0.0430	0.0668	0.0287	0.0245	0.0442	0.0540
	(0.0144)	(0.0328)	(0.0185)	(0.0367)	(0.0229)	(0.0205)

Note: Province-fixed and year-fixed effects are included in all regressions. The "long-run effect" is defined as the increase in the steady-state density rate that would result if the weighted average provincial labour relations index (0.41 in 2012) was assigned a value of 1.0 (in other words, if all provinces had a labour relations index value of 1.0). The public opinion measure varies between 0 and 1, with 1 being most supportive of unions (see section 3.4). *p < .10 **p < .05 ***p < .01

Table 9. Estimates of legislative effect for 10 largest industry-education-occupation-gender cells

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
lag un rate	0.4941***	0.4359***	0.4290***	0.5787***	0.4043***	0.3412***	0.4585***	0.4201***	0.3863***	0.4833***
	(0.0486)	(0.0493)	(0.0528)	(0.0443)	(0.0536)	(0.0524)	(0.0531)	(0.0469)	(0.0502)	(0.0455)
LR index	-0.0004	0.0038**	0.0093*	0.0075***	0.0084**	0.0062**	0.0057*	0.0037*	-0.0008	0.0055*
	(0.0019)	(0.0018)	(0.0051)	(0.0021)	(0.0039)	(0.0025)	(0.0034)	(0.0022)	(0.0031)	(0.0033)
unem rate	0.0268	-0.0002	0.1630	0.2167***	0.4712**	0.2746*	-0.0039	-0.1192	0.0784	0.4960**
	(0.1237)	(0.0973)	(0.2327)	(0.0832)	(0.1830)	(0.1550)	(0.1865)	(0.1301)	(0.1590)	(0.1954)
inflation rate	0.2729	-0.2949**	0.4229	0.2792*	0.0512	-0.0704	-0.0651	0.2361	0.4467**	0.1612
	(0.1973)	(0.1502)	(0.3635)	(0.1582)	(0.2753)	(0.2511)	(0.3051)	(0.2151)	(0.2204)	(0.3273)
manuf share	-0.1657**	-0.1054*	0.3968*	0.0142	0.3488**	-0.1376	-0.9054***	-0.0797	-0.0668	0.0303
	(0.0777)	(0.0610)	(0.2209)	(0.0608)	(0.1457)	(0.0969)	(0.1688)	(0.0860)	(0.1431)	(0.1296)
tastes	0.0313	0.0363*	-0.0197	-0.0786***	-0.2023***	-0.0286	-0.1128	-0.0430	0.0010	-0.1156**
	(0.0365)	(0.0210)	(0.0679)	(0.0251)	(0.0771)	(0.0454)	(0.0802)	(0.0347)	(0.0426)	(0.0484)
constant	0.2562***	0.1241***	0.2869***	0.0770***	0.5151***	0.5425***	0.5779***	0.1640***	0.1939***	0.4104***
	(0.0387)	(0.0270)	(0.0817)	(0.0227)	(0.0733)	(0.0620)	(0.0827)	(0.0357)	(0.0511)	(0.0648)
sector	services	services	manuf	services	public	public	services	services	services	public
education	high school	high school	high school	high school	college	university	college	college	high school	university
occupation	blue	admin	blue	blue	profes	profes	blue	admin	admin	profes
gender	male	female	male	female	female	female	male	female	male	male
observations	310	310	310	310	310	310	310	310	310	310
long run effect	-0.0007	0.0067	0.0164	0.0179	0.0141	0.0094	0.0105	0.0064	-0.0013	0.0107
	(0.0037)	(0.0033)	(0.0088)	(0.0050)	(0.0065)	(0.0039)	(0.0063)	(0.0037)	(0.0051)	(0.0064)

Notes: Year dummies and province dummies are included in all regressions. The variable *tastes* is between (0,1) with 1 being most supportive of unions. The specification used for all 12 regressions above is the same is in Column (4a) of Table 5. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10. Distribution of Log Hourly Wages, Men and Women, by sector.

	(a) Private Sector			
	Men		Women	
	2013	Counterfactual	2013	Counterfactual
10th percentile	2.398	2.398	2.327	2.327
Median	3.069	3.074	2.773	2.773
90th percentile	3.732	3.724	3.496	3.496
Log wage differential				
90-10	1.334	1.327	1.168	1.168
90-50	0.662	0.650	0.723	0.723
50-10	0.672	0.676	0.445	0.445
75-25	0.726	0.732	0.697	0.679
Standard dev.	0.497	0.495	0.459	0.458
	(b) Public and Parapublic Sector			
	Men		Women	
	2013	Counterfactual	2013	Counterfactual
10th percentile	2.708	2.773	2.639	2.639
Median	3.401	3.401	3.178	3.180
90th percentile	3.912	3.912	3.767	3.767
Log wage differential				
90-10	1.204	1.139	1.128	1.128
90-50	0.511	0.511	0.589	0.588
50-10	0.693	0.629	0.539	0.541
75-25	0.678	0.654	0.649	0.636
Standard dev.	0.475	0.459	0.438	0.433
	(c) All			
	Men		Women	
	2013	Counterfactual	2013	Counterfactual
10th percentile	2.398	2.416	2.351	2.351
Median	3.125	3.135	2.955	2.956

90th percentile	3.778	3.775	3.662	3.664
Log wage differential				
90-10	1.381	1.359	1.311	1.312
90-50	0.654	0.639	0.707	0.707
50-10	0.727	0.720	0.604	0.605
75-25	0.763	0.749	0.748	0.756
Standard dev.	0.504	0.500	0.483	0.482

Authors' tabulations based on Statistics Canada, Labour Force Survey, 2013. Note: The counterfactual scenario assumes that labour relations legislation is made fully supportive of unions in all provinces.

