

Induced Entry in Disability Insurance: Evidence from Canada*

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Disability insurance (DI) programs are large and experienced substantial growth over the last decades. This paper studies the induced entry effect of two important DI program parameters: (i) DI benefit generosity and (ii) financial work incentives for DI recipients. Using two Canadian DI reforms and administrative tax records from 20 percent of Canadians, we find that more generous DI benefits induce substantial entry into DI. Specifically, a 1,000 CAD increase in benefits increases average DI take up by 0.126 percentage points, implying a DI take up elasticity with respect to benefits of 0.67. On the other hand, we find that the introduction of an earnings disregard has a quantitatively small effect on labor supply and entry into DI. Specifically, a 1,000 CAD increase in the earnings disregard leads to induced entry of 0.002 percentage points.

Keywords: Disability insurance, induced entry, policy reform

JEL Codes: J14; H21; I30; D14.

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1 Introduction

The past decades have witnessed a significant rise in the number of individuals receiving disability insurance (DI) benefits in the US and other industrialized countries with substantial fiscal ramifications for government budgets (Autor et al., 2019). The significant growth of DI programs has sparked discussions on how to reform the system to improve the long-term financial stability. Autor and Duggan (2006) discuss three ways to limit the expansion of DI programs: (i) reducing incentives to seek DI benefits, (ii) providing incentives to return to work, and (iii) adopting more rigorous eligibility standards.

In this paper, we provide evidence on the impact of two of the policy instruments, reducing incentives to seek benefits and financial incentives to return to work, on induced entry into DI. We exploit two reforms in the Canadian DI system. The first reform in 1987 increased the level of DI benefits. The second reform in 2001 increased the earnings disregard, the maximum amount DI recipients can earn without risking to lose eligibility to DI benefits. Understanding the magnitude of the induced entry effect of these policy instruments is critical for designing optimal DI policy and quantifying the policy parameters' potential to limit the expansion of DI programs.

Studying the Canadian case has two key advantages. First, the Canadian DI system consists of two separate DI programs: the Quebec Pension Plan disability benefits (QPP-D) covering residents in Quebec and the Canadian Pension Plan disability benefits (CPP-D) covering residents in the rest of Canada (ROC). The two reforms changed DI generosity and the earnings disregard in the CPP-D but not the QPP-D, enabling us to apply a difference-in-differences estimation approach. Second, we can use the Longitudinal Administrative Database (LAD), which contains detailed information about demographics, earnings, income, taxes, transfers, and savings of 20 percent of Canadian tax filers between 1982 and 2016.

Exploiting the Canadian setup, we find that a 36 percent increase in DI benefits

induces significant program entry. We estimate a DI take-up elasticity with respect to DI benefits of 0.67, implying that a one percent increase in DI benefits increases DI take-up by 0.67 percent. The sizeable entry effect suggests that providing better insurance through higher DI benefits has high costs. Put differently, a one-dollar increase in DI benefits costs not just one but 1.67 dollars because of induced entry. Hence, a reduction in DI benefit levels would lead to a meaningful cost reduction if we assume that a benefit decrease triggers the same quantitative behavioral response as a benefit increase. However, program costs are only one component when considering the welfare effects of a change in DI benefits: We need to compare our cost reduction estimate to the loss in insurance value of lower DI benefits. Estimating the insurance value of DI is beyond the scope of this paper, but our DI elasticity estimate still provides an important benchmark. Following the logic of optimal social insurance benefits, it implies that lowering DI benefits is welfare-improving if one dollar in the hands of a DI recipient has a social value of fewer than 1.67 dollars, the total cost of providing this dollar to DI recipients.

By contrast, we find that the effects of introducing an annual earnings disregard are much smaller in magnitude. While the labor supply and the induced entry effect are both statistically significant, they are economically small. We estimate that introducing an annual earnings disregard of 5,300 CAD increases the share of DI recipients by 0.008 percentage points.¹ Hence, a 1,000 CAD increase in the earnings disregard has an induced entry effect of 0.002 percentage points, while a 1,000 CAD increase in DI benefits has an induced entry effect of 0.123 percentage points.² Hence, in the Canadian context, the induced entry of a one-dollar increase in DI benefits is over 60 times larger than the induced entry effect of a one-dollar increase in the earnings disregard.

In contrast to our findings on induced entry, we find no evidence that relaxed

¹The earnings disregard of 5,300 CAD is measured in 2019 Canadian dollars. The annual earnings disregard in 2001 Canadian dollars, the year it was introduced, is 3,800 CAD.

²The induced entry effect of an increase in DI benefits is constructed by dividing the DI level estimate from Table 2 (0.455) by 3.6, as the 1987 reform increased annual DI benefits by CAD 3,600.

earnings restrictions create any program outflow, which weakens the commonly used stepping-stone argument for less restrictive earnings restrictions in DI. The idea is that DI recipients might transition more quickly back to regular employment if they are allowed to work to some extent while receiving DI benefits. Thus, our estimates imply that introducing an earnings disregard did increase overall DI expenditures, but this does not mean that increasing the earnings disregard is a bad idea from an optimal policy perspective. Since we observe a small but significant increase in DI recipients' labor supply, there is again a trade-off: increasing the earnings disregard raises program costs, but it also increases the insurance value for existing DI recipients who can now earn more. While our estimates do not permit us to draw welfare conclusions, they suggest the effectiveness of increasing the earnings disregard as a policy instrument is limited in our context.

Contribution to Literature. We are not the first to study the effects of the two Canadian reforms. Gruber (2000) analyzes the same 1987 reform that increased the level of DI benefits. Our analysis adds to his study in three crucial ways. First, he uses survey data that do not directly measure DI benefit receipt. Therefore, he limits his analysis to the effect on labor force non-participation. Yet, the impact on DI entry is vital to gauge the fiscal implications for the government. Our analysis shows that the reform led to a significant increase in DI take up, but the magnitude of this effect is smaller than the increase in labor force non-participation. Second, Gruber (2000) focuses on men aged 45-59 due to data limitations. We estimate the effects for all age groups and both genders and document significant differences in DI inflow across population subgroups. Third, Gruber (2000) provides limited evidence as to whether outcome variables would have followed the same trends in ROC and Quebec in the absence of the reform, which is the crucial assumption needed for a causal interpretation of the estimates. We offer evidence supporting this assumption by looking at pre-reform trends.

Campolieti and Riddell (2012) evaluate the 2001 reform that increased the earnings disregard. Using a difference-in-differences approach, they find that the reform had no impact on DI take up but increased labor force participation of DI recipients. We build on this paper by applying a novel estimation strategy – a difference-in-differences bunching estimator – that offers an opportunity to document the labor supply response along the earnings distribution in addition to the participation response, which is the focus in Campolieti and Riddell (2012). This analysis reveals that many DI recipients adjust their earnings to bunch precisely at the new earnings disregard, suggesting that they understand the incentives of the earnings disregard well.

We also contribute to the growing literature that studies the impact of DI on labor market outcomes (e.g., Autor and Duggan 2003; de Jong, Lindeboom, and van der Klaauw 2011; Staubli 2011; Maestas, Mullen, and Strand 2013; Moore 2015; Gelber, Moore, and Strand 2017; French and Song 2014; Deshpande and Li 2019). Our evaluation of the 1987 reform adds to this literature by providing empirical evidence on induced entry for a relatively large change in DI benefit levels. For instance, Mullen and Staubli (2016) provide evidence on induced entry leveraging more minor changes in DI benefits in the Austrian context. They report a DI claiming elasticity of 1.2. This paper also contributes to the evaluation of work incentives in DI. Kostol and Mogstad (2014) estimate the labor supply effects of a benefit offset scheme in Norway that allowed DI beneficiaries to keep \$0.4 of every \$1 earned above an earnings threshold. Using a regression discontinuity design, they find that the scheme led to a significant increase in DI recipients' labor supply. Specifically, they find that three years after its implementation, the benefit offset increased labor force participation by 8.5 percentage points among DI recipients below age 50. Ruh and Staubli (2019) exploit bunching at the earnings disregard in Austria and estimate an earnings elasticity of 0.27 among DI recipients. Gelber et al. (2017) study how differences in benefit levels reduce labor supply through an income effect of DI recipients in the United States, documenting that this income effect accounts for a majority of DI-induced reductions in earnings. We add to

these studies by estimating both the labor supply responses and the induced entry effect within the same context.

