

Removing Disability Insurance Coverage: The Effects on Work Incentive and Occupation Choice

Atsuko Tanaka, Ha Nguyen, and Hsuan-Chih (Luke) Lin*

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Abstract

This paper studies how removing disability insurance coverage affects workers' work incentive and occupation choice. To do so, we exploit the 1997 Canadian Pension Plan (CPP) disability program reform, which required longer work experience for individuals to be eligible for disability insurance. The empirical strategy includes difference-in-difference and triple-difference estimations. The results show that the reform significantly increased work incentive for male individuals with a long non-employment spell. However, the rise in work incentive increased only unemployment, not employment. We also find that the reform barely affected the distribution of employment across occupation.

Keywords: Disability Insurance, Employment, Labor Supply Elasticity

JEL Classification: I18, J32, H51

*Tanaka: Department of Economics, University of Calgary. (atanaka@ucalgary.ca). Ha: Department of Economics, University of Calgary. (honghanguyencuu@gmail.com). Lin (Corresponding Author, +886-2-27822791#207): Institute of Economics, Academia Sinica, 128 Academia Road, Section 2, Nankang, Taipei 115, Taiwan. (linhc@econ.sinica.edu.tw). We are indebted to Stefan Staubli, Jean-Francois Wen, and Tzu-Ting Yang for their useful suggestions and warm encouragement. All errors are ours.

1 Introduction

Disability insurance (DI) is one of the largest government-sponsored insurance programs across the developed world. The DI program insures workers against disability risks by providing benefits when workers become disabled and incapable of earning sufficiently. With an aging population and a rise in chronic conditions, achieving financial sustainability in DI programs is becoming more and more challenging. Many developed countries have recently reformed their DI programs. In re-designing a DI program, policymakers must balance equity and efficiency goals, just as they must for other social programs (Burkhauser and Daly (2002)). On the one hand, making the screening too restrictive might fail to provide benefits to workers in need. On the other hand, a generous program could reduce work incentives for workers with minor work limitations. Thus, to strike a balance between these goals under budget, understanding the benefit obtained from achieving equity and the efficiency cost from the potential DI reform is very important. Economists have especially been concerned about the work disincentive effects of the DI program. While there is a growing body of literature that analyzes DI reform, the literature mostly looks at the effects of changing the amount of DI benefits or changing the screening criteria with a focus on older workers.

This paper adds to the literature by evaluating the impact of taking DI coverage away from workers. We will estimate the effects of removing the DI coverage on workers' labor supply and occupation choice. To do so, we exploit the unique setup of the 1997 Canada Pension Plan (CPP) Disability Program reform (the 1997 reform, hereafter) that tightened DI eligibility criteria in order to reduce the expense of the DI program.¹ Before the reform, workers needed to have worked 2 out of 3 years or 5 out of 10 years in the past in order to be eligible for the DI program. After the reform, workers needed to work 4 out of 6 years. Owing to its design, the reform most affected those with a short work history (e.g. young workers) and those with a weaker labor force attachment (e.g. women).

To analyze the effects of the 1997 reform, our empirical strategy includes both the difference-in-differences method and the triple-difference method. Usually empirical tools have limits in estimating the effects of DI reforms, partly because all workers typically face the same policy change. Canada, however, has two independent but similar DI programs: the CPP and the Quebec Pension Plan (QPP), which makes a comparison possible. In this study, we compare Quebec and the rest of Canada (non-Quebec) using a difference-in-differences method. We will also exploit the variation in intensity of treatment by applying a triple-difference method. Instead of comparing Quebec-non-Quebec, a triple-difference method allows us to examine whether those who are more affected reacted differently than those who are less affected. However, due to data limitations, the sample for this triple-difference approach is restricted to those who did not work for more than one year; we do not have information about the entire work history for all workers. In all cases, the design of the Canadian DI setup provides exogenous variation in DI eligibility, thereby allowing us to isolate the causal effects of our

¹Annual Report of the Canada Pension Plan 2012–2013

interest.

Using a difference-in-differences method, we find that the reform slightly increased the employment of low-income female workers, but scarcely changed employment outcomes for the rest of demographic groups. While the reform was expected to most impact young workers, we do not find any evidence for changes in the employment rates or unemployment rates in these affected groups after the reform. Even for low-income female workers, who are found to be most affected by the reform, the impact of the reform was found to be moderate and not economically significant.

When we apply a triple-difference method, we find significant effects on unemployment. We find that the reform substantially increased job search activity for male workers who did not work more than two years (i.e., those who were not eligible for the DI program after the reform). This result indicates that the reform reduced the fraction of people who were out of the labor force but did not increase the size of employment.

These findings turn out not to support the hypothesis that the DI program reduces employment, often discussed in the literature.² Instead, it confirms economists' perception that young workers' employment is not responsive to changes in DI reform (Gruber (2000)). While the 1997 reform literally removed DI coverage for many young workers, we find that there is no significant change in young workers' labor supply decisions, occupation choices, or the wage in each occupation. Thus, our results indicate that the 1997 reform succeeded in balancing the budget simply because it reduced expenses by making some of the potential DI recipients ineligible; the reform did not increase revenue by increasing the employment of potential DI recipients who became ineligible after the reform.

Therefore, this paper contributes to the literature by presenting empirical evidence that workers' labor supply is inelastic to DI availability. There are many papers that look at labor supply elasticity with respect to accessibility (tightening/liberalizing the screening process) and the generosity of DI programs (changing the amount of benefits), but not many that study availability. Among the seminal literature to study changes in the stringency of the screening process are Gruber and Kubik (1997), Karlstrom et al. (2008), and Staubli (2011). Gruber and Kubik (1997) estimate the effects of the U.S. DI reform in the 1970s that made the screening process stricter on the labor market participation of 45-64 year old men. Karlstrom et al. (2008) study a policy change in Sweden, and Staubli (2011) examines a case in Austria, both of which tighten the screening for older workers in a certain age range. These papers all study reforms that tightened the DI screening process. Another strand of the literature looks at how changes in these benefits affects labor supply. For example, Autor and Duggan (2003) estimate the combined effects of the liberalization of the disability screening process and an increase in the benefits by studying the U.S. 1984 DI reform.

Unlike the existing literature, our study looks at the worst type of DI reform from the workers' perspective; the situation where DI coverage is removed. Making DI unavailable can

²For the literature review, see Bound and Burkhauser (1999), Burkhauser and Daly (2002), and Autor and Duggan (2006)

be regarded as the tightest level of screening criteria (none will be accepted) or the lowest amount of benefit payment (\$0 is awarded). With the worst possible change for workers, we expect workers would react more than ever. The percentage of contributors qualifying for the DI program dropped from 78% to 66% due to the reform, and for young workers under age 25, it dropped from 68% to 34%. Nonetheless, we find little effect on labor supply outcomes. Our finding suggests that the disincentives to work created by the disability benefits appear to be less prominent than has been suggested by other economics research.

Our paper also differs from the other studies by analyzing the effects for all workers, including young workers. While there is a large body of literature on DI reforms,³ most existing studies limit their sample to older workers, aged 45 and above. The exception is [Autor and Duggan \(2003\)](#), who look at low-skilled workers. A major reason why the literature focuses on older workers or low-skilled workers is that these workers consider disability insurance to be a realistic option in their choice set ([Gruber \(2000\)](#)). Consequently, few studies investigate the effects of a DI reform on work incentive problems for young workers.⁴

This paper is the first study to investigate the 1997 DI reform in Canada. Many studies have taken advantage of the Canadian DI structure to estimate the effects of DI reforms, but they have looked at reforms other than the 1997 reform.⁵ For example, [Gruber \(2000\)](#) studies the 1987 CPP reform and estimates the effect of a benefit increase in a CPP Disability program in the 1980s on the labor supply of men aged 45-60. [Campolieti \(2004\)](#) studies the QPP reform in the early 1970s and estimates the effect of a benefit change in Quebec on labor supply. More recently, [Campolieti and Riddell \(2012\)](#) look at the CPP reforms in 2001 and 2005 and estimate the effect of changes in two key features in the Canadian disability policy: the maximum amount of earnings that disability insurance recipients can earn without losing their disability benefits and automatic reinstatement, where recipients can have benefits reinstated without re-application and re-testing for disability determination. We follow a similar methodology to investigate the 1997 reform through a difference-in-differences approach, but later relax assumptions by also applying a triple-difference approach.

The rest of the paper proceeds as follows. Section 2 describes the 1997 CPP Disability Program reform. Section 3 summarizes the data and presents descriptive statistics. Section 4 outlines the empirical strategy. Section 5 presents the results. Section 6 discusses the policy implications. Section 7 concludes.

³Please refer to [Gruber \(2000\)](#) and [Campolieti and Riddell \(2012\)](#) for details.

⁴Among the few studies that study young workers' behavioral response to the DI program are [Kitao \(2014\)](#) and [Low and Pistaferri \(2015\)](#). They study the effects of a counterfactual DI reform while we study the effects of an existing reform.

⁵For the full-scale summative evaluation of the CPP program since 1996, see the report by Human Resources and Skills Development Canada (2011).

2 Background

2.1 Disability Insurance in Canada

Disability insurance in Canada operates in a similar way as it does in the United States, except that the Canadian program is covered under two different plans. All workers are required to contribute to the CPP or QPP program. Which plan covers the contributors solely depends on their residential area. In addition to this setup, two notable differences between the Canadian programs and the American program are the structure of benefits and stringency of the screening process. The Canadian schedule of DI benefits is much less progressive than that of the U.S. Consequently, the Canadian program offers on average lower benefits compared to the U.S. program. The average DI replacement rate in 1998 is 50 percent,⁶ while the replacement rate (including the value of Medicare) in the U.S. for a low-income older male worker was 68 percent in 1984 and increased to 86 percent in 2004 (Autor and Duggan (2006)).

The CPP Disability program is a joint federal and provincial scheme (Quebec excluded). Canada attempts to achieve the single public insurance model (Madore and Tiedemann (2005)), and consequently, private insurance has a non-primary role in Canada; the scope of private health insurance coverage is restricted, which is unique among OECD countries. Especially for long-term disability, the public disability program in Canada is the single largest DI program. Thus, the government funded disability insurance program plays an important, non-substitutable role for all workers in Canada. In 2014–2015, 329,000 disabled contributors and 83,000 children of theirs received these benefits, and the total benefits for these recipients amounted to \$4.2 billion. The DI expenditure is substantial; it is \$18.8 in 2014.⁷

The CPP Disability program aims at providing a basic level of earnings replacement for contributors and their families when the contributors cannot earn an sufficient amount of labor income due to their work limitations. However, with an aging population, achieving financial sustainability in DI programs is becoming more and more challenging. As in other developed countries, the number of DI applicants and recipients have been in an increasing trend in Canada for the past few decades.