Table 11. Mean log hourly wages, by education, union status, sector, and gender

	(a) Private Sector			
	Men		Women	
	Non-union	Union	Non-union	Union
High School	2.859	3.077	2.655	2.816
Postsecondary	3.113	3.259	2.875	2.964
University	3.326	3.252	3.096	3.129
	(b) Public/Parapublic Sector			
	Men		Women	
	Non-union	Union	Non-union	Union
High School	2.926	3.182	2.804	3.065
Postsecondary	3.242	3.346	3.011	3.206
University	3.447	3.530	3.236	3.453

Authors' calculations based on Statistics Canada, Labour Force Survey, 2013. Refers to all employees covered by a collective agreement, not just union members.

Table 12. Distribution of log hourly wages and log weekly earnings, Canada, 2013 and counterfactual

	Log Hourly Wages		Log Weekly Wages	
	2013	Counterfactual	2013	Counterfactual
10th Percentile	2.375	2.374	5.478	5.481
Median	3.021	3.041	6.625	6.633
90th Percentile	3.719	3.719	7.440	7.438
Log wage differential				
90-10	1.344	1.344	1.962	1.958
90-50	0.698	0.677	0.815	0.805
50-10	0.646	0.666	1.146	1.153
75-25	0.761	0.744	0.932	0.933
Standard dev.	0.499	0.496	0.804	0.799

Source: Statistics Canada, Labour Force Survey, 2013, and authors' estimates. Note: The counterfactual scenario assumes that labour relations legislation is fully supportive of unions in all provinces.

Table 13. Household survey descriptions.

Survey	1981 SWH	1984 SUM	1986-1990 LMAS	1991 SWA	1993, 1994, 1996 SLID	1995 SWA	1997-2012 LFS
Format	Person file	Person File	Person file	Person file	Person (1993,1996), Job (1994)	Person file	Person file
Frequency	One Time (annual)	One Time (annual)	Annual	Two years	Annually	Two years	Monthly
Union status	Monthly	Annually	Weekly	Annually	Monthly	Annually	Monthly
Reference period	Week of 15th of each month	December 1984	Each week	November	Monthly	November	Week of 15th of each month
Variable definitions:							
Class of worker	claswkr: paid worker	clwsker: paid worker	q15cow: paid worker; no distinction of private/public	f05q76: paid worker	clwkr9 (1993,1994), clwkr1 (1996)	cowmain: paid worker	cowmain: public or private
Labour force status	q13: employed.	lfstatus: employed. q11: 'paid worker last week' in reference to reference week	clfs_: employed in week 2 of month	lfstatus: employed q10: 'paid worker last week'	mtwrk1 (1993); mtwr1c (1994); ml*v28 (1996)	lfsstat: employed	lfsstat: employed (at work or absent from work)
Union membership	q26: member only	q13_20; q14_21: member or covered	q112; q113: member or covered	q29: member and covered are combined in one variable	uncoll1 (1993, 1996); uncol1c (1994)	swaq29; swaq30: member or covered	union: member or covered
Industry	siccode: exclude fed gov't employees	sic1_: exclude fed gov't employees	sic`i': exclude fed gov't employees	f05q7374: no way to distinguish federal government employees	sigc3g10 (1993, 1994); nai3g10, no way to distinguish federal government employees (1996)	ind30: exclude fed gov't employees	naics_43: exclude fed gov't employees

Survey	1981 SWH	1984 SUM	1986-1990 LMAS	1991 SWA	1993, 1994, 1996 SLID	1995 SWA	1997-2012 LFS
Age	age: < 70 years old	age: < 70 years old	agegrp: < 70 years old	f03q33: < 70 years old	yobg21 (1993); eage26c (1994, 1996)	ageg: < 70 years old	age_12: < 70 years old
Main job	q21 & q22: calculated from data on hours worked per week	Identified by Statistics Canada based on most weekly hours worked	hrs,day: calculated from data on hours worked per week	Job information applies to 'main job'; survey was supplement to LFS. See SWA 1995 codebook	awh (1993, 1994); refers to job #1, no concept of main job in public-use data file (1996)	Job information applies to 'main job'; survey was supplement to LFS	Identified by Statistics Canada based on most weekly hours worked

Table 14. Comparability of CALURA and LFS union density rates

Issue	CALURA	LFS	COMMENT	SOURCE
100+ members	Only unions (national or international) with 100+ members in Canada reported their union members.	Conditional on being employed, the respondent can answer whether she is in a union or not.	CALURA understates relative to LFS; numerator is smaller.	Mainville and Olinek (1999 p. 11 Table 2). Akyeampong (1998 p. 30.)
Retired / Unemployed	Seasonally unemployed workers with recall rights may be included. Retired very unlikely to be included.	Union question asked conditional on employment. Must be paid worker.	CALURA overstates relative to LFS.	Galarneau (1996 p. 44,46). Table 1 (1970 CALURA report). Mainville and Olinek (1999 p.14). Bill Murnighan (CAW) email July 25, 2013.
Age	All union members. No age limit.	Age ranges from 15 to 70+, each of which has union members in LFS.	CALURA overstates relative to LFS.	Galarneau (1996 p. 44).
\`Employees' denominator	From Dec LFS for each year; conditional on employee.	Data are available for all months of year.	CALURA overstates relative to LFS due to seasonal unemployment in Atlantic Canada. We use July LFS to correct.	Galarneau (1996 p. 44)
Multiple jobholders	Would be counted twice in CALURA.	LFS only asks about main job.	CALURA overstates relative to LFS. LFS only allows main job per respondent so will not double-count.	Akyeampong (1997 p. 45). Historical CALURA data on CANSIM: a note to users.
Union members numerator – report date	Date unions report is as of Dec 31st.	Date report is as of Dec 15th.	No issue.	Galarneau (1996 p. 44). Mainville and Olinek (1999 p. 17 table footnotes). "Historical CALURA data on CANSIM: a note to users".
Union members numerator – new profession	In 1983, teachers, nurses, doctors added based on 1981 legislation.	N/A – these professions included.	CALURA understates relative to LFS (and itself) for pre-1983 SWH.	Mainville and Olinek (1999 p. 3-4, 9). Akyeampong (1998 p.31)
Self-employed	CALURA may include self-employed in (mostly) construction industry	LFS identifies self-employed and we exclude.	CALURA overstates relative to LFS.	"Historical CALURA data on CANSIM: a note to users".