Road Map. The remainder of this paper is structured as follows. Section 2 describes the institutional background and data. Section 3 presents the results of the 1987 reform that increased DI benefit generosity, and Section 4 discusses the effects of the 2001 increase in the earnings disregard. Section 5 concludes.

2 Institutional Background and Data

2.1 Institutional Background and Policy Variation

A peculiarity of the Canadian social insurance system is the existence of two DI programs. Residents of Quebec—the second largest province in Canada—are covered by the Quebec Pension Plan disability benefits (QPP-D). In contrast, residents in the rest of Canada (ROC) are covered by the Canadian Pension Plan disability benefits (CPP-D). The CPP-D and QPP-D are similar in many aspects, but two reforms in 1987 and 2001 changed the generosity of CPP-D but not the QPP-D. The unique setup with two DI programs and two reforms implemented only in one of the two programs form the basis for our empirical analysis.

The eligibility criteria for benefits are similar in both programs. Individuals need to contribute to the program in two of the last three or five of the previous ten years to qualify for DI benefits. Moreover, applicants must be unable to pursue gainful employment due to a severe and prolonged disability. Finally, both programs feature a four-month waiting period to receive disability benefits to determine whether the disability is long-lasting.

The 1987 Reform. DI benefits consist of three parts in both programs: a lump-sum benefit identical for all eligible recipients, an earnings-related benefit, and a child al-

lowance, which is a fixed amount per month per child below 18. The earnings-related benefit is calculated identically in both programs.³ The lump-sum and child allowance components differ between the two programs. Before 1972, the lump-sum component was the same in both programs, but thereafter the lump-sum part grew much faster in the QPP-D than in the CPP-D. In 1986, the lump-sum transfer in the QPP-D was almost three times larger than in the CPP-D (Gruber, 2000). In an effort to align the two programs, the government decided to raise the CPP-D benefit level in January 1987 so that the benefits in the two programs would be equally generous. This change resulted in a rise in CPP-D benefits of almost 3,600 CAD per year, an increase in the replacement rate of about 36 percent. Figure 1 plots the average monthly DI benefit payments in the CPP-D and QPP-D. Before the 1987 reform, DI benefits in the CPP-D were less generous than in the QPP-D. The adjustment in the lump-sum component of the CPP-D benefits raised CPP-D benefits to and above the QPP-D level. After the reform, the average monthly DI benefit payments in the two programs move in parallel.

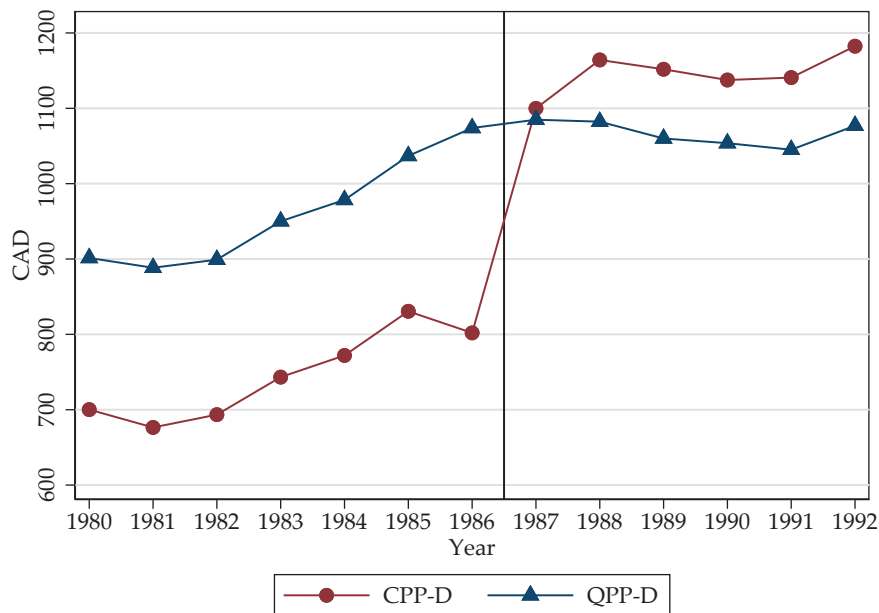
The reform in 1987 changed not only the benefit generosity but also the eligibility criteria with respect to the required earnings history. Before 1987, eligibility to DI benefits in the CPP-D required contributing to the program during the last ten years or during one-third of an individual's career. In 1987, the criteria changed to contributing to the program in two of the previous three years or five of the previous ten years. Hence, eligibility criteria became less demanding in 1987.⁴ The reform in 1987 also introduced an early-retirement option at age 60 in the CPP. Therefore, we restrict our sample to people age 15 to 59.⁵

³For the earnings-related part, the DI recipient's earnings history is inflated by a wage index, and the lowest 15 percent of real monthly earnings are dropped. The earnings-related benefit is then calculated as 18.75 percent of the average monthly earnings of the remaining earnings history.

⁴Ideally, we would want to restrict our sample to individuals who fulfill both pre- and post-reform eligibility criteria to isolate the benefit generosity effect. However, we cannot confirm the contribution criteria over the last ten years as the data only start in 1982. We do a back-of-the-envelope calculation that suggests that the change in eligibility criteria can explain at most 1/10 in the increase in DI take-up.

⁵The early-retirement option could cause anticipation effects before the age of 60. As Gruber (2000) argues, it is unlikely that anticipation effects play an important role as the literature finds no labor supply response among directly affected workers in the age group 60 to 64 (Baker and Benjamin, 1999; Staubli and Zhao, 2021).

Figure 1: Average monthly DI benefit payments in CPP-D and QPP-D 1980-1992

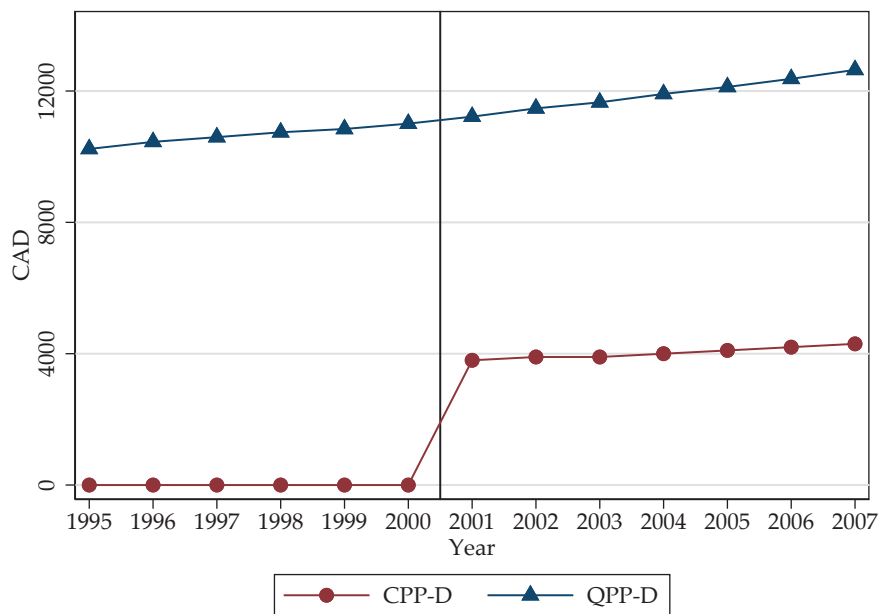


Notes: This Figure shows the average monthly DI benefit payments in the CPP-D and QPP-D (in 2019 CAD). Before the reform in 1987, DI benefits in CPP-D were less generous than in QPP-D. The adjustment in the lump-sum component of the CPP-D benefits in the 1987 reform raised CPP-D benefits to and above the QPP-D level. Pre- and post-reform, the CPP-D and QPP-D benefits evolve in parallel. Numbers are based on Diarra (2015).