Starting in the early 1980s, the government was concerned about the long-term viability of the CPP and facing both an increase in life expectancy and an aging baby boom demographic who would soon be retiring. However, no action was taken until 1995, when the government made reforms to the program in order to address the issue of its sustainability.

The CPP went through a reform in 1995, prior to the 1997 reform central to this study, which revised its medical determination guideline. This reform greatly tightened the screening process by abolishing the practice of taking socio-economic factors into consideration. Since 1995, the DI program has allowed only medical factors to be used to determine eligibility for DI benefits. If the 1995 reform had long-lasting effects on the labor market outcomes is unclear, as it is hard to separate the effects of the two reforms. Yet, it seems the 1995 reform greatly

⁶Source: Own calculations, based on Human Resources and Skills Development Canada (2011).

⁷Source: 2015 Actuarial Report on the Employment Insurance Premium Rate.

affected the number of DI recipients. See Figure A1 for details. We address this potential problem when estimating the effect of the 1997 reform.

Since then, the CPP program has gone through various reforms in part in order to moderate the escalating costs of the CPP. These changes in CPP aimed to ensure the affordability of the CPP to future generations.

Although the QPP Disability program is almost identical to the CPP Disability program, these two programs often go through reforms with different timing, resulting in slightly different benefits provision schemes and eligibility criteria. This setup creates optimal conditions for clear-cut comparison.

2.2 1997 CPP reform

The 1997 reform is historically the largest DI reform in Canada.⁸ As is also the case for the other Canadian DI reforms, the driving force behind the 1997 reform is surging concern about the CPP’s financial sustainability.

The 1997 reform attempts to reduce DI program expense by changing eligibility requirements, which in turn affected labour incentive. This reform tightened eligibility criteria and required stronger labor force attachment for individuals to be eligible for the DI program. Before the reform, in order to be eligible for the DI program, an individual must have contributed to the CPP in two of the last three years, or five of the last ten years. After the reform, an individual has to have made CPP contributions in 4 of the last 6 years in order to receive the disability benefits. During that period, the person must have contributed at least 10 percent of each year’s maximum pensionable earnings.

The 1997 CPP reform was announced in 1997 and came into effect in January 1998. According to the Parliament of Canada, the proposal for the 1997 CPP reform was first officially documented in draft legislation in February 1997. However, the bill died when an election was called in April 1997. The updated version of the proposal of the reform post-election was tabled as Bill C-2 in the House of Commons on 25 September 1997.⁹ However, given its history, people might have anticipated the reform since early 1997. We will investigate whether there is such an anticipatory effect when we report the results from the main analysis in Section 4.1.

According to the 2011 report officially documented by Human Resources and Skill Development Canada (HRSDC), the 1997 reform resulted in a substantial and immediate decrease in the proportion of those eligible for CPP’s Disability program. The age group most affected was those under age 25; their eligibility dropped from 68 percent to 34 percent.

In addition to tightening eligibility, the 1997 reform contains four other changes: (i) lowering DI benefits; changing the earnings-related portion of the disability benefit to be based on the last 5 working years (instead of 3 years), which in effect lowered the maximum benefit by

⁸Table A4 summarizes the major DI reforms in Canadian between 1993 and 2007.

⁹Source: Parliament of Canada.

about \$147 per year on average; and, (ii) increasing contribution rates as part of a move towards fuller funding from a pay-as-you-go pension plan; (iii) changing death benefits to be less generous; (iv) making a vocational rehabilitation program, first introduced as a pilot program in 1993, permanent.

Although the 1997 reform made various changes, we argue that the Quebec-non-Quebec comparison allows us to isolate the effect of tightening eligibility from other effects under some strong assumptions. The QPP, our control group in a difference-in-differences approach, went through the reform at the same time as the CPP. Unlike the 1997 CPP reform, however, the QPP did not change the eligibility criteria. Yet, they changed (i) the calculation of DI benefits and (ii) contribution rates in the exactly same manner as the CPP did. When it comes to labor supply outcomes, we consider that the potential effects of (iii) are small since death benefits are a lump-sum payment with the maximum of \$2,500 to the estate on behalf of a deceased CPP contributor. The last item (iv) did not introduce any new change, and therefore it should not affect behavior in any significant way in 1997. Thus, these reforms were the same in every major way except one; the QPP did not change eligibility requirements, but the CPP did. To isolate the effect of tightening eligibility, we assume that the two major changes in the 1997 QPP reform affected workers in the QPP areas in the same manner as these changes impacted workers in the CPP areas.

We are aware that such assumptions are strong since the effects of the changes are heterogeneous across the two regions. We therefore relax these assumptions when we apply a triple-difference strategy.

Table 1: Distribution of Contributors Qualifying for CPPD by Gender and Year of Contribution

Year	Percentage of Contributors Qualifying Under 4 out of Last 6 Years MQP			
	Females	Males	Both Sexes	% Change Year to Year
1997**	75.0%	81.0%	78.0%	
1998	61.7%	70.0%	66.0%	-15.2%
1999	61.8%	69.9%	66.0%	2.0%
2000	62.2%	70.0%	66.3%	2.6%
2001	62.9%	70.1%	66.6%	3.1%
2002	64.0%	70.0%	67.0%	2.3%
2003	64.0%	71.0%	68.0%	2.1%
2004	64.8%	70.5%	67.8%	1.7%
2005	65.0%	70.0%	68.0%	1.5%

**Note: 1997 qualifying contributors' estimates are based on 2 out of 3 or 5 out of 10 years.

Source: Whetstone Group Consulting, Technical Report on Trends in CPPD Applicants and Caseload, HRSDC, 2008, p.9.

3 Data

3.1 Data Source

The main analysis uses the Survey of Labour and Income Dynamics (SLID) data. The SLID data are collected by Statistics Canada annually and are the primary Canadian source for income data. The SLID was the first Canadian household survey to provide national data on the fluctuations in income that a typical family or individual experiences over time which gives greater insight on the nature and extent of low income in Canada.

The timing of the data collection is as follows. For each sampled household in SLID, interviews are conducted over a six-year period. Every year in January, interviewers collected information on labour market experiences, educational activity, and family relationships during the previous calendar year. We can find detailed information about income sources and government program participation status because Statistics Canada asked respondents' consent to use their T1 tax information for the purposes of SLID. According to the 1996 SLID guideline, over 80 percent of SLID's respondents gave their consent to the use of the tax information.

3.2 Sample Universe

Although the data are available until 2011, we restrict our sample in the main analysis to the data from 1996 to 2000. Since our goal is to isolate the effect of the 1997 reform from that of the other changes, we use the data periods that are not far away from the policy implementation in order to avoid our estimates being affected by other changes in economic or political conditions. While the SLID data are available from 1993, we do not use the data prior to 1996 because the period from 1993-1995 was a transition year that switched the data from SCF cross-sectional data to SLID rotational panel data; as such, there are many missing variables and inconsistencies in the definition of the variables until 1995.

Table 2 presents summary statistics of labor force status, government transfer, and demographic characteristics in our sample. The sample consists of men and women aged 16 to 64 in Canada. After the reform, the proportion of those who receive the DI benefits in our sample slightly decreased from 13% to 12.6% in the CPP areas. Other than the DI recipients, the proportion of government transfer recipients shifts in the same direction in both CPP and QPP areas. For example, the proportion of pension recipients increased in both areas. The amount of benefits from the other government programs also moved in the same direction in both areas. Overall, the summary statistics show that there are no obvious differences in employment outcomes between the CPP area and the QPP area before and after the reform. We will investigate the differences in these two regions before and after the reform conditional on observable characteristics of workers using regression analysis. In the Appendix, Tables A2 and A3 present the same set of variables as in Table 2, but conditional on their gender and age. Again, we do not observe any remarkable changes in the CPP area; the summary

Table 2: Summary Statistics before and after the 1997 reform (1996-2001)

	CPP			QPP		
	Before	After	Diff.	Before	After	Diff.
A. Labor force status (%)						
Out of labor force all year	19.1	19.1	0.0	24.4	23.7	-0.6
Employed all year	55.6	58.1	2.6	49.9	52.9	3.1
Unemployed all year	2.0	1.3	-0.7	3.1	2.4	-0.7
B. Other Government Transfer						
Pension (%)	17.8	19.4	1.6	16.5	18.5	2.0
DI Benefits (\$)	872.3	1,016.3	144.0	764.3	895.4	131.0
Employment Insurance benefits (\$)	592.7	519.6	-73.2	778.6	656.9	-121.7
Social Assistance (\$)	415.2	314.0	-101.2	527.7	403.8	-124.0
Child tax benefits (\$)	248.6	280.0	31.3	349.0	361.1	12.1
C. Background characteristics						
Age	43.0	43.9	0.8	43.2	44.2	1.0
Household size	3.0	3.0	-0.1	2.9	2.8	0.0
# weeks employed per year	35.7	36.8	1.1	32.1	33.6	1.5
# weeks unemployed per year	3.4	2.6	-0.8	4.3	3.3	-1.0
Annual earnings	17,531.9	20,193.6	2,661.7	15,531.9	17,635.0	2,103.1
Number of observations	99,302	190,924	91,622	23,303	45,960	22,657

Source: SLID data; Before: 1996 - 1997; After: 1998 - 2000

statistics before and after the reform show that the trends in the CPP areas move in the same direction as those in the QPP areas.

For our triple-difference analysis, we require the data on work history. Due to data limitations, however, we cannot track everyone's work history backwards for more than one year. We only know how many weeks each individual spends in each labor force status (employed/unemployed/out of labor force). Thus, we can analyze the relationship between the weeks of being employed/unemployed within the reference year and the labor force status observed at the end of the year for all individuals.

Although we do not know workers' complete work history in the data, the SLID data contains a survey question about the spell of non-employment only if an individual did not have a job at the end of survey reference year. To be specific, the survey asks how many months it has been since an individual has last worked. Using this information, we can analyze the relationship between the duration of an individual's non-employment spell in the past years and their current labor force status if an individual did not work for more than one year (Note that if an individual did work within the reference year, their non-employment spell is 52 weeks at maximum. Thus, we do not know their labor force status in the past years). During the sample period between 1996 and 2000, 30.1% of those surveyed were not working at the time period and answered this question. Among this non-working population, 37% of them did not work for more than one year. Conditional on being unemployed for more than a year,

the average non-work spell is 161 weeks and the mean is 116 weeks (i.e., more than half of them had not worked more than two years).