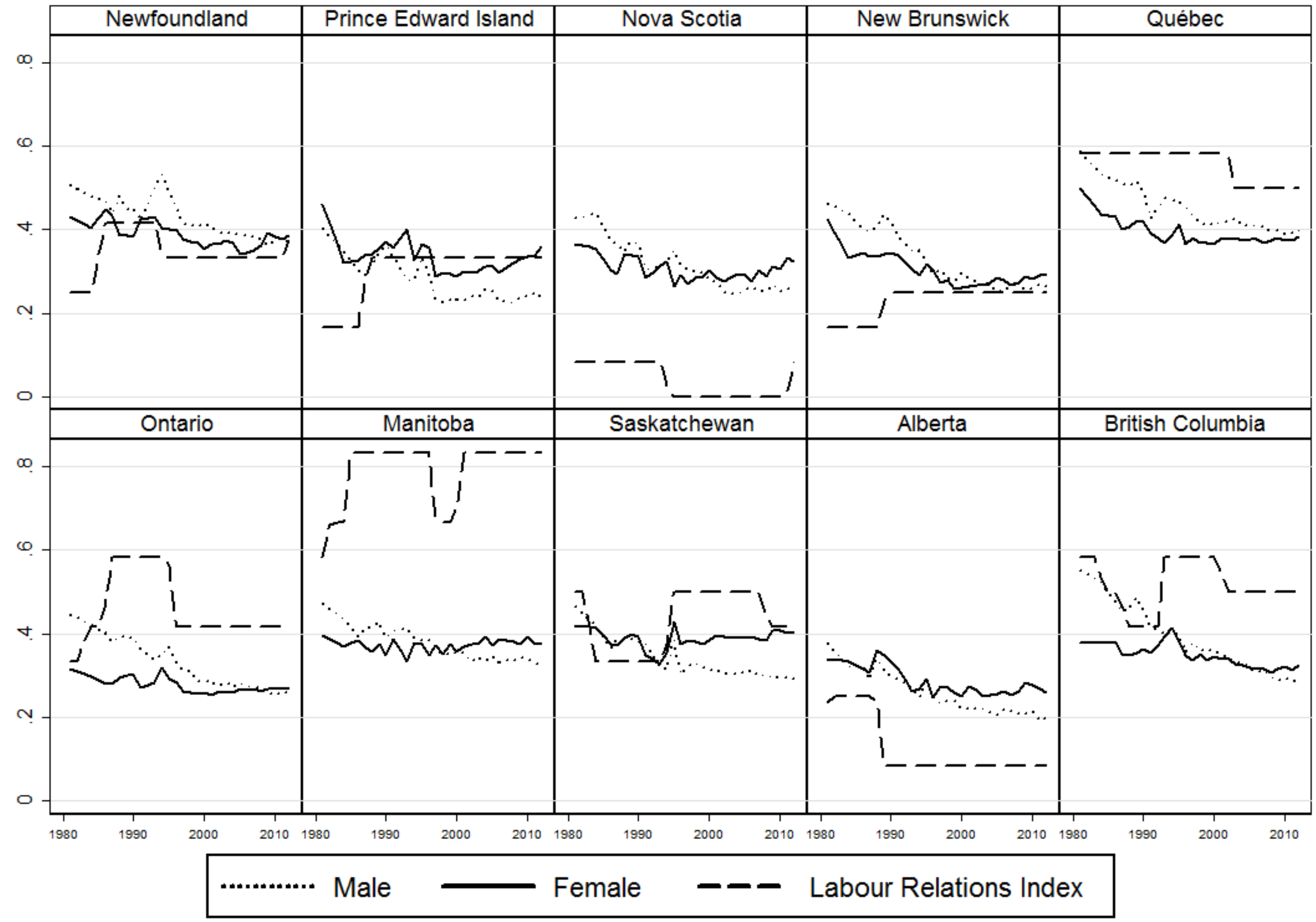
Figure 1. Distribution of log hourly wages (2013 dollars) among women, by union status, Canada, 1984 and 2012

Source: Authors' tabulations based on Statistics Canada, Survey of Union Membership, 1984, and Labour Force Survey, 2012. Note: Vertical lines represent the average provincial minimum wage (in 2013 dollars) in 1984 and 2012. Union refers to all employees covered by a collective agreement, not just union members.

