Riding Out the Downturn: The Re-Employment Effects of an Unemployment Insurance Extension

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Abstract

Do more generous unemployment benefits help workers find better jobs? I study a temporary extension of potential benefit durations in Canada's Employment Insurance program introduced during an economic downturn. Using linked administrative data and a fuzzy regression discontinuity design based on a payroll tax threshold, I show that longer entitlements extend benefit receipt but improve re-employment outcomes: earnings rise, return-to-industry rates increase, and self-employment declines. Both before- and after-tax incomes grow, while Social Assistance use falls. Combining these effects reveals that the short-run cost of extended benefits is largely—sometimes fully—offset by longer-run tax revenues. The results highlight the importance of incorporating longer time horizons when assessing the fiscal externalities of unemployment insurance.

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1 Introduction

Does unemployment insurance (UI) help workers find better jobs? The answer to this question matters for how we assess the fiscal incidence and welfare consequences of UI. In classic analyses of UI policy (Baily, 1978, Chetty, 2006), the social planner weighs the welfare gains from consumption smoothing against the fiscal externality arising from longer unemployment spells, including higher benefit payments and forgone tax revenues. If UI also affects the quality of post-unemployment matches, however, the fiscal consequences depend not only on what occurs during the unemployment spell but also on outcomes after re-employment. Higher-quality job matches that yield greater earnings generate additional tax revenues, offsetting part of the program cost. Conversely, if extended benefits reduce subsequent earnings, the resulting decline in tax revenues increases the fiscal burden. Understanding these long-run effects is therefore essential for evaluating the overall fiscal incidence of UI.

Theory offers mixed predictions about how UI affects post-unemployment outcomes. More generous benefits, whether through higher replacement rates or longer entitlements, may allow unemployed workers to be more selective in the jobs they apply for or accept, and this greater selectivity can lead to better matches and higher earnings upon re-employment. At the same time, longer unemployment spells can entail skill depreciation or stigmatization by employers, reducing re-employment prospects and wages. UI may also influence participation in other income support programs, such as Social Assistance or public pensions, either by serving as a substitute or by altering the timing and level of income from work. Ultimately, the sign and magnitude of these effects are empirical questions, the answers to which are necessary for assessing the net fiscal cost of unemployment insurance.

This paper examines the effects of more generous UI benefits on post-unemployment outcomes using quasi-experimental evidence from Canada. I exploit a one-time extension of potential benefit duration (PBD) introduced in response to a regional economic downturn. The analysis compares unemployed workers whose payroll tax contributions placed them just above or below an eligibility cutoff that granted up to 20 additional weeks of benefits.

Using UI administrative records linked to tax filings, I follow individuals for up to five years after job loss to capture both immediate and longer-term impacts on employment, income, and program participation.

The results show that extended benefits significantly affect workers' subsequent earnings and labor-market trajectories. Consistent with prior research, longer PBDs increase the duration of benefit receipt and unemployment spells (Krueger and Meyer, 2002, Schmieder et al., 2016, Lopes, 2022, Cohen and Ganong, 2024). Beyond these short-run effects, I find that additional weeks of entitlement raise re-employment earnings by 15 to 16 percent during the first two years following job loss. Treated workers are also substantially more likely to return to their pre-displacement industry, and these higher industry-match rates persist throughout the post-treatment period. These earnings and industry results suggest that extended benefits improve the quality of job matches rather than merely delay re-employment.

I also find that workers eligible for extended benefits are less likely to turn to self-employment as a way of offsetting lost income. This is a novel result, as self-employment has received little attention in empirical evaluations of UI; UI programs typically exclude the self-employed from coverage, making it difficult to observe this margin when relying on social insurance administrative data. My findings show that while self-employment can help cushion earnings losses after displacement, more generous UI reduces both the likelihood of entering self-employment and the level of self-employment income.