We use the above information as the variables of work history and conduct a triple-difference analysis. With the sample restricted to those who did not work for more than a year, we have 26052 individual-year observations.

4 Effects of the 1997 Reform on Labor Supply

To evaluate the 1997 reform, we adopt a difference-in-differences approach. Our approach is similar to the one first adopted in Gruber (2000), which evaluates the 1987 reform. In the literature on other social programs, Baker and Benjamin (1999a; 1999b), Milligan (2005), and Lemieux and Card (2001) are among the studies that use CPP and DPP programs as comparison groups. The first difference is over time (before and after the policy), as both accessibility of disability benefits and generosity decreased after the reform. The second difference is across regions—Quebec and the non-Quebec areas—because only the non-Quebec region was affected by the 1997 reform. That is, our treated group is individuals in non-Quebec and our control group is those in Quebec.

We implement the difference-in-differences analysis by estimating the following regression:

$$y_{it} = \alpha + \beta_1 CPP_{it} + \beta_2 Post_t + \gamma(CPP_{it} \times Post_t) + X'_{it}\delta + \varepsilon_{it} \quad (1)$$

where i denotes individual, t time, and y_{it} is the three kinds of labor supply outcomes: employed, unemployed, and out of labour force. We measure each outcome as a binary categorical variable and the number of weeks in each labor force status. For example, when we use a categorical variable to look at employment outcomes, y_{it} is a dummy variable that takes 1 if individual i is employed during the reference year. The variable CPP is the regional dummy for the treatment group which is equal to 1 if it is the non-Quebec region and 0 otherwise. The associated coefficient β_1 captures time-invariant regional differences between Quebec and the non-Quebec areas. The third term $Post$ is the dummy which is equal to 1 after 1998. The coefficient associated with this term (β_2) captures changes common across the regions before and after the reform. In other specifications, we also include a vector of time fixed effects, λ_t instead of $Post$. As the after-policy dummy, the time fixed effects capture the time trend common across regions, but in a more flexible manner. The vector X_{it} is a set of individuals' characteristics including gender, marital status, age, household size, and an individual's working history (whether the person worked in the previous year).¹⁰ The coefficient of interest is the estimated effects of the policy change on the outcome variables, which is captured by γ .

We also apply the triple-difference estimator (difference-in-differences-in-differences) and estimate the effects of changing the DI eligibility criteria based on work history. This method is in the same spirit as a difference-in-discontinuities estimator widely used in the empirical

¹⁰Although the number of dependents is an important determinant that could affect the time trend of the outcome, we do not observe this in the data. Instead, we include the household size reported in our data.

literature (Grembi et al. (2012); Casas-Arce and Saiz (2015)). The basic idea is that, given that the new eligibility criteria require a stronger labor force attachment (longer employment periods), the policy affected workers who did not meet the new criteria (worked more than 4 out of the past 6 years) than those who passed the new criteria. The implementation is similar to the difference-in-differences method introduced above, except that we have the treated and the control group defined by work history in addition to before-after and Quebec-non-Quebec. The triple-difference analysis is analyzed by estimating the following regression:

$$\begin{aligned} y_{it} = & \alpha + \beta_1 CPP_{it} + \beta_2 Post_t + \beta_3 (CPP_{it} \times work\ history_{it}) \\ & + \gamma_1 (CPP_{it} \times Post_t) + \gamma_2 (work\ history_{it} \times Post_t) \\ & + \gamma_3 (CPP_{it} \times Post_t \times work\ history_{it}) + X'_{it} \delta + \varepsilon_{it} \end{aligned} \quad (2)$$

where i denotes an individual, t denotes time, and y_{it} is the outcome variable.

Compared to Equation (1), this specification has additional terms that include *work history*, which capture an individual's work history in an attempt to isolate the effects of changes in eligibility from those of other changes (e.g. decreased benefits). The term *work history* is a dummy for not working for more than a certain length of time. As before, we control other factors that affect the employment outcome. The three coefficients β s are estimated and capture just how much differences in region and time can account for differences in the labor market outcome. The effects of changes in eligibility are captured by γ_3 . For a robustness check, we also conduct the analysis with a slightly different model specification by including a vector of time fixed effects, λ_t , instead of the term $Post_t$.

For the estimation method to be valid, we need to assume that there are no effects of the reform on the outcomes of our interest prior to the actual implementation. However, such anticipatory effects might exist because individuals might anticipate the change coming and adjust their behavior before the policy change actually happens. In this regard, the timing of the policy announcement is critical. For this reform, the actual timing of the announcement is not clear from the official documents, but we can test the presence of anticipatory effect using the data by looking at how outcome variables are affected over time.

To investigate whether there is any anticipatory effect, Equation (1) is generalized by adding a full set of treatment times year interaction terms, $\sum_{s=1996}^{2001} \gamma_s (CPP_{it} \times \lambda_s)$:

$$\begin{aligned} y_{it} = & \alpha + \beta_1 CPP_{it} + \beta_2 Post_t + \gamma (CPP_{it} \times Post_t) \\ & + \sum_{s=1996}^{2001} \gamma_s (CPP_{it} \times \lambda_s) + X'_{it} \delta + \varepsilon_{it} \end{aligned} \quad (3)$$

We can test whether the coefficients of the interaction terms for the lagged terms (i.e. γ_s for $s > t$) are significantly different from zero. If any of these coefficients are different from zero, it indicates that there are anticipatory effects, and thus the estimated results cannot be interpreted to capture the pure effects of the employment quota as the estimates are biased. We will present the test results at the end of the next section.

4.1 Results

We present the effects of the 1997 reform on labor supply for each gender. Examining the effects conditional on gender is important because, generally speaking, men and women face different time/resource constraints and respond very differently to work incentives in terms of their labor market responses.

4.1.1 Descriptive Evidence

Before presenting the main results, we plot the trend in employment and unemployment rates around the time of the reform in order to provide indirect evidence of the effects of the reform. Plotting time trends informs us whether a time trend is common across comparison groups before the reform. If the treated group and the control group have different time trends, then estimation will be biased.

Figures 1 and 2 present the time trend of employment and unemployment rates to check if the time trend of the treated group is parallel to that of the control group by age group. Here, we simply plot the employment rate and the unemployment rate by age for each area over years. Overall, the time trend lines run parallel between the comparison groups. The parallel trend is especially clear from the graphs that represent the age groups for 45 and younger, 46-50, and 51-55. In particular, the graph for workers aged 56-60 illustrates very fluctuating rates of unemployment over time. This is due to the fact that we have relatively few observations aged between 56 and 60 in the sample. Since the size of this group of older unemployed people is small, it does not affect the overall unemployment rates to a significant extent.

4.2 The Effects on Labor Supply

We first look at the effects of the policy reform on the labor supply outcomes for workers by age groups. To estimate Equation (1), we use the linear regression. For the discrete labor force status outcomes, we use a logit model.¹¹ For the number of weeks in each labor force status, we use ordinary least squares (OLS). For presentation purposes, we only report the regression results using the number of weeks as the outcome variable.¹²

We focus on the effects of labor supply by age groups because the 1997 reform is expected to affect those with a short work history (e.g. young workers). We estimate the effects of the reform on labor supply for men and women using the whole sample. The results do not change in a fundamental way; they are insignificant, and are presented in the online Appendix.

We look at the effects of workers by age, education, and earnings in the following analysis. For presentation purposes, we here report the regression results that we ran separately for each subgroup. However, the results remain the same when we run regression using the entire sample by adding a full set of treatment times age interaction terms.

¹¹A linear probability model gives almost identical estimates.

¹²We present the rest in the Online Appendix.

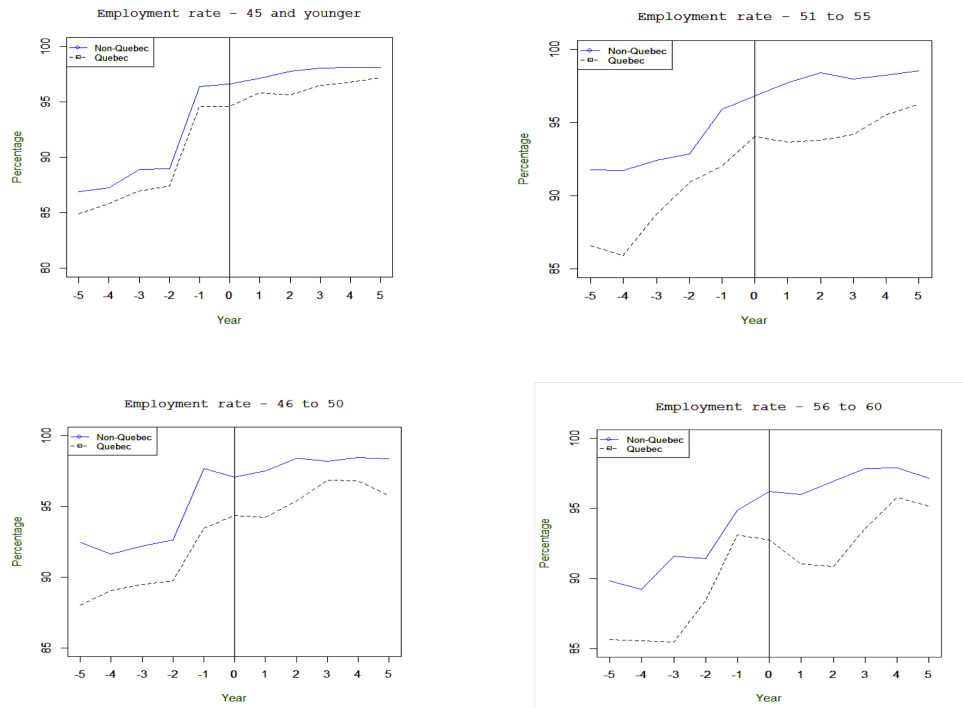


Figure 1: Employment Rates for CPP and QPP

Source: Own calculations, based on SLID.