Figure 2. Distribution of log hourly wages (2013 dollars) among men, by union status, Canada, 1984 and 2012

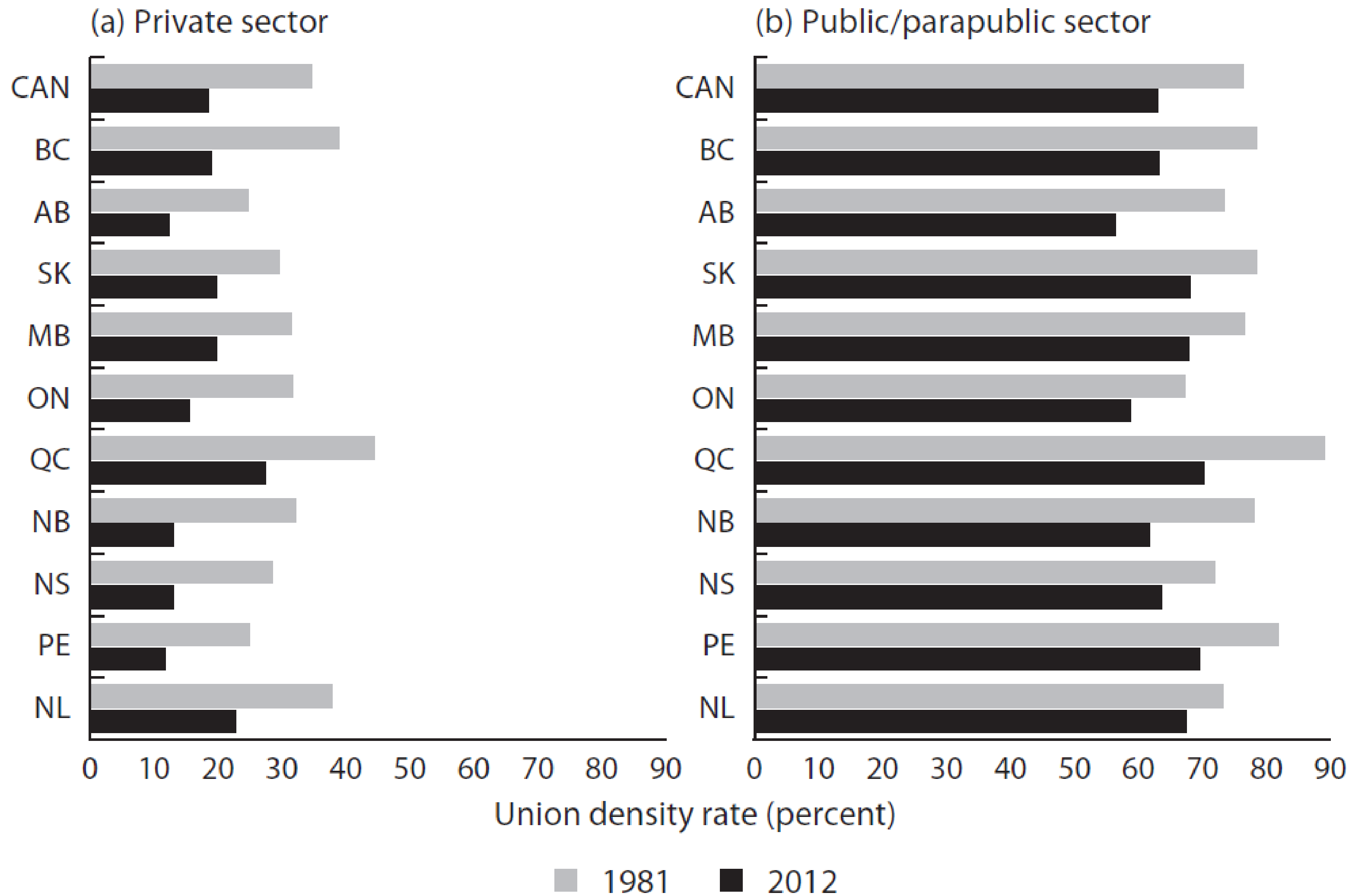
Source: Authors' tabulations based on Statistics Canada, Survey of Union Membership, 1984, and Labour Force Survey, 2012. Note: Vertical lines represent the average provincial minimum wage (in 2013 dollars) in 1984 and 2012. Union refers to all employees covered by a collective agreement, not just union members.

Figure 3. Union density rates by gender and by province, and labour relations index by province, Canada, 1981-2012