The 2001 Reform. Before 2001, DI recipients in the CPP-D were not allowed to earn any labor income. Positive earnings were judged as a violation of the inability of gainful employment and could lead to termination of benefit eligibility. In June 2001, the CPP-D introduced an annual earnings disregard threshold of 3,800 CAD. This earnings disregard works the same way as the substantial gainful activity (SGA) threshold in the U.S. SSDI. DI recipients can have annual earnings up to this threshold without requiring approval or losing eligibility to benefits. After 2001, the earnings disregard threshold increased each year by 100 CAD. The QPP-D introduced an earnings disregard already in the 1960s, but the earnings restrictions in the QPP-D are not based on annual earnings. Instead, a DI recipient's gross earnings are not allowed to exceed three times the monthly maximum pension over a period of three consecutive months. In 2001, this corresponded to a three months earnings limit of 2,805 CAD. The implied maximal annual earnings limit in the QPP-D is then four times this three-month limit,

i.e., 11,220 CAD in 2001, and is considerably higher than the earnings disregard in the CPP-D.⁶ The earnings disregard in the QPP-D increases each year because of the indexation of the maximum pension. Figure 2 illustrates the variation in the earnings disregards over time in both the QPP-D and CPP-D. There were no other changes in 2001 in both programs.⁷

Figure 2: Yearly earnings disregard in CPP-D and QPP-D 1995-2007



Notes: This Figure plots the yearly earnings disregard in the CPP-D and QPP-D in each year. Before the reform in 2001 DI recipients in the CPP-D were not allowed to earn any labor income (positive earnings were judged as a violation of the inability of gainful employment and could lead to termination of DI eligibility). In 2001, the CPP-D introduced an earnings disregard of 3800 CAD. This threshold increased by 100 CAD each year after 2001. The QPP-D introduced earnings restrictions already in the 1960s. In the QPP-D earnings over a period of three consecutive months are not allowed to exceed three times the maximum monthly pension. In 2001 this three month earnings limit was 2,805 CAD , which corresponds to an annual earnings limit of 11,220 CAD. The QPP-D earnings limit increases over the years because of the indexation of the maximum monthly pension.

⁶Earnings exceeding the three-month limit lead to termination of DI eligibility after three months, i.e., individuals receive their DI pension in the first three months when returning to work irrespective of how much they earn.

⁷Between 1987 and 2001, there were some changes in both programs. However, these changes do not interfere with our evaluation of the 1987 and 2001 reforms. Table 1 in Campolieti and Riddell (2012) provides a comprehensive overview of the policy variation after 1993.

2.2 Data and Sample

Our analysis uses data from the Canadian Longitudinal Administrative Database (LAD). The LAD contains detailed information of 20 percent of individuals filing an income tax return between 1982 and 2016. Importantly for our context, the LAD also has information on the receipt of DI benefits, demographics, earnings, income, and other government transfers, enabling us to study in detail how changes in the level of disability benefits and the earnings disregard affect DI claiming and labor supply. We create separate analysis samples for the 1987 and 2001 policy reforms.

1987 Sample. The sample for the analysis of the 1987 reform covers 15- to 59-year-olds who live in one of the ten Canadian provinces and are in the LAD in each year between 1982 and 1992 unless they died before 1992. Individuals do not necessarily need to file a tax return to be in the LAD; non-filers are imputed from a spouse's tax return or from information from an earlier year. Tax filing rates in Canada are high and exceed 90 percent for the working-age population (Stepner, 2019).

2001 Sample. The sample we use for the analysis of the 2001 reform imposes the same age and geography restrictions as the 1987 sample but focuses on the time period between 1998 and 2006. The main outcome of interest in this sample is earnings. We deflate earnings to 2018 dollars using the SGA earnings disregard for each year to have a consistent earnings measure over time. For example, we multiply earnings in 2001 by $\frac{SGA_{2018}}{SGA_{2001}}$.⁸ We then assign each earnings level a 250 CAD bin j running from 0 CAD to 15,000 CAD. The 15,000 CAD bin also includes the few DI recipients who earn more than 15,000 CAD annually. For each of these 61 earnings bins, we collapse the data into annual, provincial disability counts D_{pjt} . We convert these counts into percent using the total provincial population aged 15 to 59 in each year as the denominator. Our final

⁸The SGA exempt amount was 3,800 CAD when it was introduced in 2001, and it subsequently increased by 100 CAD each year after 2001. To deflate earnings before 2001, we assume that the SGA threshold would have been 100 CAD lower per year before 2001.

sample contains 4,320 bin-year-province observations.

3 Impact of Benefit Generosity

3.1 Estimation Strategy

The 1987 reform increased the CPP-D pension to the level of the QPP-D pension while leaving the QPP-D program unchanged. This change increased the CPP-D pension by about 3,600 CAD per year (in 2019 CAD), corresponding to an average increase in the replacement rate of 36 percent (Gruber, 2000). We exploit the policy-induced variation in DI benefit generosity across provinces and time in a difference-in-differences (DiD) design. Specifically, we compare the change in an outcome variable, for example, DI take-up, in the Rest of Canada (ROC) with the change in the same outcome variable in Quebec over time. This comparison can be implemented with the following regression

$$Y_{it} = \alpha + \beta \cdot (Post_t \times Treat_i) + \theta_p + \pi_t + X'_{it}\delta + \epsilon_{it}, \quad (1)$$

where Y_{it} is an outcome variable of individual i in year t , $Post_t$ is a dummy that is equal to 1 for observations after 1986, $Treat_i$ is a dummy that is equal to 1 for individuals living in ROC, θ_p are province fixed effects, π_t are year fixed effects, and X_{it} is a vector of demographic and labor market characteristics (the provincial unemployment rate, age, and its square). The coefficient of interest is β , capturing the average causal effect of the reform-induced benefit increase in ROC over the period 1987-1992 relative to the before-period (1982-1986).

The identification assumption is that, absent the increase in the CPP-D benefit level, the change in Y_{it} across years would have been comparable between ROC and Quebec. A potential concern is that trends in the outcome variable could change across provinces for reasons unrelated to the reform. To investigate this possibility, we also

run a variation of equation (1) that includes a set of treatment times year interaction terms, T_{it} :

$$Y_{it} = \alpha + \sum_{t=1982, t \neq 1986}^{1992} \beta_t \cdot T_{it} + \theta_p + \pi_t + X'_{it}\delta + \epsilon_{it}. \quad (2)$$

Here, each β_t -coefficient measures the average causal effect of the reform-induced benefit increase in year t relative to the base year, 1986. The estimated β_t -coefficients in the pre-reform years, $t < 1987$, provide placebo checks for spurious trends. They should not be statistically significant if trends are parallel. In all regressions, we cluster the standard errors at the census division level.⁹

3.2 Descriptive Evidence

Table 1, Panel A. reports the averages of the variables of interest in the years before (1982-1986) and after the reform (1987-1992) for ROC and Quebec. DI benefit receipt increased by 1.15 percentage points in ROC (from 1.25 percent before the reform to 2.4 percent after the reform). However, DI benefit receipt in Quebec increased by only 0.69 percentage points (1.33 percent to 2.02 percent). Hence, a simple difference-in-differences estimate without any controls suggests that the reform increased DI benefit receipt by 0.46 percentage points (increase in ROC minus increase in Quebec: $1.15 - 0.69 = 0.46$). This simple difference-in-differences estimate is very close to our estimated average effect of 0.455 percentage points from Table 2, Panel A. where we implement regression (1). Table 1, Panel A. also shows that non-employment slightly increases in ROC (+0.23 percentage points), while it falls in Quebec (-1.02 percentage points). Hence, the simple difference-in-differences estimate implies an increase in non-employment of 1.25 percentage points, which is again very close to the average estimate we report in Table 2, Panel A. (+1.17 percentage points). Table 1, Panel A. also reports the average DI benefits of recipients and the average earnings. Note that

⁹Census divisions are geographical areas more granular than provinces. Canada has close to 300 census divisions.

these two variables are measured in nominal Canadian Dollars and are rounded to 100 dollar amounts to comply with vetting guidelines for LAD.

Table 1, Panel B. reports average characteristics in ROC and Quebec before and after the reform. Overall, characteristics are similar between ROC and Quebec both before and after the reform. Note that our balanced sample is aging over time because we follow the same individuals over time. Hence, the average age increases from 34 before the reform to 39 in the five years after the reform.