Thanks to the linkage with tax records, I can directly observe changes in government revenues through taxes paid and transfers received. This allows me to estimate the net fiscal cost of the UI extension, including both UI expenditures and downstream fiscal effects. In some cases, the higher short-term benefit payments are partly or fully offset by increased tax revenues over subsequent years. I also examine substitution across transfer programs, testing whether extended UI recipients are less likely to draw on other benefits such as Social Assistance, disability or retirement pensions, and workers' compensation. These interactions within the broader tax-and-transfer system are crucial for understanding the fiscal incidence of UI and, ultimately, for informing its optimal design (Inderbitzin et al., 2016, Mueller et al., 2016, Leung and O'Leary, 2020, Lawson, 2017).

This study builds on a large literature examining the short- and long-term effects of unemployment insurance. Evidence on re-employment outcomes remains mixed. Several studies report imprecise or null effects on re-employment wages (Card et al., 2007, Lalive, 2007, Van Ours and Vodopivec, 2008, Johnston and Mas, 2018). Among those finding significant effects, results diverge: Schmieder et al. (2016) estimate that an additional month of benefits in Germany reduces re-employment wages by 0.1 percent, whereas Nekoei and Weber (2017) find that a nine-week extension in Austria increases wages by 0.5 percent, and Ahammer and Packham (2023) report 2 percent gains for women facing a similar extension. Other studies have examined outcomes such as job tenure, occupation, or hours worked, with similarly mixed conclusions (Belzil, 2001, Centeno, 2004, Schmieder et al., 2012a, Le Barbanchon, 2016). In contrast to earlier findings of small or insignificant effects, recent work from the United States finds much larger earnings impacts of gaining UI eligibility, ranging from 10 to 50 percent (Chao et al., 2024, McQuillan and Moore, 2025).

This paper also relates to research on early-career shocks and earnings dynamics. The first two decades of a career are the period during which most life-cycle earnings growth occurs, and matching with the right firm, occupation, and industry plays a central role in shaping that growth (Topel and Ward, 1992, Guvenen et al., 2022). Studies of labor-market entry during recessions show persistent earnings scarring (Kahn, 2010, Oreopoulos et al., 2012, Von Wachter, 2020), and job loss early in one's career can lead to lasting income declines (Jacobson et al., 1993, Kletzer and Fairlie, 2003, Morissette et al., 2013, Birinci et al., 2023). The policy intervention studied here disproportionately affected early-career workers, and the results suggest that such temporary extensions can mitigate some of the long-term losses associated with displacement.

Finally, this paper contributes to the literature evaluating the optimality of existing UI systems. Much of this work applies sufficient-statistics methods to assess the welfare implications of benefit levels and durations, using behavioral elasticities as key inputs (Chetty, 2009). The validity of these approaches depends on whether the estimated elasticities capture all relevant behavioral margins (Kleven, 2021). My findings demonstrate that UI affects multiple dimensions—unemployment duration, re-employment earnings, self-employment,

and substitution across programs—that are rarely observed jointly. They also highlight the value of using comprehensive administrative data to estimate the net fiscal cost of UI directly, rather than inferring it indirectly from behavioral elasticities (Lee et al., 2021).

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 details the data and empirical strategy. Section 4 presents the results, and Section 5 concludes.

2 Institutional Background

Unemployment insurance in Canada is administered federally through the Employment Insurance (EI) regular benefits program.[1] Weeks of benefit entitlement depend on both prior hours worked and the regional unemployment rate. Benefit levels replace 55 percent of average weekly earnings up to a maximum; in regions with higher local unemployment rates, the average is calculated using a smaller number of a worker's highest-earning weeks in the year preceding job loss.

Canada is divided into 62 EI regions. Each region's official unemployment rate, updated monthly as a seasonally adjusted three-month moving average from the Labour Force Survey. Figure 1 depicts how PBD varies with hours worked in the qualifying period and regional unemployment rates.[2]

Although these parameters adjust automatically to local conditions, the federal government has occasionally implemented discretionary extensions. One such intervention followed the sharp collapse in global oil prices between mid-2014 and early 2016—a 70 percent decline driven by surging U.S. shale production and OPEC's decision not to cut output (Stocker et al., 2018). Because Canada's oil and gas sector is concentrated around the Western Canadian Sedimentary Basin and off the coast of Newfoundand, the shock led to widespread layoffs and sharply rising regional unemployment.