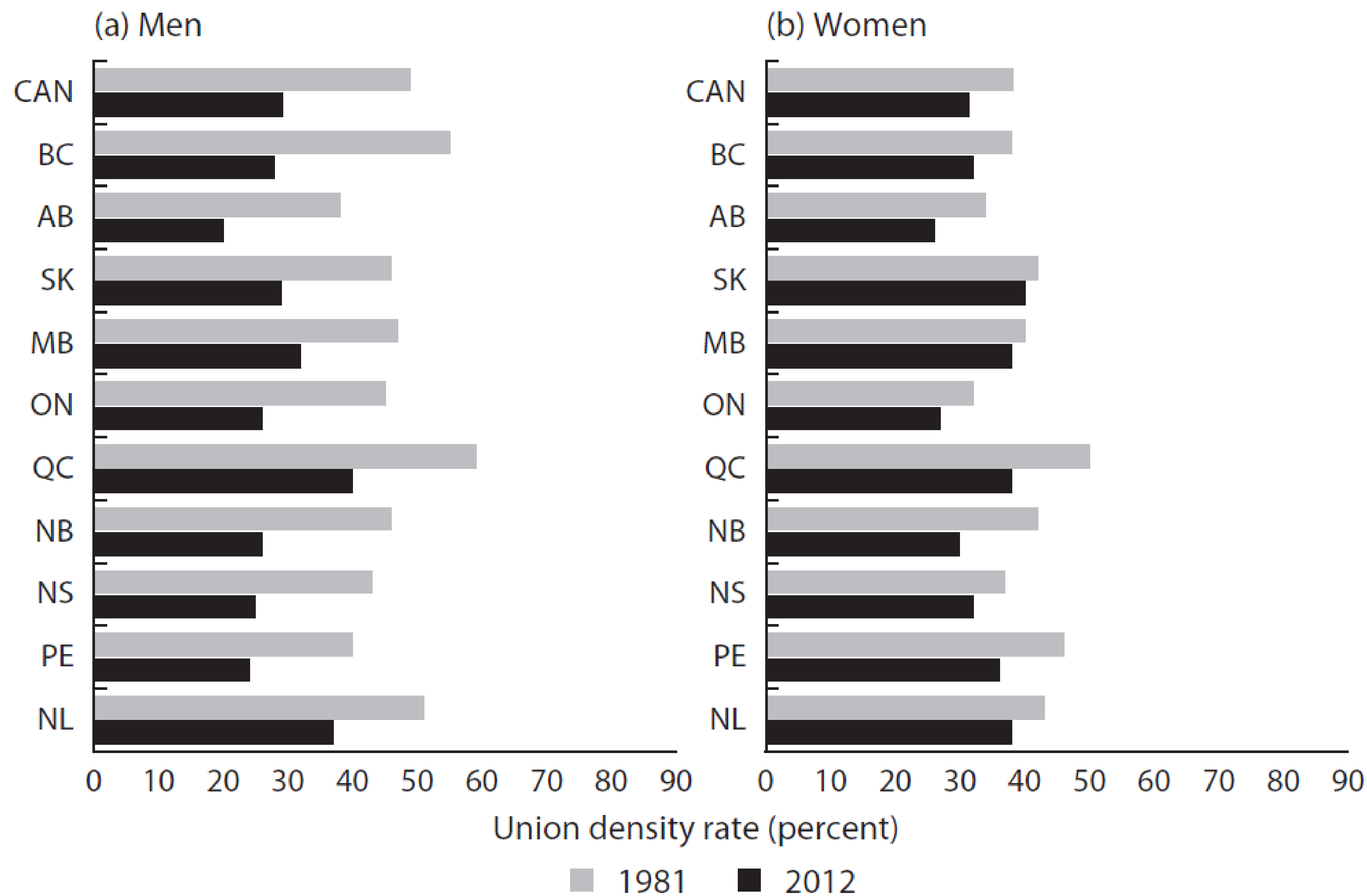
Source: Union density rates based on authors' tabulations; see section 3.2 for details. The labour relations index is described in Section 3.3 and in Table 4. The index is the unweighted average of the [0,1] values in each province in each year. Union density rate refers to the percentage of employees covered by a collective agreement, not just union members.

Figure 4. Union density rate in the private and public/parapublic sectors, by province, Canada, 1981 and 2012



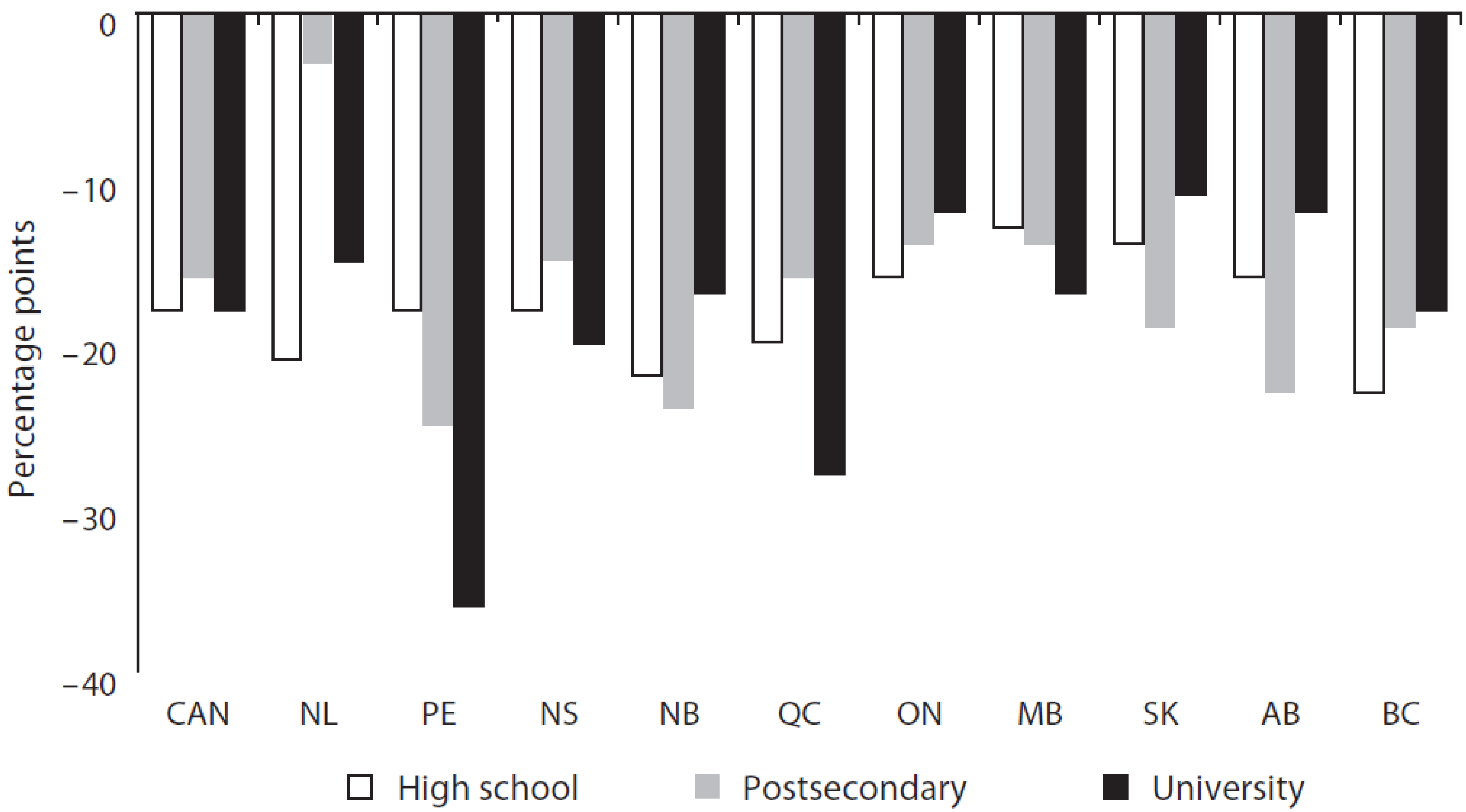
Source: Authors' tabulations based on Statistics Canada, Survey of Work History, 1981, and Labour Force Survey, 2012. Union density rates refers to the percentage of employees covered by a collective agreement, not just union members.