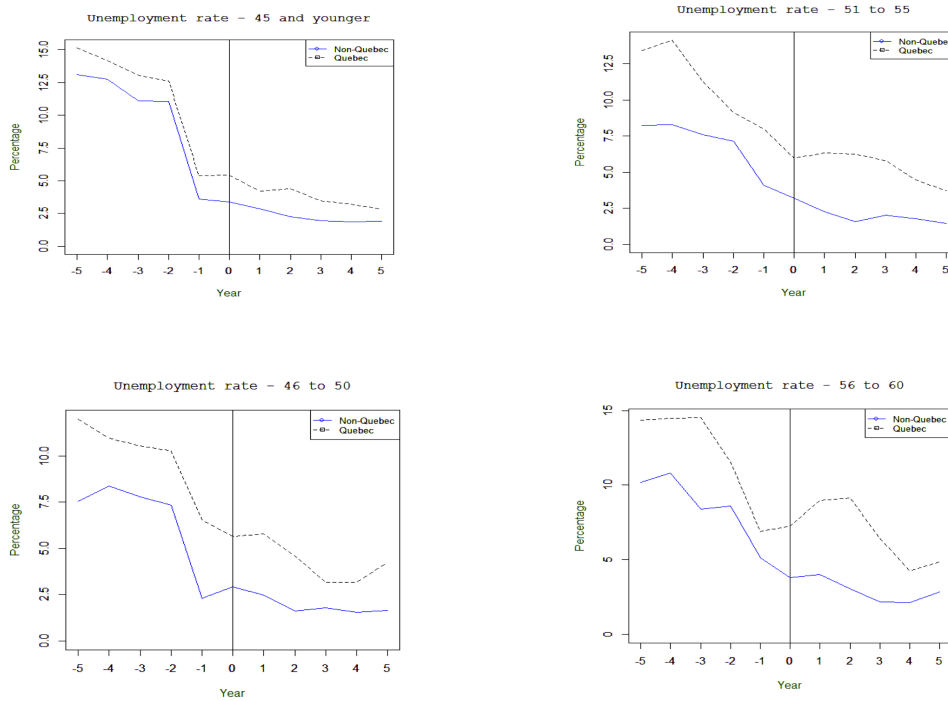


Figure 2: Unemployment Rates for CPP and QPP

Source: Own calculations, based on SLID.

Table 3: Estimated Effects for Men by Age

Dependent Variable: Number of weeks in each labor force status during the ref. year

Sample: Men under 45	Base specification (1996-99)			w/o transition yr (1996,98-99)		
	Empl	Unempl	Out LF	Empl	Unempl	Out LF
	(1)	(2)	(3)	(4)	(5)	(6)
CPP×Post	-0.05	0.11	0.05	-0.15	0.21	0.06
	(0.33)	(0.20)	(0.29)	(0.40)	(0.25)	(0.36)
CPP	-8.90***	4.90***	-4.00***	-8.64***	4.68***	-3.96***
	(0.36)	(0.22)	(0.32)	(0.46)	(0.28)	(0.41)
Observations	68,430	68,430	68,430	50,751	50,751	50,751
R squared	0.22	0.04	0.19	0.22	0.04	0.19
Sample: Men 46-50	Base specification (1996-99)			w/o transition yr (1996,98-99)		
	Empl	Unempl	Out LF	Empl	Unempl	Out LF
	(1)	(2)	(3)	(4)	(5)	(6)
CPP×Post	-0.83	1.25**	0.42	-0.74	1.43**	0.69
	(0.73)	(0.50)	(0.60)	(0.90)	(0.61)	(0.74)
CPP	-6.88***	3.84***	-3.04***	-6.32***	3.52***	-2.80***
	(0.81)	(0.55)	(0.65)	(1.03)	(0.69)	(0.84)
Observations	10,711	10,711	10,711	8,052	8,052	8,052
R squared	0.13	0.06	0.08	0.13	0.06	0.08
Sample: Men 51-55	Base specification (1996-99)			w/o transition yr (1996,98-99)		
	Empl	Unempl	Out LF	Empl	Unempl	Out LF
	(1)	(2)	(3)	(4)	(5)	(6)
CPP×Post	0.51	-0.06	0.45	0.38	0.20	0.58
	(0.91)	(0.51)	(0.82)	(1.13)	(0.64)	(1.03)
CPP	-6.74***	2.74***	-4.00***	-6.44***	2.19***	-4.25***
	(1.04)	(0.59)	(0.94)	(1.33)	(0.75)	(1.21)
Observations	9,328	9,328	9,328	6,997	6,997	6,997
R squared	0.10	0.05	0.06	0.10	0.05	0.06
Sample: Men 56-60	Base specification (1996-99)			w/o transition yr (1996,98-99)		
	Empl	Unempl	Out LF	Empl	Unempl	Out LF
	(1)	(2)	(3)	(4)	(5)	(6)
CPP×Post	0.46	-0.66	-0.20	0.59	-0.56	0.03
	(1.36)	(0.66)	(1.32)	(1.69)	(0.81)	(1.63)
CPP	-6.47***	1.25	-5.22***	-6.56***	0.64	-5.92***
	(1.57)	(0.76)	(1.52)	(2.01)	(0.97)	(1.94)
Observations	6,903	6,903	6,903	5,184	5,184	5,184
R squared	0.06	0.04	0.03	0.05	0.03	0.03

Notes: The dependent variable is the number of weeks in each labor force status during the reference year. Standard errors in parentheses allow for clustering by province. Asterisks indicate significance levels: *** p<0.01, ** p<0.05, * p<0.1. Additional control variables included in all specifications are CPP dummy, year dummies, province dummies, age dummies, education dummies, marital status, and a constant term. The time period is 1996-1999 for base specifications. "Without transition years" excludes observations in 1997.

Table 3 looks at the effects for the subset of the male workers, divided by their age group. We find little effects on labor supply for all age groups, except for male workers aged 45 to 49, whose results are presented in the second top table in Column (2). For those men in this age range, we find a small yet significantly positive effect on the labor supply measured by the number of weeks being unemployed. The positive effect is confirmed with a different model specification. The corresponding results are presented in the same table in Column (5). We find that, compared to years prior to the reform, the average number of weeks unemployed increased by 1.25-1.43 weeks after the reform.¹³

While an increase in the unemployment spell might be counterintuitive, it is not a surprising effect of the reform since the reform may increase work incentive and bring those workers who were out of the labor force (but were not counted as unemployed) to actively look for a job (thus becoming counted as unemployed). In fact, the number of weeks in the labor force also increased although the increase is not statistically significant. An increase of 1.25-1.43 weeks per year is regarded as economically significant; the rise corresponds to a 32.8 to 37.5 percentage point increase since the average number of weeks unemployed prior to the reform was 3.81 weeks for this demographic group.

Despite the fact that many younger workers became ineligible, we find no significant effects on their labor supply outcomes. The results are robust when we divide the group of age under 45 into a finer group.

Table 4 presents the results of changes in labor supply for women. The information presented in this table is similar to Table 3, the table for male workers. Overall, the results indicate that the effects of the reform on these labor supply indicators are not significantly different from zero. That is, we did not detect any significant effects of the policy reform on women's labor supply outcomes when we look at them by age.

We also look at the heterogeneous effects by education and earnings. Overall, we observe little effect of the reform for any demographic group except women whose household earnings are less than \$5,000. Since the sample of workers with a shorter contribution history disproportionately included women, these findings go along with the prediction that women were among the most affected.

Indeed, the analysis from the differences-in-differences allows us to investigate whether the labor supply outcome patterns change in the affected area before and after the reform, but it is subject to strong assumptions, as mentioned before. The assumptions necessary for identification include that the two changes in the 1997 QPP reform and the 1997 CPP reform (reduction in DI benefits and a gradual increase in the contribution rates) affect Quebec and non-Quebec similarly. While plotting the common trend between the two regions before and after the reform may serve as supportive evidence for the assumption, we need to address this issue further.

¹³Such results are robust to other specifications. Please refer to the online Appendix for details.

Table 4: Estimated Results for Women by Age

Dependent Variable: Number of weeks in each labor force status during the ref. year

Sample: Women under 45	Base specification (1996-99)			w/o transition yr (1996,98-99)		
	Empl	Unempl	Out LF	Empl	Unempl	Out LF
	(1)	(2)	(3)	(4)	(5)	(6)
CPP×Post	-0.24	0.24	-0.00	-0.07	0.27	0.21
	(0.40)	(0.20)	(0.38)	(0.49)	(0.24)	(0.47)
CPP	-5.40***	3.03***	-2.36***	-5.41***	3.04***	-2.37***
	(0.44)	(0.22)	(0.42)	(0.56)	(0.27)	(0.53)
Observations	69,262	69,262	69,262	51,466	51,466	51,466
R squared	0.14	0.03	0.12	0.14	0.03	0.12
Sample: Women 46-50	Base specification (1996-99)			w/o transition yr (1996,98-99)		
	Empl	Unempl	Out LF	Empl	Unempl	Out LF
	(1)	(2)	(3)	(4)	(5)	(6)
CPP×Post	0.18	-0.13	0.06	-0.10	-0.56	-0.66
	(0.99)	(0.48)	(0.93)	(1.21)	(0.58)	(1.14)
CPP	-3.91***	1.48***	-2.43**	-3.32**	1.46**	-1.86
	(1.11)	(0.54)	(1.04)	(1.40)	(0.67)	(1.33)
Observations	11,071	11,071	11,071	8,284	8,284	8,284
R squared	0.10	0.02	0.09	0.10	0.02	0.09
Sample: Women 51-55	Base specification (1996-99)			w/o transition yr (1996,98-99)		
	Empl	Unempl	Out LF	Empl	Unempl	Out LF
	(1)	(2)	(3)	(4)	(5)	(6)
CPP×Post	0.29	-0.54	-0.25	1.51	-0.74	0.77
	(1.14)	(0.49)	(1.12)	(1.43)	(0.60)	(1.40)
CPP	-5.84***	2.70***	-3.15**	-6.83***	3.27***	-3.57**
	(1.30)	(0.55)	(1.27)	(1.66)	(0.70)	(1.63)
Observations	9,430	9,430	9,430	7,141	7,141	7,141
R squared	0.11	0.02	0.10	0.11	0.02	0.09
Sample: Women 56-60	Base specification (1996-99)			w/o transition yr (1996,98-99)		
	Empl	Unempl	Out LF	Empl	Unempl	Out LF
	(1)	(2)	(3)	(4)	(5)	(6)
CPP×Post	1.27	-0.56	0.71	1.81	-1.24**	0.57
	(1.46)	(0.52)	(1.48)	(1.79)	(0.63)	(1.81)
CPP	-3.88**	3.33***	-0.54	-5.10**	3.89***	-1.21
	(1.63)	(0.58)	(1.66)	(2.07)	(0.73)	(2.10)
Observations	6,963	6,963	6,963	5,241	5,241	5,241
R squared	0.09	0.02	0.08	0.09	0.02	0.08

Notes: The dependent variable is the number of weeks in each labor force status during the reference year. Standard errors in parentheses allow for clustering by province. Asterisks indicate significance levels: *** p<0.01, ** p<0.05, * p<0.1. Additional control variables included in all specifications are CPP dummy, year dummies, province dummies, age dummies, education dummies, marital status, and a constant term. The time period is 1996-1999 for base specifications. "Without transition years" excludes observations in 1997.