Table 1: Summary Statistics

	ROC		Quebec	
	1982-1986	1987-1992	1982-1986	1987-1992
<u>A. Outcomes</u>				
DI benefit receipt (in %)	1.25	2.40	1.33	2.02
Non-employment (in %)	15.61	15.84	19.26	18.24
DI benefits of recipients	2,300	4,800	3,100	5,200
	(1,500)	(3,000)	(2,100)	(2,500)
Earnings	16,700	24,600	15,000	21,700
	(18,600)	(34,000)	(15,100)	(24,500)
<u>B. Characteristics</u>				
Share Female	0.51	0.52	0.51	0.52
Share Married	0.66	0.71	0.62	0.63
Age	34.05	39.45	34.11	39.51
	(8.98)	(8.95)	(8.75)	(8.72)
No. Kids	0.28	0.25	0.28	0.22
	(0.65)	(0.59)	(0.66)	(0.56)
No. Observations	6,491,183	7,728,238	2,315,605	2,747,771

Notes: This Table reports averages for the rest of Canada (ROC) and Quebec in the years before (1982-1986) and after the reform (1987-1992).

Figure 3 plots the evolution of the main variables of interest over time in ROC (red dots) and in Quebec (blue triangles). Panel (a) shows that before the 1987 reform, the prevalence of DI receipt is slightly lower in ROC than in Quebec and that DI rates are moving in parallel in both regions. After 1987, DI rates in ROC grow faster than in Quebec. In 1992, the DI recipient rate is around 0.8 percentage points higher in ROC than in Quebec. Panel (b) plots non-employment rates in ROC and Quebec. Non-

employment is significantly higher in Quebec in all years. Until 1990, non-employment is falling in both ROC and Quebec. Before the reform, non-employment falls at similar rates in ROC and Quebec. Between 1987 and 1990, non-employment falls at a slightly higher rate in Quebec, which closes the gap in non-employment to some degree. The 1990 to 1992 recession increases non-employment in both ROC and Quebec but at a higher rate in ROC, which further closes the gap in non-employment. Panel (c) plots the logarithm of disability benefits.¹⁰ Before the reform, DI benefit payments in Quebec are higher than in ROC, reflecting that both DI recipient rates as well as DI benefit levels are higher in Quebec. After the reform, DI benefit payments in ROC exceed payments in Quebec. This is driven by two effects. On the one hand, the reform closes the gap in DI benefit levels. On the other hand, DI take-up rises in ROC after the reform (Panel (a)). Lastly, Panel (d) plots the logarithm of annual earnings.¹¹ In general, ROC has higher average earnings than Quebec, but the initial gap in earnings closes significantly after the reform.

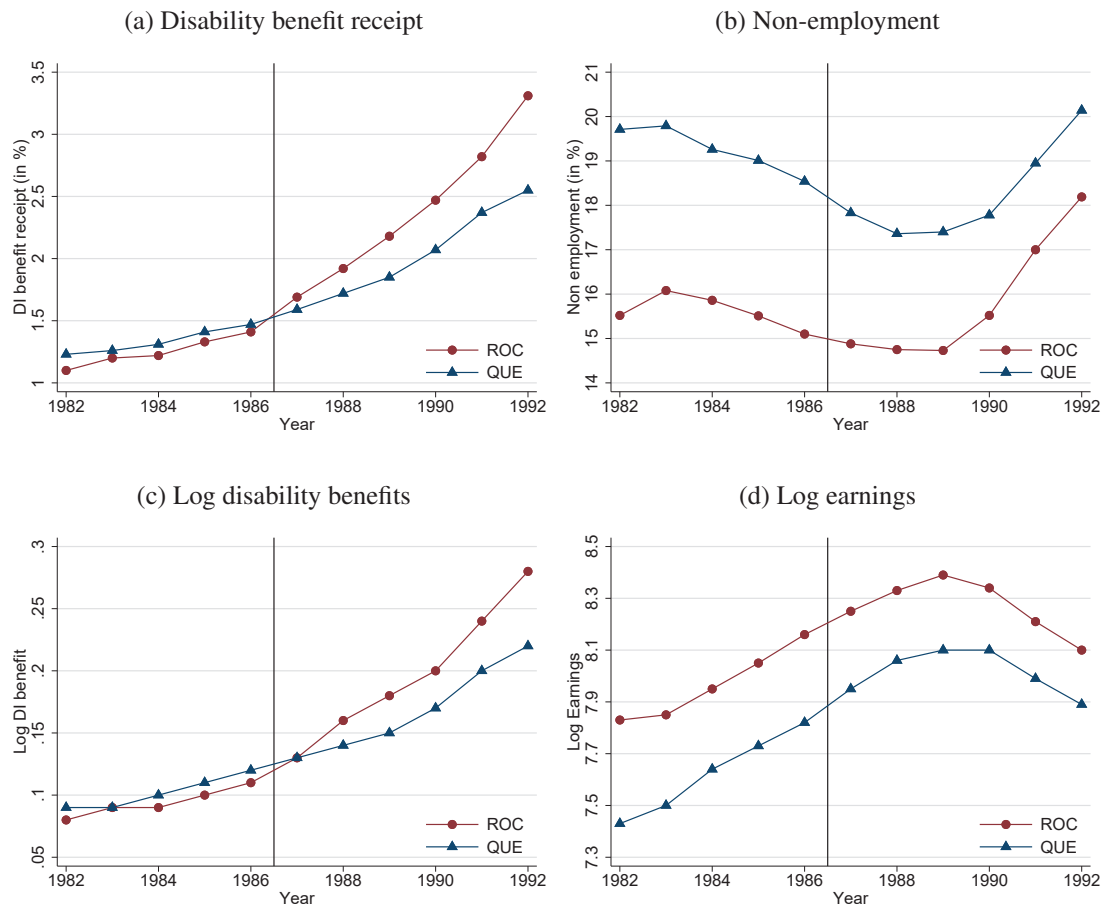
3.3 Empirical Results

Main Results. Figure 4 plots the DiD estimates by year from regression (2) with the 95% confidence interval. Panel (a) contains the estimates for DI benefit receipt, and Panel (b) contains the estimates for non-employment. Panel (a) conveys two main messages. First, the estimates are centered around zero and statistically insignificant before the 1987 reform, i.e., ROC and Quebec have parallel trends in DI reciprocity rates before the reform. Second, DI reciprocity rates increase significantly and steadily in ROC relative to Quebec after the reform. In 1992, the DI reciprocity rate increased by 0.83 percentage points in ROC relative to Quebec due to the reform, a 50 percent increase compared to the average DI reciprocity rate in ROC before the reform. Hence,

¹⁰The logarithm of disability benefits is defined as $\ln(\text{DI benefits} + 1)$ to also include individuals who receive no disability benefits.

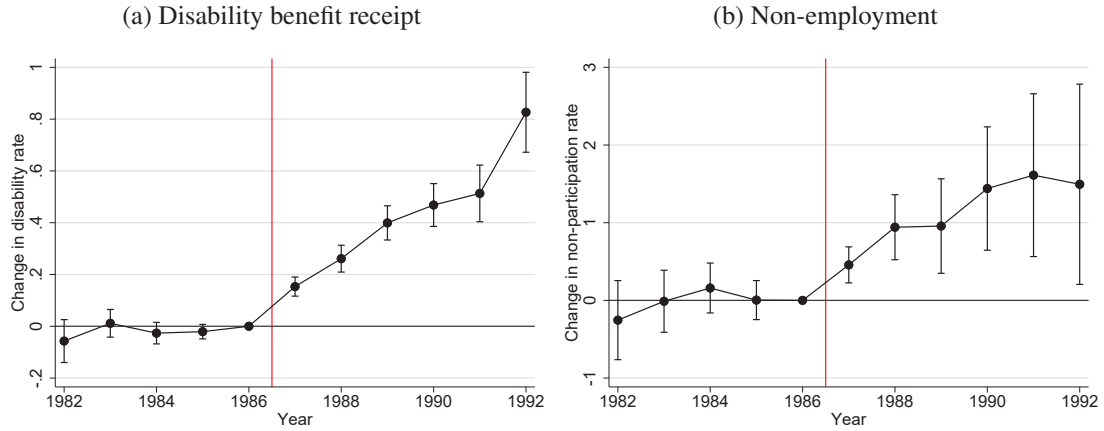
¹¹Again, the logarithm of earnings is defined as $\ln(\text{earnings} + 1)$ to also include individuals who have zero earnings.