Figure 5. Union density rate by gender and province, Canada, 1981 and 2012



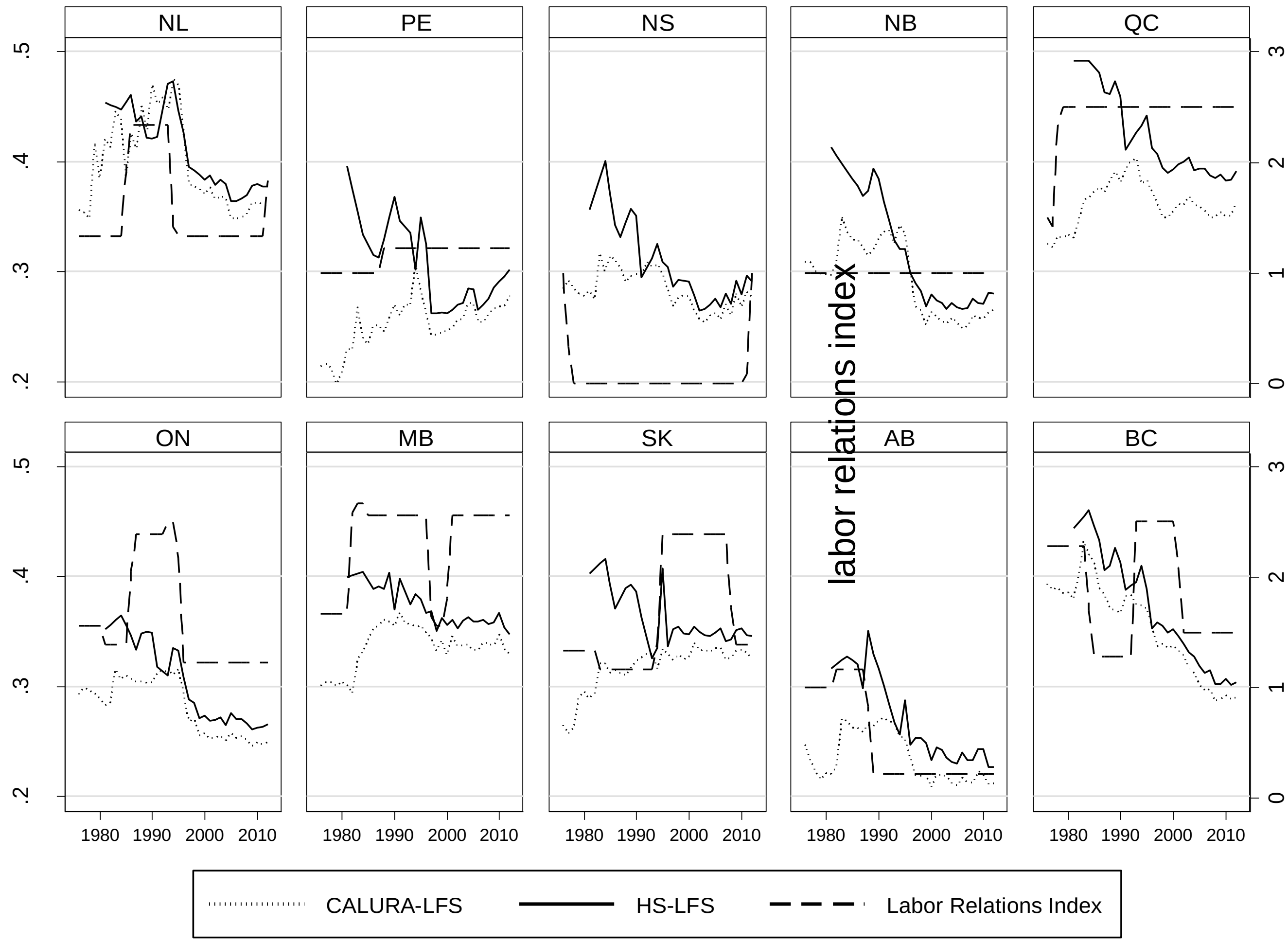
Source: Authors' tabulations based on Statistics Canada, Survey of Work History, 1981, and Labour Force Survey, 2012. Union density rates refers to the percentage of employees covered by a collective agreement, not just union members.