[1] Hereafter, UI and EI are used interchangeably.

[2] The qualifying period normally covers the 52 weeks preceding a claim but may be extended up to 104 weeks if the worker was on leave, or shortened if a claim was filed in the prior year.

In March 2016, the federal government announced temporary EI extensions for 12 regions most affected by the downturn. Regions qualified if, between July 2015 and March 2016, their unemployment rate exceeded by at least two percentage points the minimum rate recorded between December 2014 and February 2015, and had not subsequently fallen back to within one point of that minimum. In May 2016, three additional regions were added based on meeting the same criteria in the intervening months.[3]

The extensions, effective July 3 2016, applied retroactively to claimants who had filed since January 4 2015 and remained unemployed, and prospectively to all new claimants. All claimants in the selected regions received at least five additional weeks of benefits. Those designated as long-tenured workers based on their history of prior EI receipt and payroll tax contributions were eligible for larger extensions: initially up to 25 weeks, then gradually reduced to 17 weeks (October 2016–February 2017) and 10 weeks (February–July 2017) before expiring in July 2017. The difference in additional entitlement between long-tenured and other claimants forms the basis of my empirical design.

3 Data and Empirical Strategy

3.1 Data Sources

Administrative records serve as the primary data source for this study. The Employment Insurance Status Vector (EISV) administrative dataset encompasses all EI claims from 1997 to 2018. It includes information on claimants' occupation, residential location, industry of employment, age, reason for separation, hours worked, and earnings during the qualifying period. The EISV also provides details on claimants' weeks of entitlement, weeks of benefits used, and total benefits received. Finally, it indicates whether a claimant is classified as a long-tenured worker.

[3]The 15 regions comprised Newfoundland and Labrador, Sudbury, Northern Ontario, Northern Manitoba, Saskatoon, Southern and Northern Saskatchewan, Calgary, Edmonton, Northern and Southern Alberta, the Southern Interior and Northern British Columbia, Whitehorse, and Nunavut.

The primary approach I use to analyze the effect of the PBD extension compares long-tenured workers with other EI claimants in the 15 regions that received the extension. The federal government classifies EI claimants into three groups: long-tenured workers, frequent claimants, and occasional claimants. Long-tenured workers are long-time net contributors to the EI Operating Account, having paid payroll taxes over multiple years while receiving relatively few weeks of unemployment benefits. Frequent claimants are individuals who have collected 60 or more weeks of benefits and have filed at least three separate unemployment claims in the past five years—typically seasonal workers who face recurring unemployment in the off-season. Occasional claimants constitute the residual group and include all claimants who do not meet the criteria for the other two categories.

Table 1 presents summary statistics for EI claimants residing in the regions selected for the PBD extension. Columns 1 and 2 report statistics from the EISV for all long-tenured workers and for occasional claimants, respectively.[4] Column 3 shows the differences between long-tenured workers and occasional claimants.

The comparison between Columns 1 and 2 in Table 1 reveals statistically significant and meaningful differences between long-tenured workers and occasional claimants in the 15 selected regions. Long-tenured workers are, on average, 8.7 years older, earn $2,210 more in insurable earnings, and have worked an additional 191 hours prior to job loss. They are also five percentage points more likely to be male.

Given that long-tenured workers, by definition, exhibit stronger attachment to the labor market, it is unsurprising that they have higher pre-layoff earnings and are, on average, older. A naive comparison between long-tenured and other claimants would therefore confound the effect of extended benefits with pre-existing differences in worker characteristics. To address this concern, I exploit the legislated definition of a long-tenured worker to identify otherwise comparable EI claimants, some of whom happen to qualify for extended benefit entitlements solely because they narrowly satisfy a payroll tax contribution requirement.