Figure 3: Descriptive Figures



Notes: This Figure plots the evolution of the main variables of interest over time in ROC (red dots) and in Quebec (blue triangles).

Figure 4: Effect on DI benefit receipt and Non-employment



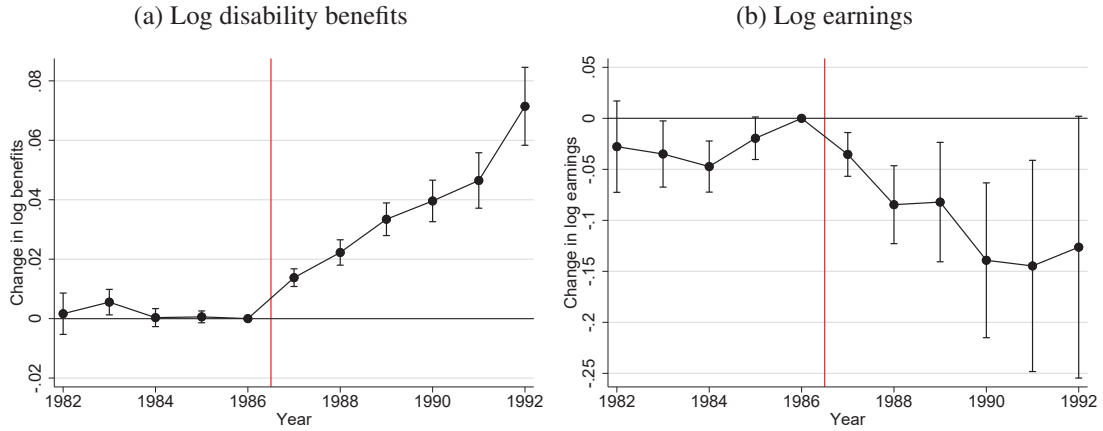
Notes: This Figure plots the DiD estimates (black dots with 95% confidence interval) by year from regression (2) for DI benefit receipt in Panel (a) and non-employment in Panel (b). Standard errors are clustered at the census division level.

higher benefit levels induce entry into DI.

Panel (b) shows that the reform also increased non-employment significantly in ROC compared to Quebec, i.e., the reform reduced employment. The non-employment effect is larger than the induced DI entry effect (note the different scaling in Panels a and b). In 1992, we estimate that non-employment increased by 1.5 percentage points in ROC compared to Quebec due to the reform. The non-employment point estimate is almost twice as large as the DI benefit receipt point estimate, suggesting that looking only at non-employment as Gruber (2000) overestimates the induced DI entry effect. However, the non-employment effect is not precisely estimated, and we cannot reject that the DI benefit receipt effect is of the same magnitude as the non-employment effect. Figure 4 plots the DiD estimates for the logarithm of disability benefit payments and earnings, showing that the reform increased DI benefit payments and reduced earnings.

Table 2 reports the average DiD-estimates for different samples. Panel A. reports the average DiD estimates for our “long sample,” in which we compare the outcomes in the post-reform years 1987-1992 to the pre-reform years 1982-1986. On average, the higher DI benefits increase DI benefit receipt by 0.455 percentage points and non-employment

Figure 5: Effects on DI benefit payments and Earnings



Notes: This Figure plots the DiD estimates (black dots with 95% confidence interval) by year from regression (1) for Log disability benefits in Panel (a) and Log earnings in Panel (b). Standard errors are clustered at the census division level.

by 1.17 percentage points. More generous DI benefits can increase DI benefit receipt through two distinct channels. On the one hand, more generous DI benefits make it more attractive to enter DI (inflow effect). On the other hand, it becomes more attractive to stay longer on DI (outflow effect). Table 2 shows that the increase in DI benefit receipt is entirely driven by more DI inflow. We find no statistically significant or economically meaningful effect of higher DI benefits on DI outflow. The DI benefit receipt estimate implies a DI benefit receipt elasticity with respect to benefits of 0.647. This elasticity identifies the fiscal externality in a simple Bailey-Chetty style model of optimal DI. Put differently, our estimate implies that a one-dollar increase in DI benefits costs not just one but 1.67 dollars in total because of induced entry. To evaluate the welfare effects of the 1987 reform, we would need to compare this fiscal cost estimate against the insurance value of DI. Estimating the insurance value of DI is beyond the scope of this paper. Still, our DI elasticity estimate provides an important benchmark. The Baily-Chetty model of optimal social insurance benefits implies that higher DI benefits are optimal if one dollar in the hands of a DI recipient has a social value of more than 1.67 dollars, the total cost of providing this dollar to DI recipients.

Panel B. reports the average DiD estimates for a sample with a shorter time horizon. We compare the two years before the reform to the first three years after the reform, which is the period chosen by Gruber (2000) in his analysis of the 1987 reform. For this sample, we find smaller effects on DI take-up and non-employment consistent with Figure 4 which shows that the effects steadily grow over time.

Panel C. shows our DiD estimates when we further restrict the sample to men aged 45-49, as in Gruber (2000). We find smaller DI and non-employment effects in this sample compared to our “long sample.” We estimate an elasticity of non-employment with respect to DI benefits of 0.123. Gruber (2000) reports an elasticity between 0.28 and 0.36. Our elasticity is smaller.¹² There are several differences between Gruber (2000) and our approach that can explain the different elasticities. First, Gruber (2000) uses the Canadian Survey of Consumer Finances (SCF), which is an annual supplement to the nationally representative monthly Labor Force Survey and is conducted in April. This data is cross-sectional, and Gruber (2000) measures non-participation as a dummy indicating whether an individual was not working during the week of the SCF survey. We use the LAD and measure non-employment as zero earnings during the entire year in the tax data. Second, we control for province and year fixed effects while Gruber (2000) controls for a respondent’s age, marriage status, education level, and the number of children.

Heterogeneity. Who responds to more generous DI benefits and takes up DI because of the reform? To shed light on this question, we estimate the effects for different population subgroups. Figure 6 presents the DI benefit receipt estimates (left figure) and the non-employment estimates (right figure) for these different subgroups. First, we divide individuals into income quintiles based on their taxable income in 1986 and estimate the DI benefit receipt effect within these quintiles. Figure 6 shows that we find the strongest induced entry effect in the first income quintile with an increase of around

¹²Gruber (2000) does not report standard errors for his elasticity. Therefore, it is impossible to judge whether the difference in our estimates is statistically significant.