Figure 6. Change in union density rate by educational attainment and province, Canada, 1981-2012



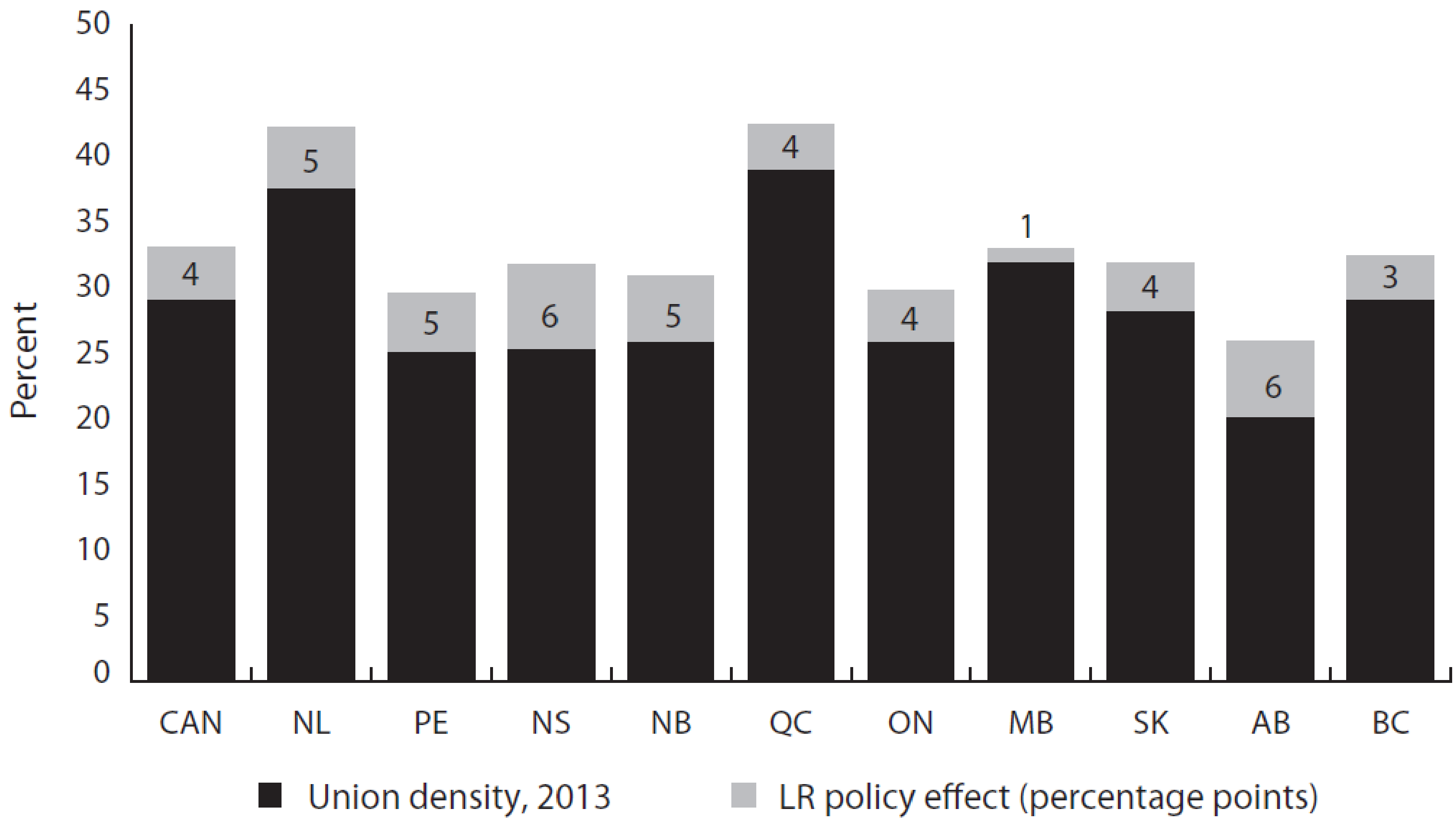
Source: Authors' tabulations based on Statistics Canada, Survey of Work History, 1981, and Labour Force Survey, 2012. Note: Union density among those with a high school diploma or less ranged from 17 percent (PE, AB) to 33 percent (QC) in 2012. Union density among those with a postsecondary certificate or diploma ranged from 25 percent (AB) to 43 percent (QC, NL) in 2012. Union density among those with a university degree ranged from 31 percent (AB) to 48 percent (NL) in 2012.