4.3 Results from Triple-Difference analysis

To estimate the effects of removing DI coverage on labor supply outcomes, we also exploit the variation in intensity of the treatment by applying a triple-difference approach.

Now, we consider how the change in eligibility conditions affected workers' labor supply decisions. The 1997 reform made the eligibility criteria stricter by requiring workers to work more than four out of six years to be eligible. Before the reform, working two out of three years or five out of ten years was sufficient to meet the eligibility criteria. This change in eligibility criteria made some people who were eligible before the reform ineligible after the reform. For example, an individual who worked for more than five years out of ten years but did not work for the past three years (e.g. worked only three out of the recent six years) are among these whose eligibility was nullified after the reform. Thus, we can look at the difference in labor supply outcomes between two individuals with similar work history using one of those who became ineligible as the more intensively treated group and the other remain eligible after the reform as the less intensively treated group. e.g., Both of them worked more than two out of three years, but one of them worked for three years and nine months in the past six years (treated) and the other worked four years (control). In this way, we see whether the tightening the eligibility had effects on labor supply outcomes.

This approach is more appropriate than a differences-in-differences approach as it relaxes the assumption that Quebec and the rest of Canada share a common trend in terms of labor force outcomes. Instead, we assume that a common trend exists between workers who have a longer non-employment spell (the treated group) and a shorter non-employment spell (the control group) after controlling for observable difference in trend. two group.

We can also ignore the fact that the 1997 reform contains various changes other than tightening eligibility because the other changes did not impact our comparison groups in the triple-difference approach differently. What is more, the triple-difference approach addresses the concern that our estimates are contaminated by the preceding reform in 1995. Since the 1995 reform did not affect our comparison groups differently either, we can expect that the estimated effect only captures the effect from the 1997 reform, not the 1995 one.

However, we do not observe the complete work history of each individual in the data, as described in the data section. Due to the data limitation, we restrict our sample to those who did not work for more than one year because for these individuals, we can observe how long it has been since they last worked. That is, our sample in the triple-difference analysis consists of those people who did not work more than one year (12 months) at the time of the interview; we look at how many weeks they were looking for a job in the year when they received the interview. Note that the data on employment status is collected from January 1 to December 31 of the previous year. Using the available information, we classify these non-employed individuals by the spell of their non-working period. In our preferred analysis, we divide them into 3 groups: (i) have not worked for more than 12 months but less than 18 months, (ii) have not worked for more than 18 months but less than 24 months, and (iii) have

not worked for more than 24 months. We know with certainty that the third group, who did not work more than 2 years, is not eligible for the DI program after the reform.

Assuming that the longer the spell of non-work period is, the weaker labor force attachment is, we deduce that the longer the spell of the non-work period is, the more likely that the individual is not eligible for the DI program. Since the reform required stronger labor force attachment (a longer employment history), we can consider that those with longer non-employment spells are more likely to be affected by the reform. In fact, within those workers who had not worked for more than two years (those categorized as Group (iii)), nobody was eligible after the reform; while workers who worked more than five years in the past ten years were eligible before the reform.

By sample construction, everyone in the sample had not worked in the reference year. Thus, they are either unemployed or out of the labor force. We will look at the number of weeks unemployed as the outcome variable and see whether the more affected group(s) are more likely to be unemployed after the reform.

Table 5 presents the estimated results of the effects of removing DI coverage on work incentive. We use workers who did not work for more than 12 but less than 18 consecutive months as a reference group. Compared to them, we find that those workers who did not work for more than 24 consecutive months were more likely to look for a job in the survey year after the reform. In fact, the overall effect is found to be 4.4 weeks. This means that if an individual did not work for the past two years or more, the number of weeks during which he/she actively looked for a job increased by 4.4 weeks.

By looking at men in the affected areas, we find that workers who did not work for 18-24 months spend 5.1 more weeks looking for a job compared to those who did not work for 12-18 months after the reform.

Next, we investigate which demographic groups were most affected by the reform. The results are also presented in Table 5 in the column labeled age group. When we look at male individuals by age, we find that such effects are significant and large among those aged 45 to 54. We find that for men aged 45 to 49, workers with no work history for 18-24 months spend 26 additional weeks looking for a job and those with no work history for more than 2 years spend 38 additional weeks when compared to those who have a shorter non-work period (12-18 months). For men aged 50-54, those with 12-18 months of no work spend 14 additional weeks and those with 18-24 months spend 23 additional weeks. We also find such effects for male workers with less than a high school education and some college as well as men with a lower income.

In sum, we find that the reform barely affected employment using the difference-in-differences method. The triple-difference analysis limits our sample to those with more than one year of non-employment and shows that removing DI coverage increased work incentive among workers.

Table 5: Estimated Effects for Men

Dependent Variable: Number of weeks being unemployed during the ref. year

Sample:	All		By age		
Men with non-employment spell more than 1 yr		Under 45	46-50	51-55	56-60
No work (1.5 - 2 yrs)×CPP×Post	5.09**	3.67	26.26**	13.96*	2.08
	(2.28)	(6.82)	(10.64)	(7.29)	(4.72)
No work (2+ yrs)×CPP×Post	3.70	-4.36	37.52***	22.63***	-9.18
	(2.72)	(7.49)	(13.16)	(8.58)	(5.73)
No work (1.5 - 2yrs)	-2.32	-5.82	2.97	-3.17	-1.69
	(1.89)	(4.83)	(9.10)	(5.38)	(3.81)
No work(2+ yrs)	-3.93**	-10.68**	-14.07*	1.58	-3.13
	(1.60)	(4.78)	(7.48)	(4.56)	(3.32)
Post×no work (1.5 - 2 yrs)	0.95	8.38	-18.15	-13.89*	11.64**
	(2.37)	(6.40)	(11.68)	(7.23)	(4.90)
Post×no work (2+ yrs)	-0.76	3.48	-8.78	-8.47	0.80
	(1.97)	(5.87)	(9.28)	(6.08)	(4.04)
Observations	6,015	1,341	588	729	1,148
R-squared	0.18	0.13	0.21	0.14	0.08

Sample: Men	By edu				By income	
	LT high schl	High schl	Some coll.	4yr univ.	<5k	5-60k
No work (1.5 - 2 yrs)×CPP×Post	4.21	6.74	10.95**	-16.67*	5.26**	8.30
	(3.17)	(5.40)	(5.23)	(8.89)	(2.34)	(11.51)
No work (2+ yrs)×CPP×Post	6.81*	-9.37	16.12**	-13.02	4.46	-15.36
	(3.79)	(6.72)	(6.30)	(8.59)	(2.79)	(13.12)
No work (1.5 - 2yrs)	-1.54	-13.67***	7.20	-10.03	-2.29	-3.01
	(2.53)	(5.05)	(4.59)	(6.56)	(1.92)	(11.90)
No work(2+ yrs)	-5.90***	-1.09	-0.06	-5.95	-4.02**	3.12
	(2.21)	(3.80)	(3.63)	(6.64)	(1.63)	(10.29)
Post×no work (1.5 - 2 yrs)	-0.84	8.56	-5.34	14.37*	0.86	3.77
	(3.28)	(6.00)	(5.57)	(7.61)	(2.43)	(12.44)
Post×no work (2+ yrs)	1.19	-5.26	-4.19	11.82	-0.95	-3.84
	(2.73)	(4.61)	(4.55)	(7.94)	(2.02)	(10.70)
Observations	3,304	1,121	1,124	466	5,767	248
R-squared	0.20	0.17	0.25	0.24	0.18	0.47

Note: Standard errors in parentheses allowing for clustering by province. Asterisks indicate significance levels: *** p<0.01, ** p<0.05, * p<0.1. Additional control variables included in all specifications are CPP dummy, year dummies, province dummies, age dummies, education dummies, marital status, and a constant term. Also included are work history dummy (a dummy for not working for more than 18 but less than 24 months; a dummy for not working for more than 24 months), the interaction terms of work history and CPP dummy, and the interaction terms of year dummy and CPP dummy.

4.4 Robustness Check

In order to address endogeneity issues further, we conduct anticipatory effect tests. These tests can show how reasonable the assumption of the common time trend is. Indeed, the common trend assumption is verified when we plot a pre-treatment period graph of employment and unemployment rates between comparison groups at the beginning of this section. However, confirming the common trend by eye-balling tests may not serve as a rigorous testing method, especially since we are concerned about the long-term effects of the reform two years prior to the 1997 reform. Therefore, we carefully examine whether the time trend in the CPP areas was similar to that in the QPP area.

Table 6 reports the results of anticipatory effects tests. We detect anticipatory effects by checking whether the coefficients of the interaction terms for the lagged terms (i.e. γ_s for $s > t$) are significantly different from zero. If any of these coefficients are different from zero, it indicates that there are anticipatory effects. We run regression using the model specification in Equation (3) with 1996 as the reference year. The first three columns present the results when we use a binary variable that takes one if a respondent is in a certain labor force status for all of the year and zero otherwise as outcome variables. The last three columns present the results when we instead use the number of weeks in each labor force status as outcome variables.

The results show that there are no such effects because the interaction term for the lagged term γ_{1997} is statistically not different from zero. Such a finding indicates that the time trend in the CPP areas does not differ from that in the QPP areas prior to the 1997 reform.

The estimated results also test the presence of the post-treatment effects of the reform. Although we find that the estimated coefficients in 2000 are significantly different from zero, it is hard to interpret this as stemming from the effects of the 1997 reform. Since there are no effects till 1999, two years after the reform, we rather interpret these effects as other factors affecting the labor force outcomes across comparison groups in a different manner.

5 Effects of the 1997 Reform on Occupation Choice

In addition to work incentive effects, the 1997 reform might have affected workers' job preference. Since the reform tightened eligibility and reduced benefits, a job with a high risk of becoming disabled became less attractive to workers. Especially young workers, who have a short work history, would be more reluctant to choose a job with high risk of injury, such as a construction job.