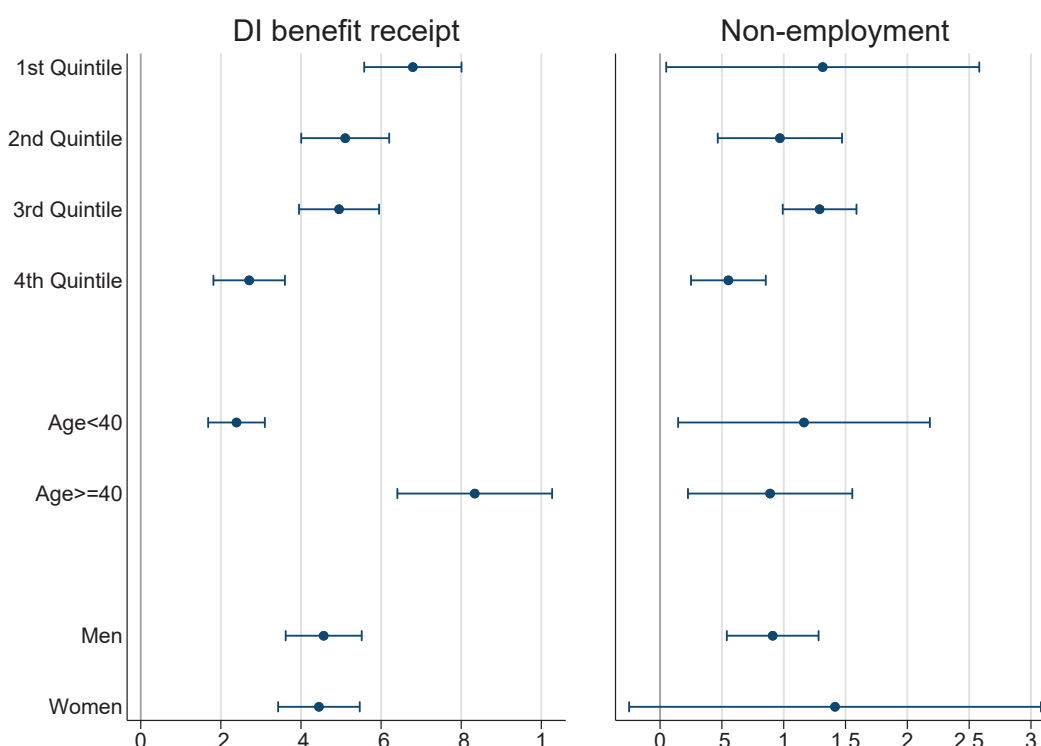
Table 2: Average Effects of 1987 Reform

	DI benefit receipt	Non-employment	Log DI benefits	Log earnings	DI inflow	DI outflow
<u>A. Long Sample (1982-1992)</u>						
Estimate	0.455*** (0.046)	1.17** (0.491)	0.036*** (0.004)	-0.076* (0.044)	0.125*** (0.009)	-0.003 (0.006)
Elasticity	0.674*** (0.068)	0.207** (0.087)				
<u>B. Short Sample (1985-1989)</u>						
Estimate	0.28*** (0.025)	0.62** (0.272)	0.023*** (0.002)	-0.044* (0.024)	0.098*** (0.009)	-0.006 (0.005)
Elasticity	0.456*** (0.04)	0.115** (0.05)				
<u>C. Gruber Sample (Men 45-59, 1985-1989)</u>						
Estimate	0.386*** (0.069)	0.466*** (0.159)	0.041*** (0.006)	-0.031 (0.019)	0.181*** (0.032)	-0.006 (0.007)
Elasticity	0.358*** (0.064)	0.123*** (0.042)				

Notes: This Table reports the average effect of the 1987 reform for different outcomes (the β -coefficient from equation (1)). Standard errors are reported in parentheses and are clustered at the Census division. Levels of significance: *1%, **5%, and ***1%.

0.7 percentage points (“1st Quintile”). Higher incomes show a significantly smaller DI take-up response. The DI benefit receipt estimate is in the highest income quintile is half as large as in the lowest income quintile. Second, we divide individuals into two subgroups according to their age in 1986. Individuals below the age of 40 show a smaller response compared to older individuals. This age gradient reflects that DI is more prevalent at older ages. Third, we split the sample population by gender and find that men and women show very similar DI take-up responses. For non-employment, we observe that the estimates are less precisely estimated and that the heterogeneity is less pronounced compared to DI benefit receipt.

Figure 6: Heterogeneity estimates DI benefit receipt



Note: This Figure presents the average DiD estimates for DI benefit receipt (left figure) and Non-employment (right figure) for different subgroups.

3.4 Non-employment versus induced DI entry

We find that more generous DI benefits increase non-employment more strongly than DI take-up. At first sight, this is surprising. We would expect that these two effects either go hand in hand since individuals leave employment and enter the DI program or that the DI take-up effect is larger than the non-employment effect as some individuals were already not employed before transitioning to DI benefits. Hence, what could explain that non-employment reacts more strongly than DI take-up? Individuals cannot simply claim DI benefits if they want to. They need to apply and pass a medical evaluation. In Canada, around 60 percent of initial applications are rejected, and individuals can only apply to DI if they are not employed. Hence, they need to leave the labor force before applying. As not all individuals who apply to DI are awarded DI benefits, such

a system implies that the non-employment effect can be larger than the DI effect. This effect would be particularly pronounced if individuals do not easily transition back to work once their DI application is rejected.

Unfortunately, Canadian DI application data are not available, and we cannot directly shed light on this potential mechanism. Therefore, we focus on a cross-country comparison, exploiting differential work requirements at the DI application stage. Similar to Canada, the U.S. has strict DI application requirements with respect to labor market participation: DI applicants are not allowed to work. By contrast, most European DI programs allow for more direct transitions from employment to DI. Hence, in the European context, we would expect that the non-employment effect is more in line with the DI take-up effect when DI generosity changes. To assess this hypothesis, we take estimates from the DI literature and compare the relative size of the non-employment and DI take-up responses to reforms. To measure the relative size across different reforms, we construct the non-employment to DI take-up ratio, the “ND-Ratio.” This ratio is the estimated non-employment effect if a DI policy parameter P changes relative to the corresponding effect on DI levels. A change in a policy parameter, dP , can be a change in benefit generosity or eligibility criteria, or other margins relevant for the DI application decision. Formally, the ND-Ratio is defined as

$$\text{ND-Ratio} \equiv \frac{-\frac{d\text{NonEmp}}{dP}}{\frac{d\text{DI}}{dP}}.$$

If the ND-Ratio is above one, the non-employment effect exceeds the DI effect. If it is below one, employment declines less strongly than DI take-up increases.

Figure 7 plots the calculated ND-Ratios based on existing literature for countries with strong and weak non-employment requirements at the DI application stage. Appendix Table A.1 explains how we construct the ND-Ratio from the literature and how we categorize weak and strong non-employment requirements. A striking pattern emerges from Figure 7. DI programs with strong non-employment requirements

exhibit ND-Ratios above one. On the other hand, programs with weaker requirements exhibit ND-Ratios below one. Almost all countries with weak requirements are European countries, and arguably there could be other differences driving the ND-Ratio differences. The comparison between the U.S. SSDI program and the U.S. Veteran Disability Compensation (DC) program enables us to alleviate this concern. The U.S. SSDI program features strong non-employment requirements and has an ND-Ratio of 1.33. In contrast, the U.S. Veteran DC program features no non-employment requirements and has a much lower ND-Ratio of 0.18. Applicants and recipients in the U.S. Veteran DC can work as much as they like without any consequences for their award chances or benefits. Of course, this evidence is only suggestive, and further research is needed to draw strong conclusions on the interaction between non-employment and work requirements at the application stage.

Figure 7: ND-Ratio from Literature



Notes: This Figure shows ND-Ratios for different countries constructed from estimates from the literature. Appendix Table A.1 explains in detail how we construct the ND-ratios. In short, U.S. SSDI is based on Autor and Duggan (2003), U.K. on Bell and Smith (2004), Netherlands on Borghans et al. (2014), Austria on Staubli (2011), U.S. Veteran Disability Compensation on Autor et al. (2016), and Switzerland on Müller and Boes (2020). The Canadian ND-Ratio can be directly calculated from Table 2 as $\text{ND-Ratio} = 1.17 / 0.455 = 2.57$.

4 Impact of Earnings Disregard

4.1 Estimation Strategy

In June 2001, the CPP-D introduced an annual earnings disregard, allowing beneficiaries to earn up to 3,800 CAD without having their benefits suspended. The purpose of this policy was to encourage work among CPP-D beneficiaries. We analyze the impact of this reform with a difference-in-differences bunching estimator closely following Cengiz et al. (2019). Our difference-in-differences bunching approach estimates the effect of the increase in the earnings disregard by earnings bins across the earnings distribution. In particular, we compare the earnings distribution pre- and post-reform in ROC (first difference) to the change in earnings distribution in the control province Quebec (second difference). This comparison captures the earnings response to the policy change at each point in the earnings distribution. We implement this strategy with the following regression

$$\frac{D_{pjt}}{N_{pt}} = \sum_{k=-6}^6 \alpha_k T_{pjt}^k + \mu_{pj} + \rho_{jt} + \varepsilon_{pjt}, \quad (3)$$

where D_{pjt} is the number of DI recipients with earnings in 250 CAD-bin j in province p in year t , and N_{pt} is the size of the population in province p in year t . The treatment dummy T_{pjt}^k equals one after the earnings disregard was implemented in ROC for the wage bins j that fall between k and $k + 1000$ Canadian Dollars relative to the earnings disregard.¹³ For instance, $k = 0$ denotes the earnings interval between the earnings disregard and the earnings disregard plus 999 CAD. Similarly, $k = -1$ represents the earnings interval between 1,000 CAD and 1 CAD below the earnings disregard. We also include province-by-bin fixed effects (μ_{pj}) to control for province-specific factors as well as bin-by-year (ρ_{pj}) fixed effects to control for nationwide changes in the earnings

¹³The earnings disregard went into effect on June 1, 2001. In our main specification, we choose to include 2001 as a post-reform year, but the results are similar when we treat 2001 as a pre-reform year.

distribution. We cluster standard errors at the bin-by-province level.