Figure 7. Union density rate and labour relations index by province, 1976-2012



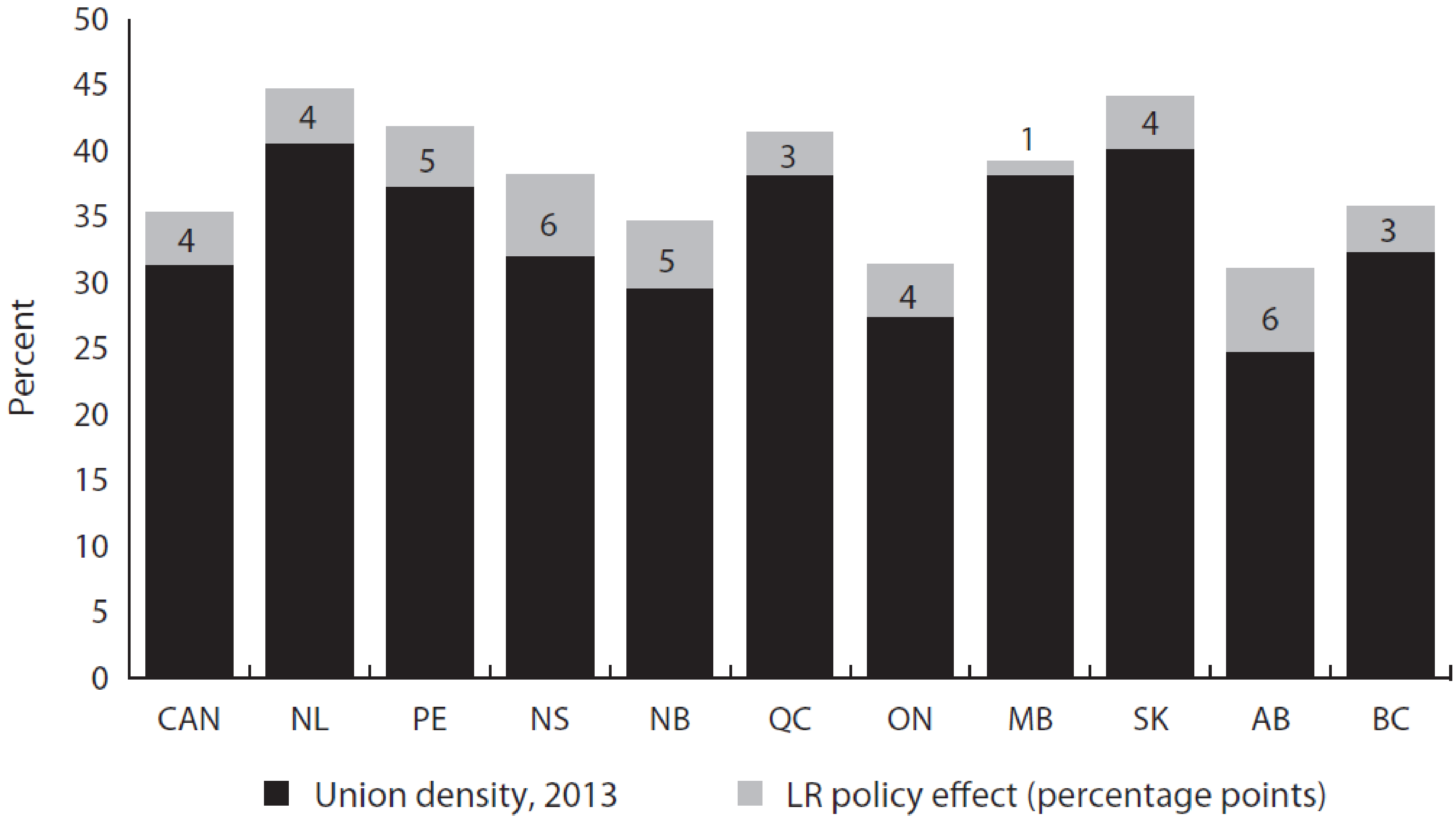
Source: Author's calculations. HS-LFS created by combining several Statistics Canada household surveys. CALURA-LFS created using CALURA administrative data. See Section 3.2 and 3.3 for more details on the construction of these series.

Figure 8. Potential effects of union-friendly labour relations (LR) policy on union density rate among men, by province, Canada, 2013



Source: Statistics Canada, Labour Force Survey, 2013, and authors' estimates.

Figure 9. Potential effects of union-friendly labour relations (LR) policy on union density rate among women, by province, Canada, 2013



Source: Statistics Canada, Labour Force Survey, 2013, and authors' estimates.

Dissertation Conclusion

Many important public policy decisions depend critically on understanding how individuals will respond to reforms, and often economic theory does not give us a clear prediction. In these situations, economists turn to empirical work to further inform the debate. In this dissertation, I have attempted to inform our understanding of how Canadians respond to changes in both personal income tax reforms and labour relations reforms, and in turn, what these responses imply for the ability of government policy to influence income inequality.

In the case of cuts in statutory marginal tax rates, in contrast to other Canadian research, I have found evidence of small elasticities across a number of income sources, income levels, and worker types. As is often true in economics, however, averages can be very misleading and can suppress the role of interesting results that are occurring on the margin. Chapter 1 provided some evidence that there may in fact be some large responses among very high income individuals (specifically, the top 0.01%). Chapter 2 provided some evidence that women with a weak attachment to the labour force may have fairly elastic labour supply. In my other Canadian research, found in Wolfson and Legree (2015), we present evidence that tax planning responses to tax reform may be very important among another narrowly defined subpopulation, namely professionals with corporations. For all of the above reasons, future tax research in Canada may benefit from moving away from the analysis of the overall population, and instead identifying particular subsamples of the population that the theory predicts are likely to yield substantial behavioural responses.

In the case of labour relations reforms, I have provided evidence that union-friendly legal reforms are unlikely to translate into reduced labour market inequality. The reason for this seems to be that those workplaces where labour relations reforms are most likely to translate into higher unionization rates on the margin are not those where unskilled and low-wage workers are located. This result, similar to the results of Chapter 2 for different worker types, highlights the importance of recognizing heterogeneous responses to policy of different worker types within Canada.

It is my hope that this thesis challenges the “conventional wisdom” on the potential for tax and labour relations reforms to influence income inequality. Well-intentioned policy design that does not account for many of the unintended consequences that often follow implementation is one of the reasons why analysis such as that contained within this thesis is necessary. For example, before undertaking this research I had not contemplated such issues as asymmetric tax planning responses among high income earners, nor had I considered how little unskilled workers would have to gain on the margin from an improved labour relations environment. Ideally, future research will be undertaken to build upon this research and sharpen our understanding of how individuals respond to incentives within the Canadian tax and labour relations environments. At the current historic levels of inequality, public policy proposals within these two arenas are likely to dominate Canadian political discourse in the coming years, and further research is warranted.

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