To investigate whether the reform actually had such effects on occupation choice, we check whether the distribution of occupation shifted over years. In order to test this, our hypothesis is that if the reform affects the job characteristics such that young workers desire a risky job less than before, the cumulative distribution of occupation could shift from riskier jobs to less risky jobs. In such a case, older workers could fill up the vacancy created by these young

Table 6: Anticipatory Effects Test

Sample: Men						
Dependent Variable:	Dummy for each labor force status			Number of weeks in each labor force status		
	Employed	Unemployed	In labor force	Employed	Unemployed	In labor force
	(1)	(2)	(3)	(4)	(5)	(6)
CPP×1997	0.03	0.11	-0.03	-0.03	0.28	0.24
(γ_{1997})	(0.05)	(0.13)	(0.08)	(0.40)	(0.22)	(0.36)
CPP×1998	0.06	0.04	-0.03	0.22	0.23	0.45
(γ_{1998})	(0.05)	(0.13)	(0.08)	(0.40)	(0.22)	(0.36)
CPP×1999	0.00	-0.14	0.04	-0.22	0.27	0.05
(γ_{1999})	(0.05)	(0.14)	(0.08)	(0.40)	(0.23)	(0.37)
CPP×2000	-0.13***	0.01	0.16**	-1.48***	0.90***	-0.58
(γ_{2000})	(0.05)	(0.15)	(0.08)	(0.41)	(0.23)	(0.37)
CPP×2001	-0.08	0.01	0.19**	-1.15***	0.77***	-0.39
(γ_{2001})	(0.05)	(0.15)	(0.08)	(0.41)	(0.23)	(0.37)
CPP	-0.96***	0.68***	0.40***	-7.59***	3.67***	-3.92***
	(0.04)	(0.10)	(0.06)	(0.35)	(0.20)	(0.32)
Observations	147,570	147,570	147,570	147,570	147,570	147,570
Sample: Women						
Dependent Variable:	Dummy for each labor force status			Number of weeks in each labor force status		
	Employed	Unemployed	In labor force	Employed	Unemployed	In labor force
	(1)	(2)	(3)	(4)	(5)	(6)
CPP×1997	0.04	-0.09	-0.05	0.52	-0.17	0.35
(γ_{1997})	(0.05)	(0.14)	(0.06)	(0.47)	(0.21)	(0.45)
CPP×1998	-0.00	-0.10	-0.04	0.46	-0.02	0.44
(γ_{1998})	(0.05)	(0.14)	(0.06)	(0.47)	(0.21)	(0.45)
CPP×1999	-0.01	-0.34**	-0.03	0.24	-0.08	0.16
(γ_{1999})	(0.05)	(0.15)	(0.06)	(0.47)	(0.21)	(0.46)
CPP×2000	-0.08	-0.24	0.06	-0.60	0.15	-0.44
(γ_{2000})	(0.05)	(0.16)	(0.06)	(0.48)	(0.21)	(0.46)
CPP×2001	-0.07	-0.09	0.08	-1.05**	0.31	-0.75
(γ_{2001})	(0.05)	(0.16)	(0.06)	(0.48)	(0.21)	(0.46)
CPP	-0.54***	0.78***	0.05	-4.96***	2.58***	-2.38***
	(0.04)	(0.11)	(0.05)	(0.41)	(0.18)	(0.39)
Observations	151,275	151,275	151,275	151,275	151,275	151,275

Notes: Each column reports the results from a different regression. The dependent variable is an indicator for employment status dummy as indicated in the column header. Standard errors in parentheses allowing for clustering by province. Asterisks indicate significance levels: *** p<0.01, ** p<0.05, * p<0.1. Additional control variables included in all specifications are CPP dummy, year dummies, province dummies, age dummies, education dummies, marital status, and a constant term.

workers partly because older workers are more likely to be covered by CPP disability insurance after the reform (since they are more likely to have worked more than four out of the past six years).

Figure 3 plots the cumulative distribution function (CDF) of the occupation distribution, which is sorted from a riskier occupation to a less risky occupation. The higher the value of the risk index, the riskier the occupation. The sample is those aged 21 to 25, whose proportion of DI coverage went down from 68% to 34% due to the reform.¹⁴ The graphs on the left are those covered by the CPP and the graphs on the right are those covered by the QPP. The risk indexes on the horizontal axis are calculated as the number of the reported incidence of injury during work divided by the number of employed workers. Each graph corresponds to a subgroup, which is categorized by education. We can test the hypothesis by checking whether the CDF of occupation distribution in the CPP areas was affected by the reform in a systematic way. To be specific, the hypothesis is rejected if the occupation distribution does not shift to the right (more individuals choosing less risky occupations) after the reform after controlling for the time trend in changes of the occupation structure. To control for the time trend, we look at the shift of the occupation distribution in the control group (Quebec) before and after the reform.

The CDF graphs show that the policy little affected the distribution of occupation over years except for college graduates (Edu 4) in Quebec, which is shown in the bottom right graph. The graph shows that, in Quebec, more young workers started working in an occupation with lower risk. The change is more substantial from 1996 to 1997 and 2000 to 2001. Given that the 1997 reform was instituted in January 1998, these shifts do not seem to be attributable to the reform itself. Rather, we consider that there are other factors that caused these shifts. In the CPP areas, the CDF distributions are mostly unchanged before and after the 1997 reform. Such a finding implies that the reform had little effect on the distribution of the occupation.

The distribution of occupation is found to be more rigid for older workers. The plots for older workers are presented in the online Appendix, and we do not find any significant difference for older workers. In sum, we did not find any evidence that young workers chose a job with less risk of injury after the reform.

We also examine the impact of this policy reform on wages associated with occupation. Since risky jobs are less attractive than before, the wage premium for risky jobs might become higher to compensate for a rise in risk. We estimate the effect on wages in a similar fashion as the difference-in-differences approach only to find that the reform little affected the average wage of occupation. Overall, the findings that neither the labor allocation or wage premium over occupation changed imply that switching occupation is difficult.

¹⁴Source: the 2011 report officially documented by Human Resources and Skill Development Canada.

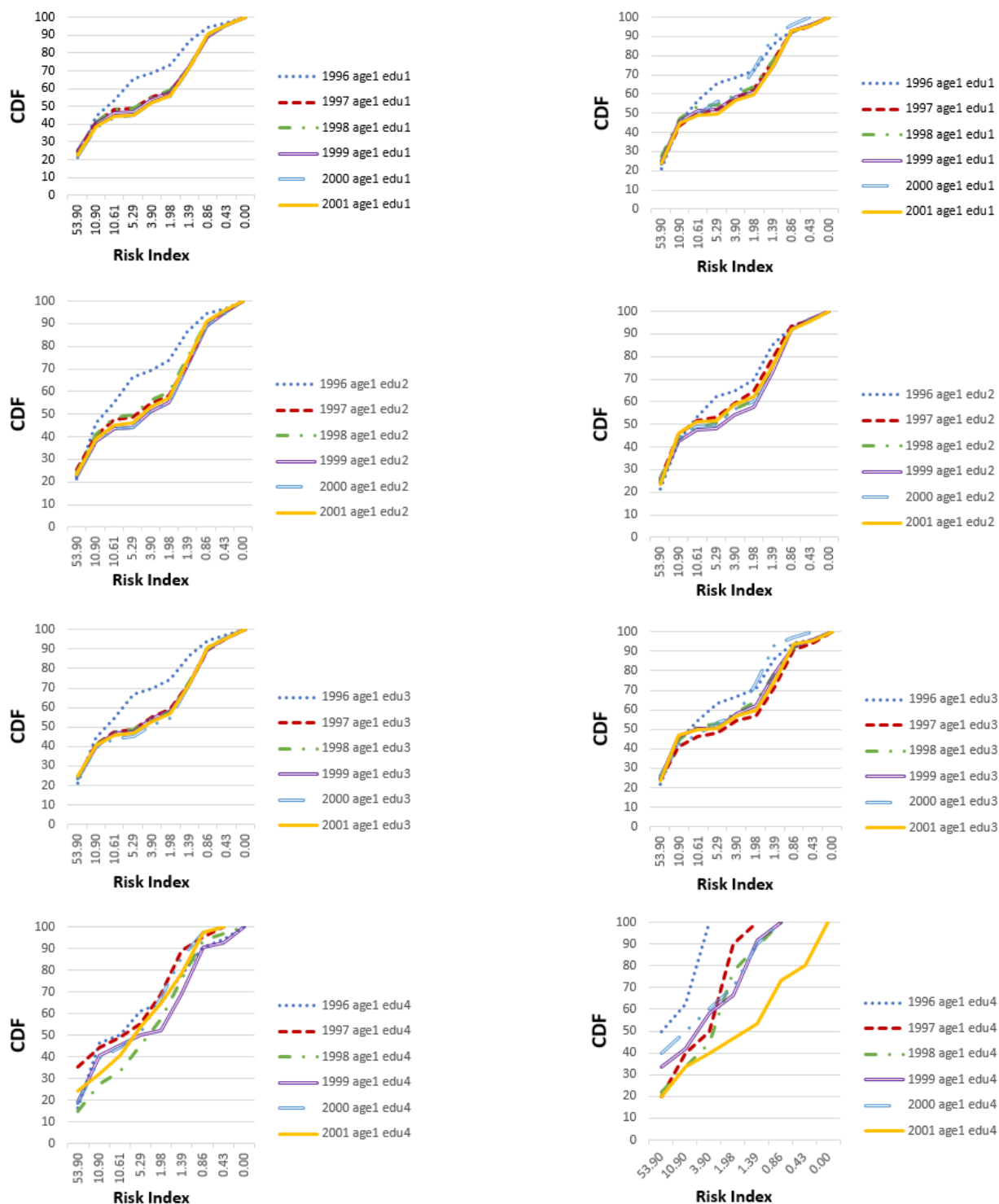


Figure 3: Changes in the distribution of occupation (Age 21-25)

Source: Authors' own calculations, based on the data from 2014 Injury Statistics Across Canada, The Association of Workers' Compensation Boards of Canada (AWCBC) and the data from Labor Force Survey. AWCBC reports the fatalities and lost time claims for each occupation. Age1 denotes the age group of 21 to 25. The plots for older groups are presented in the online Appendix. Edu 1 is less than high school; Edu 2 is high school graduates; Edu 3 is some college; Edu 4 is college graduates.

6 Policy Implications

The objective of the 1997 reform was to reduce the expense of the CPP Disability program in order to achieve financial sustainability. According to the government’s own evaluation report, these reforms have been successful in balancing the DI budget¹⁵ by making DI benefits not available for some contributors, the number of DI recipients decreased. This paper contributes to the literature by presenting evidence that the reform increased work incentive; we find that more workers started spending more time looking for a job. However, we also find little effect on employment. That is, our analysis finds that most people who did not work prior to the reform remained non-employed and became unemployed after the reform.