To be considered a long-tenured worker, an EI claimant needs to meet the following

[4]Frequent claimants are excluded because they were not at the margin of long-tenured worker designation. The relevant comparison is between long-tenured workers and occasional claimants.

criteria: 1) the claimant has to have paid at least 30% of the maximum annual EI payroll tax in at least 7 of the 10 calendar years preceding the initiation of their unemployment claim; 2) the claimant has to have received fewer than 36 weeks of unemployment benefits in the 560-week (i.e., 5-year) period leading up to their current EI claim's start date. Both the 30%-for-seven-or-more-years and the fewer-than-36-weeks criteria potentially lend themselves to the use of regression discontinuity designs comparing the outcomes of claimants who fall just above and below these thresholds; these comparisons produce estimates of the impact of becoming entitled to receive extra weeks of unemployment benefits.

The EISV does not, however, include information on the payroll taxes levied on both employees and employers to fund EI.[5] The determination of payroll tax contributions is instead enabled through a linkage between the EISV and the T1 personal income tax file, conducted under the auspices of Statistics Canada's Education and Labour Market Longitudinal Platform (ELMLP).

To be included in the ELMLP, individuals must appear in one of its two core datasets: the Postsecondary Student Information System (PSIS), which records data on full- and part-time students at all Canadian universities and community colleges from 2009 onward (and, for some provinces, as early as 2004), or the Registered Apprenticeship Information System (RAIS), which tracks all participants in apprenticeship programs across Canada from 2008 onward. Both datasets are linked to T1 personal income tax files for the period 1993–2021.[6]

Although the ELMLP does not include all Canadian workers, it disproportionately features those at the margin of long-tenured worker designation based on their payroll tax contribution history. This can be shown using the Longitudinal Administrative Databank (LAD), a 20% sample of *all* Canadian taxfilers. Among taxfilers in the LAD who contributed at least 30% of the maximum annual EI premium through payroll taxes in six to seven of the 10 years preceding their unemployment spell—and who reported no unemployment benefits in the five preceding years—approximately half are also included in the ELMLP.

[5]The tax rate and maximum amount vary annually based on the need to replenish the EI operating fund. All firms pay the same tax rate, as there is no experience rating based on past layoffs.

[6]To account for the possibility that postsecondary enrollment or apprenticeship registration is influenced by entitlement to unemployment benefits (Barr and Turner, 2015), I restrict my sample to individuals who appear in the ELMLP before their unemployment spell began.

However, the LAD alone is insufficient for this study, as it lacks a direct link to the EISV for tax filers not included in the ELMLP.[7]

Column 4 provides summary statistics for EI claimants who are in the ELMLP database and who have met the 30% of the annual payroll tax contribution requirement in at least 6 but not more than 7 of the past 10 years while having collected fewer than 36 weeks of benefits in past unemployment spells; this group of EI claimants comprises the set of all individuals who are at the margin of long-tenured worker designation, and who are the focus of the analysis that follows. Approximately half of this group is designated a long-tenured worker; the average age is 34.1, which is less than the average ages for all of the long-tenured workers or all occasional claimants. Given that the ELMLP specifically focuses on individuals who had recently attended a university or community college (either as a full-time, part-time, or casual student) or who enrolled in a registered apprentice program, the younger skew of the average age is unsurprising.

3.2 Empirical Design

As noted above, the cutoffs based on payroll tax contributions or the number of prior weeks of unemployment benefits received could, in principle, lend themselves to a regression discontinuity design, provided that potential outcomes are smoothly distributed across each threshold so that any discontinuity in observed outcomes corresponds to the treatment effect of the extra benefit entitlement.