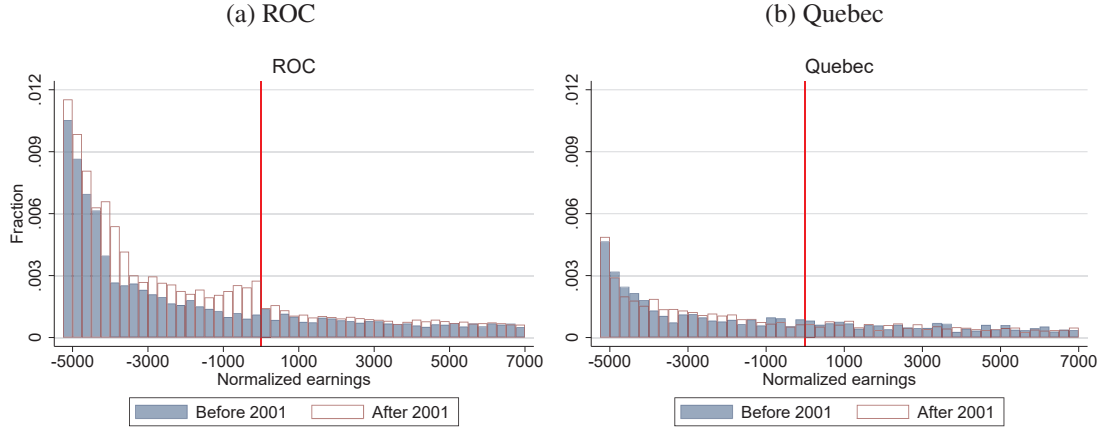
The main coefficients of interest are the α_k -coefficients, which capture the causal effect of the introduction of the earnings disregard on the share of individuals in earnings interval k . In our baseline specification, k runs from -6 to +6, where $k = -6$ is one for earnings smaller than 5,001 CAD below the earnings disregard, including no earnings at all, and $k = +6$ is one for earnings levels greater than 6,000 CAD above the earnings disregard. Note that this definition implies that α_{-6} captures any labor force participation response that the policy might have.

4.2 Descriptive Evidence

A standard static labor supply model predicts two main effects when the earnings disregard increases. First, DI recipients might work more and increase their earnings. More specifically, we expect to see “bunching” at the new earnings disregard because DI recipients have a strong incentive to keep their earnings below the disregard not to risk losing their benefits. Second, a higher earnings disregard makes DI financially more attractive for individuals who are not yet receiving DI benefits: they can receive DI benefits and work to some extent. Therefore, a higher earnings disregard might also induce entry into DI. A dynamic labor supply model might feature a third effect: A higher earnings disregard could lead to higher DI outflow if the opportunity to work a little is a stepping stone for DI recipients to transition back to the regular labor market.

We quantify these three effects with our difference-in-differences bunching approach in the next section. Here, we look at the raw earnings distributions before and after the reform. Figure 8(a) plots the earnings distribution of DI recipients in ROC before the reform (years 1998-2000) in filled blue bars and after the reform in empty red bars (years 2001-2006). Before the reform, the earnings distribution is smooth around the earnings disregard and generally declining as earnings rise. After the reform, we observe more mass below the earnings disregard, indicated by the vertical red line.

Figure 8: Bunching at Earnings Disregard in ROC and Quebec



Notes: This Figure plots the earnings distribution of DI recipients in ROC (Panel (a)) and Quebec (Panel(b)) before the reform (years 1998-2000) in filled blue bars and after the reform in empty red bars (years 2001-2006). The red vertical line indicates the earnings disregard in ROC, which the 2001 reform introduced. In ROC, a clear increase in earnings below the disregard is visible with bunching at the disregard.

Hence, there is a positive earnings response to the reform, and bunching (excess mass) at the earnings disregard is clearly visible. Above the earnings disregard, there is little difference before and after the reform. Figure 8(b) plots the earnings distribution for the unaffected region, Quebec, before and after 2001. In Quebec, the two earnings distributions look very similar before and after the reform, strongly suggesting that the observed change in the earnings distribution in ROC is caused by the 2001 reform.

4.3 Empirical Results

Figure 9 plots the α_k -coefficients from regression (3) for the full sample and shows that the share of DI recipients with earnings at the new earnings disregard increases significantly. Hence, the introduction of the earnings disregard induces DI recipients to work more. We also observe positive estimates in the bins below the earnings disregard, indicating that the share of DI recipients with positive earnings increases overall. There is also an increase in the share of DI recipients in the lowest bin that includes zero earn-

ings. On the other hand, the estimates are small and insignificant for the earnings bins above the disregard. The positive estimates below the earnings disregard can emerge for two reasons. First, individuals who are already receiving DI benefits before the reform increase their earnings. Second, the higher earnings disregard induces more individuals to enter DI and earn below the earnings disregard. If the effects were only driven by existing DI recipients increasing their earnings, we would see a reduction in mass in some bins. However, we do not find any reduction in mass (negative estimates) at any bin, suggesting that the additional mass is driven by an induced entry effect.

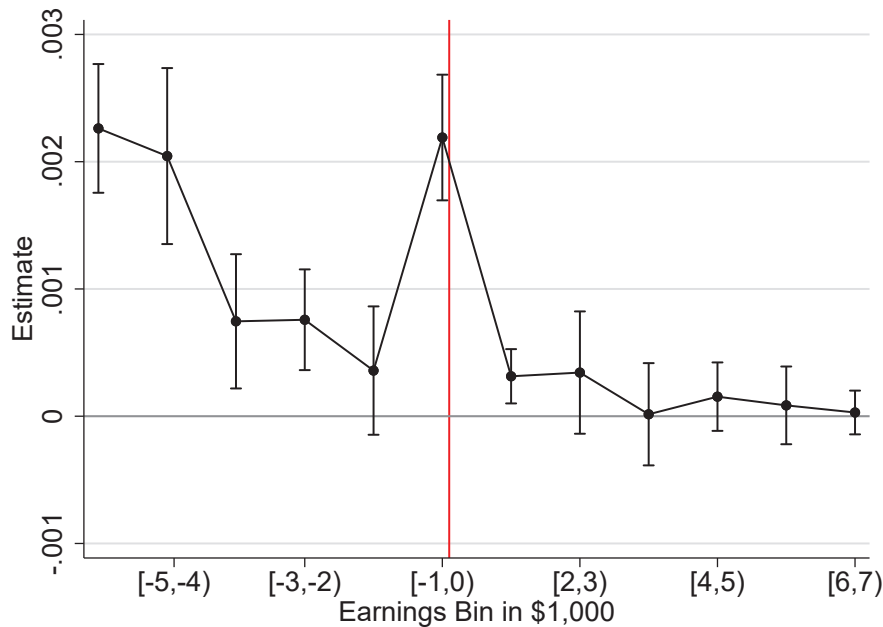
To disentangle the labor supply effect of existing DI recipients from new DI recipients who enter after the reform, we estimate regression (3) for individuals who were already on DI before the reform. Figure 10 plots the estimates for this sample. There is a small but significant increase in the bin at the earnings disregard and a much larger but noisily estimated reduction in the lowest bin with zero earnings. This pattern suggests that the policy indeed induced some DI recipients to start working and move from no participation to the earnings disregard. It could also point to an outflow effect, as the reduction in the lowest bin is larger than the increase at the earnings disregard.