Such findings provide suggestive evidence that labor supply is inelastic to the DI accessibility in Canada. That is, their decision to work is not impacted by the presence of disability insurance to a significant extent, but rather by other determinants such as work preference, labor market opportunity, non-labor income, and health conditions. Insensitivity to the DI reform is not surprising considering that the monthly payment provided is relatively low. The average annual benefits were \$8,849 and the maximum annual benefits were \$10,744 in 1998. To obtain a better sense of how high or low this amount is, the replacement rate (the ratio of potential DI benefits to the current earnings) often serves as a useful measure. The average replacement rate calculated for 1993-2004 was 48 percent. The replacement rate in Canada has historically been lower than that in the U.S. (Gruber (2000); Autor and Duggan (2006)). In fact, the finding of small employment elasticity is consistent with the perception in the Canadian government report (Annual Report of the Canada Pension Plan 2012-2013). It is also in line with other economics studies using U.S. data (Mueller et al. (2016)).

Our results also show that tightening the DI eligibility is far from a panacea. The reform left some workers out of the DI coverage. We find that workers who became ineligible after the reform look for a job more intensively but mostly remain non-employed. That means that the reform took a safety net away from workers who may have no other ways to gain substantial labor earnings of. The results may further imply that these workers have difficulty finding a job for some reason. A possible reason includes skill depreciation among those with a long spell of non-work or due to long-lasting conditions that prevent workers from finding a job, such as a health limitation. Although we do not observe their health status and do not know how many of these affected workers are in need of DI coverage, we find no evidence that workers who did not work before the reform did not work because of DI. Thus, the reform greatly contributed to financial sustainability, but might fall behind in achieving equity.

There remain concerns about estimation bias due to confounding factors. For example, our findings could be contaminated by domestic migration into and out of Quebec. The interprovincial migration between Quebec and non-Quebec (in and out of Quebec) is relatively small compared to the population. Immigrants to Quebec per year account for 0.1% of the total Canadian population and emigrants account for 0.10-0.13%. Besides, this trend does

¹⁵Annual Report of the Canada Pension Plan 2012-2013

not change around the time of the reform. Given that there is little change in the migrants-to-population ratio from non-Quebec to Quebec or vice versa, we assume that the effects of the reform on migration are ignorable and that most of members in our treated and control groups remain the same. Little mobility between two areas over time is not surprising since the official language is French in Quebec but English in most other parts of Canada. Thus, we assume no self-selection due to migration across the comparison groups. That is, we conclude that the individuals we compare before and after the policy reform are similar in each region (Quebec and non-Quebec) in the characteristics that affect the outcomes of our interest.

Another cause of potential bias in our estimates is policy changes besides the 1997 reform (both in and outside of Quebec). To apply the difference-in-differences method, we need to control for the overall economic trends that could affect labor market outcomes. If any other changes in economic or political factors affect the treated and control groups differently, we cannot obtain an unbiased estimate of the effects of the reform. In addition to the 1995 reform mentioned earlier, a major policy change that might affect labor market outcomes is a change of the name of the system now known as Employment Insurance, which was formerly called Unemployment Insurance. The name was changed in 1996 in order to alleviate negative perception and reduce social stigma. It is just a name change, but it might affect people's decisions on receiving unemployment insurance. The 1996 reform of unemployment insurance also involved changes in actual programs, including the amount of maximum insurable earnings, but this reform affected all regions in Canada. Since our main approach is to look at the differences between the CPP and QPP regions, we can control the effect of any policy changes that affect the two regional groups in the same manner.¹⁶ Thus, other than the 1997 reform, no notable reforms in the unemployment system took place in the late 1990's. In fact, there are no major changes in other social programs that affected Quebec and non-Quebec areas differently. The biggest change among social program reforms was the 1993 Child Benefit reform, a shift from a progressive universal system to an income-tested federal child benefit system. Again, this reform affected all regions in Canada, and thus would not affect the validity of our method.

Like policy changes, economic shocks around the time of the 1997 reform could affect our results; however, all of the years in our sample are in the period in which Canada experienced positive economic growth; the Canadian economy was in the middle of slow growth from 1996-2000. Although the economic growth was sluggish in 2001, around one quarter of GDP growth was negative in 2001, Canada did not experience a recession in that year (Riddell (2005); Campolieti (2011); Campolieti and Riddell (2012)). In fact, the Canadian economy had been relatively stable around 1998, the year of the DI reform of our interest, as shown in Figure A2.

A final variable that could bias our estimates is changes in disability insurance offered by private insurance firms. Indeed, analyzing the private insurance companies' reaction to

¹⁶Refer to Lin (1998) for details of policy reforms in unemployment insurance during 1940-1997.

the DI reform is important since private disability insurance can serve as a substitute for the government-sponsored disability insurance to some extent; it is often recognized that changes to one part of a government-sponsored DI system often have an effect on its other components, including other social security programs and private disability insurance programs ([Torjman \(2002\)](#), p34). However, Canada attempts to achieve the single public insurance model and, consequently, private insurance has a non-primary role in Canada due to its broad restrictions on the scope of private health insurance coverage, which is unique among OECD countries.¹⁷

Admittedly, the direct implication of this applies only to the Canadian context, where the re-placement rate is lower and the acceptance rate is higher than the DI program in the U.S. However, the DI programs have some commonalities and similar reforms are under consideration in other countries, including the U.S. and European countries. Thus, the lesson learned from this unprecedented large scale reform provides a baseline scenario and policy implications valuable to evaluating future DI reforms in the U.S. and beyond.

In particular, our analysis helps us understand how DI plays a role in young workers' labor market outcomes, which is often ignored in the literature. Due to the nature of the reform, which by design most affected young workers' eligibility for DI, our results provide informative evidence on young workers' response to DI. Studying young workers is important because, even though they make up only a small portion of DI applicants and recipients, they typically cost the government more than older workers. A young recipient typically stays in a DI program longer than an older one as the exit rate from the program is typically low.¹⁸ Thus, accepting one young applicant potentially costs the government more than accepting an old applicant, other things being equal. Furthermore, the fact that few young workers apply for or receive benefits does not diminish the role of the DI program for young generations. Since DI can guarantee that a disabled individual will receive enough income for the rest of their life, making sure that they are eligible in the case of unfortunate accidents must matter for young workers. Thus, studying young workers' response to a DI reform is not less important than studying that of older workers, despite their relatively limited numbers.

We also investigate how removing the DI coverage affects workers' occupation and industry choice. In the literature on DI reform, the effects on occupation/industry choice are considered trivial for older workers and often ignored. However, the effects could be substantial for young workers who change their jobs and associated occupation more frequently.¹⁹

¹⁷ For details, see [Young \(2002\)](#).

¹⁸ Once an individual enters the program, few people exit the program before their normal retirement age.

¹⁹ Switching employers is especially common for early-career workers. For example, [Topel and Ward \(1992\)](#) document that two-thirds of job changes occur during workers' first 10 years of work experience in the U.S. [Neal \(1999\)](#) provides a theoretical support for this phenomenon.

7 Conclusion

In this paper, we investigate how the 1997 disability program reform in the CPP affected workers' labor supply decisions. We also examine whether the reform has made jobs with a higher risk of becoming disabled less attractive to workers, especially to young workers newly joining the labor market. With this respect, we investigate whether the reform changed workers' occupation choice. To estimate the causal effects of the policy, we compare the time trends before and after the reform between Quebec (control group) and the rest of Canada (treated group). With the presence of two almost identical disability programs, Canadian data provide a good environment for researchers to isolate the policy effects of DI programs. In addition, we explore the fact that the reform had a larger effect on work incentive for the individuals with a different work history and apply a triple-difference method.

We find that the reform had little effects on employment for most of the demographic groups considered. That is, tightening eligibility and decreasing DI benefits had little effect on overall employment rates. In contrast, we find that the reform increased unemployment for male workers with long non-working spells, who became ineligible for the DI program after the reform. Thus, the reform increased work incentives, but not employment. Such a finding implies that work incentive problems may not be as serious as other literature has suggested because those who are discouraged from applying for the DI program still cannot be employed. These workers do not work or cannot find a job regardless of their eligibility for the DI program. Since the 1997 reform is historically among the largest DI reforms, we can infer that there might be little effect on employment for reforms on a smaller scale.

We also find that the reform little affected the distribution of occupation or wages in each occupation over the years considered, despite the fact that some jobs with a higher risk of being disabled became less attractive after the reform and the new limits it placed on eligibility for DI, suggesting a rigidity of occupational distribution. This rigidity was more strongly observed for older workers. These findings provide some evidence that switching occupation is difficult; it may be partly because finding a job is costly or because workers accumulate skills specific to each occupation.

Appendix

A Descriptive Statistics and Institutional Background

A.1 The 1997 CPP Disability Reform

Figure A1 shows changes in the number of DI beneficiaries by region between 1980 and 2012. From the raw statistics, we see that the 1995 reform seems to have affected the number of DI recipients more than the 1997 reform, but the 1997 reform might have contributed to the continuing downward trend.

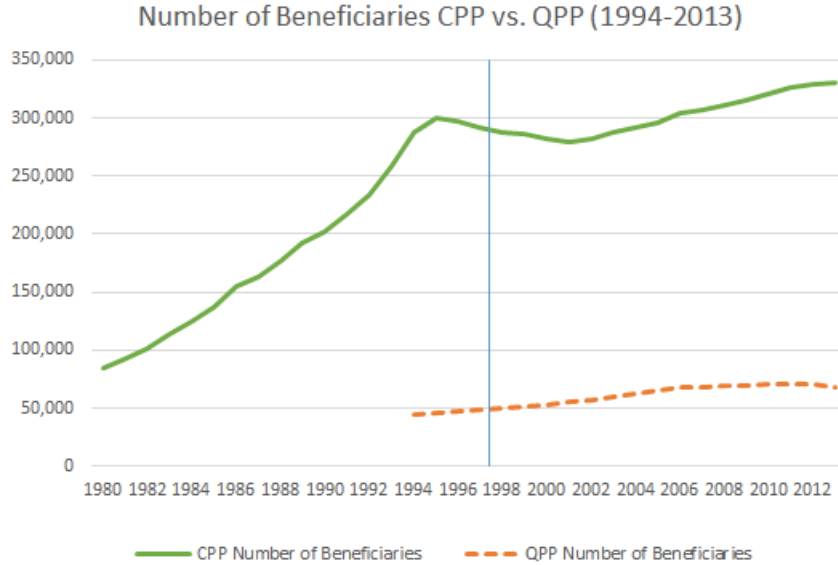


Figure A1: Number of DI beneficiaries

Source: Government of Canada and Retraite Quebec. The data for QPP is only available since 1994.