In practice, however, only the payroll tax cutoff provides a valid source of identification for a regression discontinuity design. The prior benefit weeks cutoff was chosen to be the same as the entitlement for full-time workers in low-unemployment regions. As a result, there is a pronounced excess mass of claimants who received exactly 36 weeks of benefits in the 560 weeks preceding their most recent unemployment spell; these are primarily full-time workers in low-unemployment rate regions who exhausted their entitlement. These

[7] Even if a direct linkage between the LAD and the EISV did exist, it would result in a smaller sample size than the ELMLP linkage because the LAD is only a 20% sample of all taxfilers.

claimants on the ineligible side of the cutoff are observably different from claimants .[8]

In using the 30% payroll tax contribution threshold for the purposes of a regression discontinuity, it is important to clearly define the group affected by this cutoff. For claimants who already satisfy the prior benefit weeks requirement, *and* who have paid payroll taxes equivalent to 30% or more of the annual maximum amount for at least six but not more than seven of the 10 years preceding their layoff, long-tenured worker status depends on the seventh-highest share of payroll taxes paid relative to the maximum. This value is hereafter referred to as the "focal payroll tax share." Claimants who have paid 30% or more in fewer than six years or in eight or more years are excluded from the analysis, as their seventh-highest share does not affect whether they are designated long-tenured workers; they are not at the margin.

Were it possible to access the exact information used by government administrators to assign long-tenured worker status, the focal payroll tax share could serve as the running variable in a standard regression discontinuity design: below the 30% threshold, no EI claimant would be classified as a long-tenured worker; above it, every claimant would be. However, the data available to me do not perfectly match those used by the government's computer pay system at the precise instant long-tenured worker designation is assigned.

There are two reasons why the observed running variable does not precisely match the data used to determine long-tenured worker status.

First, EI payroll taxes are not directly recorded in the T1 personal income tax filings available through the ELMLP. Instead, they must be estimated by applying the EI payroll tax rate for a given year to the T4 employment earnings reported for that year. For ELMLP observations that also appear in the LAD (which *does* include payroll taxes paid), I can assess the

[8]This bunching on the ineligible side of the threshold does not reflect strategic manipulation of prior benefit receipt. Nonetheless, claimants just below and just above the 36-week threshold differ systematically in predetermined characteristics. Specifically, individuals who received exactly 36 weeks in the past are (i) more likely to be full-time workers, (ii) more likely to reside in lower-unemployment regions, and (iii) more likely to have previously exhausted their entitlement in a past unemployment spell. These compositional differences confound any attempt to identify the causal effect of extended benefit durations by comparing claimants around the 36-week cutoff. In ongoing work, I attempt to overcome this issue by excluding individuals with a prior claim that had exactly 36 weeks of entitlement and focusing instead on individuals who had prior claims with more than 36 weeks of entitlement in total.

accuracy of these estimates by comparing them to the EI payroll tax contributions reported by tax filers; approximately 95% of values match. Discrepancies may arise because the LAD amount can include overpayments, in which case the amount I calculate may in fact be the correct one.[9] Another potential source of discrepancy is income earned in Quebec by individuals who are not residents of the province; since 2006, Quebec has had a distinct EI payroll tax rate and annual ceiling. In such instances, I would incorrectly calculate the payroll tax paid.

Second, even if exact payroll tax contributions were available in the ELMLP rather than calculated, they would not necessarily align with the data used by the computer pay system. Research-available tax records encompass all personal income tax filings for a given calendar year, but not all tax returns are filed and processed by the Canada Revenue Agency at the time an EI claim is initiated. Consequently, the payroll tax share calculated using research-available tax records equals the payroll tax share used by the computer pay system plus an error term.

Measurement error in observed running variables can create challenges for regression discontinuity designs since such errors "smooth out" the observed discontinuity in treatment around the threshold (Dong and Kolesár, 2023). However, when measurement error has a point mass at zero (i.e., when the observed running variable equals the true running variable for some positive mass of observations), it remains possible to estimate a regression discontinuity using the observed running variable in place of the true one (Battistin et al., 2009, Lee, 2017).

Panel A of Figure 6 illustrates how long-tenured worker designation changes around the 30% threshold for workers at the margin of eligibility. As shown, below the threshold approximately 20% of workers are designated as long-tenured workers; above it, the share jumps to about 80%, a 60 percentage point increase.[10]

[9]Such overpayments can occur when payroll taxes are deducted by multiple employers, resulting in a tax credit for the individual at the time the T1 tax form is filed.