To shed further light on the entry, outflow, and labor supply effects, we sum up the estimates for the different groups. Table 3 Panel A. sums up the separate bin estimates below the disregard for the full sample, pre-reform DI recipients, and awardees after 2001. The overall share of DI recipients with earnings below the earnings disregard increases by 0.0084 percentage points. Hence, the policy reform increased the share of DI recipients overall. However, the magnitude of the effect is small. A 0.0084 percentage point increase in DI levels corresponds to a 0.005 percent increase. This small increase in DI levels is driven by an increase in new awardees (0.0148 percentage points) and a very small and statistically insignificant outflow effect of previous DI recipients (-0.0065 percentage points). Panel B. sums all bin estimates, including those above the disregard, and shows that the effects are very similar to the effects below the disregard. This is reassuring; it implies that the effect is driven by the distribution below

the earnings disregard as one would expect.

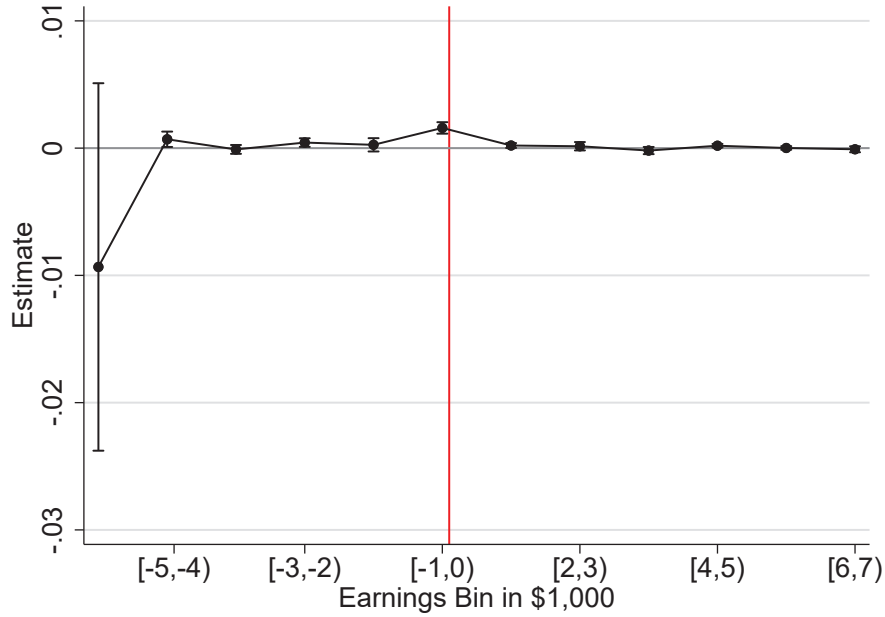
While the entry effect is statistically significant, the effect is small economically. Consequently, the higher earnings disregard in the Canadian program only has minimal effects on DI program expenditures. In particular, relaxed earnings restrictions for DI recipients are unlikely to reduce program costs, given that we do not find a sizable outflow effect of the policy. From an optimal policy point of view, it is unclear whether relaxing earnings restrictions is desirable. On the one hand, a few DI recipients who can work more profit from the policy and are better off. On the other hand, the small inflow effect increases program costs. How one weighs these two effects determines whether relaxing earnings restrictions are welfare improving or not. However, the effects of a higher earnings disregard are so small that increasing the earnings disregard in the way it was done in the 2001 reform does not play an important role in designing DI programs.

Figure 9: Bunching Estimates – Full sample



Note: This Figure plots the α_k -coefficients from regression (3) for the full sample and shows that the share of DI recipients with earnings at the new earnings disregard significantly increases.

Figure 10: Bunching Estimates – On DI pre 2001



Note: This Figure plots the α_k -coefficients from regression (3) for the sample of individuals who were already on DI benefits before the reform.

Table 3: Estimates

	Full Sample	Recipients pre 2001	Awardees post 2001
<u>A. Below Disregard</u>			
Estimate	0.0084*** (0.0007)	-0.0065 (0.0074)	0.0148** (0.0073)
<u>B. Total Effect</u>			
Estimate	0.0093*** (0.0008)	-0.0062 (0.0074)	0.0154** (0.0073)

Notes: This Table presents the aggregate effects among the earnings distribution. Panel A. sums up the separate bin estimates below the disregard for the full sample (pre-reform DI recipients and awardees after 2001). Panel B. sums all bin estimates (also above the disregard). ***, **, * denotes statistical significance at the 1%, 5%, and 10% level, respectively.

5 Conclusions

In this paper, we exploit two Canadian reforms that increased DI benefit generosity and the earnings disregard. We find that higher DI benefits induce significant entry

into DI. We estimate a DI benefit receipt elasticity with respect to DI benefits of 0.674. By contrast, a higher earnings disregard has only negligible effects on DI entry. We estimate that a 3,800 CAD increase in the annual earnings disregard increases DI levels by 0.0084 percentage points. We find that DI recipients respond to the labor supply incentives of the earnings disregard—there is sharp bunching at the earnings disregard threshold—but the overall labor supply response of DI recipients is small in magnitude. Lastly, we find no evidence for a meaningful outflow effect of more relaxed earnings restrictions. Hence, a higher earnings disregard does not lower DI expenditures in our setup.

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A Additional Tables and Figures

Table A.1: Construction of ND-Ratio

Country / Paper	Construction of ND-Ratio	ND-Ratio	Non-Participation Requirement
U.S. SSDI / Autor and Duggan (2003)	Table IV reports the effect of an increase in DI rolls on labor force participation and hence provides a direct estimate of the ND-Ratio. Table IV differentiates the effect by years (78-84 vs. 84-98) and by high school completion. We take the IV estimates and reweight them to construct an overall estimate. HS dropouts account for 7.9 percent of the labor force (footnote 24) and we put an equal weight of 0.5 for both periods. Hence, our aggregate effect is calculated as follows from Table IV: ND-Ratio = $10 \cdot 0.5 \cdot (0.079 \cdot 1.35 + (1 - 0.079) \cdot (0.2)) + 10 \cdot (0.079 \cdot (0.51) + (1 - 0.079) \cdot (-0.07)) = 1.33$. The rescaling by factor 10 is necessary because of scaling in the regression in Table IV (see page 24).	1.33	Earnings must be below the SGA to be eligible for SSDI benefits
U.S. Veteran / Autor et al. (2016)	Table 8, Panel A directly reports the effect of DI pension to labor force participation, which is equivalent to -ND-Ratio.	0.18	No restrictions with respect to work.
Austria / Staubli (2011)	Table 4, Panel A: Disability effect -7.44 to -4.30 across different specifications. Table 4, Panel D: Employment effect 3.37 to 1.04. Calculate Ratio as -Employment effect/DI effect. Different specifications imply ND-ratios from 0.27 to 0.44 with an average of 0.34.	0.34	Can work and apply, reduction in benefits if working above 440 Euro per month (see Ruh and Staubli (2019))
Netherlands / Borghans et al. (2014)	Figure 1, Panel C reports a DI effect of -0.038. Figure 5, Panel B reports an employment effect of 0.029. ND-ratio then calculated as -Employment effect/DI effect.	0.76	Partial DI system where recipients are allowed to work when receiving DI benefits.
Switzerland / Müller and Boes (2020)	Table 6 reports the effect of DI benefit receipt on the probability to be out of labor force. The paper finds an effect of 0.04, which directly corresponds to the ND-Ratio	0.04	Partial DI system.
U.K. / Bell and Smith (2004)	Table E shows the estimated non labor force participation and DI level elasticities. From these elasticities we can reconstruct the ND-Ratio. The elasticities are given by $\varepsilon_{DI} = \frac{\partial DI}{\partial b} \cdot \frac{b}{DI} = 0.258$, $\varepsilon_{NP} = \frac{\partial NP}{\partial b} \cdot \frac{b}{NP} = 0.451$ and hence ND-Ratio = $\frac{\varepsilon_{NP} \cdot NP}{\varepsilon_{DI} \cdot DI}$. We take $NP = 0.087$ (Figure 2) and $DI = 0.05$ (Figure 3).	1.00	