Table A1 shows that the 1998 reform decreased the number of both beneficiaries and applications to a non-negligible extent. The number of beneficiaries immediately decreased from 292 thousand to 287 thousand. The number of initial applications decreased from 68 thousand to 58 thousand one year after the reform.

A.2 Economic Conditions in Canada around the 1998 Reform

The Canadian economy had been relatively stable around 1998, the year of the DI reform of our interest, as shown in Figure A2. There had been a substantial fall in GDP in the early 1990s and the economy recovered briefly in 1994. While there was a small economic slump in 1995-1996, and another small recession occurred following the collapse of the dot com bubble beginning in 2000, neither affected the Canadian economy to a great extent; the real GDP growth has been positive, between 1.5 and 5.1% during 1992-2007. The lowest GDP growth was around 2% until the economy recessed in 2008. The stability in macroeconomic conditions during the study period is important in applying our estimation method; if any other changes in economic or political factors impact treated and control groups differently, we cannot obtain unbiased estimates of the effects of the policy reform.

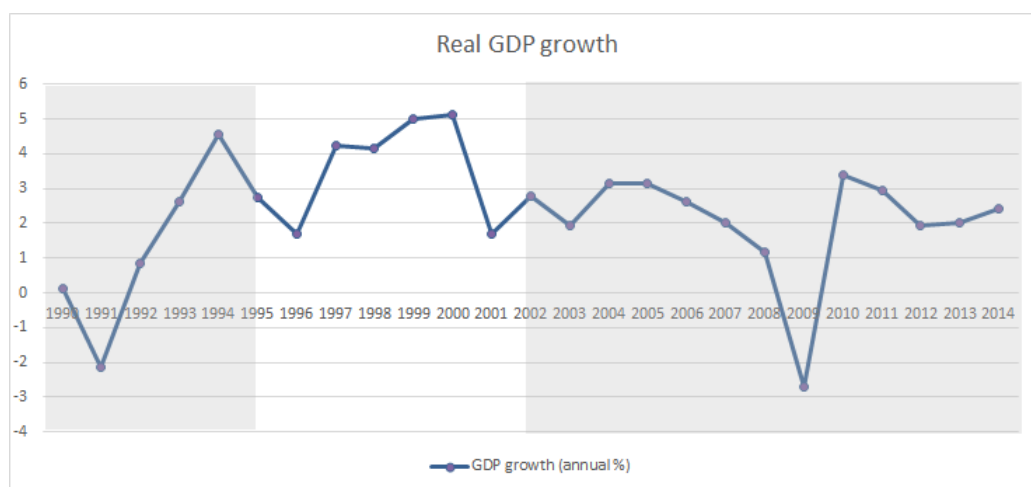
Table A1: Stock and Flow of CPPD Beneficiaries

Year	No. of Beneficiaries (annual average)	No. of Initial Applications	No. of Reconsideration Applications	No. of Terminations
1995	298,698	91,034	17,522	34,715
1996	298,966	79,778	21,307	35,032
1997	292,389	67,898	19,977	34,106
1998**	287,740	68,895	18,675	32,631
1999	287,709	58,619	16,434	31,818
2000	283,508	56,802	13,051	31,406
2001	279,352	55,625	12,575	29,880
2002	281,263	57,010	11,837	29,215
2003	286,717	58,762	10,907	29,494
2004	290,557	61,531	10,384	29,595
2005	294,537	62,962	11,965	29,871
2006	302,288	64,453	13,082	30,287
2007	306,686	63,443	11,403	31,362
1997-2007	290,250	61,455	13,663	30,879

Source: The Department of Human Resources and Skills Development Canada (HRSDC), CPP-OAS 2009 Stats Book; DSB Monthly Reports

** 1998 reforms (tightened contributory eligibility requirements)

Figure A2: Real GDP growth (1990-2014)



Source: World Bank national accounts data, and OECD National Accounts data. Annual percentage growth rate of GDP at market prices is based on constant local currency. Aggregates are based on constant 2005 U.S. dollars.

A.3 Descriptive Summary of Our Sample by Age Group

Table A2: Men: Summary Statistics by age groups before and after the 1998 reform (1996-2001)

Age	Under 45				46-60			
	CPP		QPP		CPP		QPP	
	Before	After	Before	After	Before	After	Before	After
A. Labor force states (%)								
% pension	1.20	1.33	0.91	1.09	6.71	7.06	6.45	6.19
% out of labor force all year	6.69	7.17	9.33	8.38	12.47	12.59	13.86	14.96
% employed all year	61.71	64.33	58.78	61.74	70.61	73.27	63.45	66.98
% unemployed all year	2.09	1.34	3.22	2.32	2.29	1.55	4.80	3.77
B. Other government transfer								
Benefits	48.99	54.92	22.13	19.95	453.38	470.48	386.45	373.98
Employment Insurance benefits	870.83	782.08	1,179.51	1,021.47	814.05	689.15	1,122.09	980.69
Survivor's benefits								
Social Assistance	335.25	186.78	471.95	269.83	358.99	213.68	656.09	419.22
Child tax benefits	21.72	29.79	31.42	43.41	16.91	19.26	29.07	43.37
C. Background characteristics								
Age	30.72	30.90	31.08	31.14	52.16	52.22	52.22	52.23
Household size	3.41	3.35	3.19	3.18	2.97	2.93	2.78	2.76
# weeks employed	40.86	41.92	38.75	40.25	41.83	42.65	38.44	39.72
# weeks unemployed	4.12	3.16	5.04	3.92	3.11	2.29	5.13	3.80
Annual earnings	25,169	28,932	23,035	25,699	34,385	38,485	29,119	32,676
Number of observations	29,595	53,942	6,831	12,841	10,156	21,282	2,605	5,415

Data: The Survey of Labour and Income Dynamics (SLID); Before: 1996-1997; After: 1998-2000.

Table A3: Women: Summary Statistics by age groups before and after the 1998 reform (1996-2001)

Age	Under 45				46-60			
	CPP		QPP		CPP		QPP	
	Before	After	Before	After	Before	After	Before	After
A. Labor market states (%)								
% pension	1.73	1.97	1.33	1.42	9.81	10.06	7.85	9.36
% out of labor force all year	15.43	14.64	20.70	18.53	28.12	26.37	36.57	35.17
% employed all year	54.37	56.61	49.61	52.92	55.90	60.24	45.51	50.40
% unemployed all year	2.10	1.44	2.98	2.24	2.06	1.26	2.86	2.72
B. Other government transfer								
Benefits	61.51	74.34	43.49	46.86	468.82	488.67	455.19	545.97
Employment Insurance benefits	613.36	568.09	716.48	622.15	440.27	339.41	563.01	436.98
Survivor's benefits								
Social Assistance	658.89	560.77	641.01	529.09	422.45	432.76	778.81	863.01
Child tax benefits	736.40	846.92	1,072.26	1,122.13	166.53	192.11	173.83	236.54
C. Background characteristics								
Age	30.98	31.02	31.09	31.17	52.15	52.18	52.16	52.23
Household size	3.48	3.45	3.30	3.29	2.74	2.70	2.53	2.51
# weeks employed	35.96	37.38	32.54	34.69	33.13	35.31	27.86	29.63
# weeks unemployed	3.70	2.87	4.62	3.61	2.77	1.85	3.47	2.77
Annual earnings	14,274	16,729	12,759	15,288	15,992	19,439	12,663	15,991
Number of observations	29,963	55,435	6,839	12,883	10,390	21,938	2,661	5,448

Data: The Survey of Labour and Income Dynamics (SLID); Before: 1996-1997; After: 1998-2000.

A.4 Key Legislative Changes to CPPD and QPPD, 1993 to 2007

Table A4: Major Reforms in CPP Disability and QPP Disability Programs

Year	CPP Disability Program	QPP Disability Program
1993	No changes	<ol style="list-style-type: none"> 1. Substantial changes to the medical adjudication process increasing the extent of medical history and testing required for assessment 2. Less labor force attachment required with contribution requirements lowered to the CPPD level (i.e., 2 of the last 3 years or 5 of last 10 years, see below on CPPD) 3. Language inserted to allow greater flexibility for the Board to use the Regulations to change the medical conditions and circumstances to be considered disabled.
1995	<ol style="list-style-type: none"> 1. Work incentives introduced: Beneficiaries can volunteer or attend schooling without losing benefits; 3 month work trial without losing benefits; fast-track of re-application post 3 month work trial if same disability prevents sustained employment. 2. New medical adjudication guidelines/appeals process; in particular downplaying the use of “socio-economic factors” in assessing disability.* 3. Minor client services-related changes relating to appeals and the disclosure of medical information 	No changes.
1997	<ol style="list-style-type: none"> 1. Earnings-related portion of the disability benefit changed to be based on last 5 working years (instead of 3 years), in effect lowered maximum benefit by about \$147 per year on average. 2. Greater labor force attachment required for eligibility, an increase in contribution to four of last six years (instead of 2 of last 3 or 5 of last 10). 3. Contribution rates increased substantially as part of a move towards fuller funding. 4. Death benefits changed to be less generous. 5. Vocational rehabilitation program (introduced as pilot in 1993) made permanent. 	<ol style="list-style-type: none"> 1. Benefit and contribution rates changed in same way as CPPD (except for the survivors benefit) 2. Three month rule introduced where if the beneficiary worked for three consecutive months (at an earnings equivalent to full-time at minimum wage) they would no longer be considered disabled.
2001	Earning exemption introduced effective May 2001 allowing beneficiaries to earn \$3800 (and possibly more) from work while receiving disability benefit.	No changes
2005	Automatic reinstatement of benefits introduced effective February 2005 if beneficiary is unsuccessful in sustained return to work. Eligibility criteria, i.e., attachment to labor force, is weakened, reduced to 3 of the last 6 years for individuals with at least 25 years of contributions.	No changes
2007	Savings plan for CPPD recipients with matching (up to \$3500) from the federal government. Working tax credit targeted to low income individuals	No changes

From Table 1, [Campolieti and Riddell \(2012\)](#)

* The CPPD program also moved to adjudicating claims in regional offices around this time

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