[10]I use three different sets of calendar years to calculate potential focal payroll tax shares: 2005–2014, 2006–2015, and 2007–2016. If tax records were filed and processed instantly, the first set of years would determine long-tenured worker designations for claims starting in 2015, the second set for claims in 2016, and the third set for claims in 2017. In practice, due to delays in tax filing and processing, I select the set of years that maximizes the first-stage increase in the probability of long-tenured worker designation on a week-by-week

The figure confirms the presence of measurement error: some claimants below the 30% cut-off are designated as long-tenured workers, while some above the cut-off are not. Additionally, the figure shows that the measurement error must have a point mass at zero, since a discontinuity is still apparent at the threshold. Despite some attenuation, the jump in the share of claimants designated as long-tenured workers means that the observed focal payroll tax share can serve as a running variable in a fuzzy regression discontinuity design.

To implement this design, the following regression specifications are estimated:

$$LTW_i = \alpha_0 + \alpha_1 \mathbb{1}\Big\{FocalShare_i \geq 0.3\Big\} + f(FocalShare_i) + \Omega X_i + \epsilon_{it} \tag{1}$$

$$Y_{it} = \delta_0 + \delta_1 \mathbb{1}\Big\{FocalShare_i \geq 0.3\Big\} + g(FocalShare_i) + \Lambda X_i + \nu_{it} \tag{2}$$

Equation 1 represents the first stage, where LTW_i is an indicator for long-tenured worker designation. The running variable, $FocalShare_i$, denotes the focal payroll tax share described previously. This first stage estimates the relationship between the running variable and the probability of being designated as a long-tenured worker, which determines eligibility for additional weeks of extended benefits.

Equation 2 is the reduced form, where Y_{it} represents the outcome of interest for individual i at time t. In both equations, covariates X_i are included to improve the precision of the estimates. These covariates are observed at baseline—the time when the EI claim is first established. In the main tables, X_i includes an individual's insurable earnings and hours worked during the qualifying period, their standard entitlement in weeks, and their age. Appendix tables report results without controls and with alternative combinations of controls. The parameters of interest in equations 1 and 2 are α_1 and δ_1, respectively, which represent the change in the intercept associated with crossing the 30% focal payroll tax share threshold. The fuzzy RD estimate is obtained by taking the ratio of these two parameters:

basis. Ultimately, I use the focal payroll tax share from the 2005–2014 tax years for claims starting in 2015 through mid-2016, after which the 2006–2015 tax years are used.

$$\tau = \frac{\delta_1}{\alpha_1}$$

where τ represents the local average treatment effect. Specifically, τ captures the causal effect of receiving additional weeks of unemployment benefits for individuals designated as long-tenured workers because their observed focal tax share crosses the 30% threshold—i.e., the *compliers*.

In addition to the compliers, there are *always-takers*—individuals who receive long-tenured worker designation even if their focal payroll tax share is below 30%—and *never-takers*, who never receive the designation regardless of their payroll tax share. I assume that no individual becomes less likely to be designated as a long-tenured worker upon crossing the 30% threshold (i.e., there are no *defiers*).

The presence of compliers, always-takers, and never-takers in the dataset results from measurement error rather than arising from individuals' decisions about whether to accept an offer of treatment. Furthermore, if it is the case that the measurement error is non-differential (i.e., it is independent of the potential outcomes conditional on the true value of the running variable), then the fuzzy RD estimate is equal to the average treatment effect at the margin rather than the local average treatment effect for compliers at the margin (Lee, 2017).

Equations 1 and 2 are estimated using the data-driven bandwidth selection approach of Calonico et al. (2014), employing a first-order polynomial with triangular weights to linearly approximate the underlying continuous potential outcome functions near the threshold. All standard errors reported in this study's tables are robust and bias-corrected.

3.3 Validity of the Fuzzy RD Design

The validity of the fuzzy regression discontinuity design depends on the assumption that treatment status is not subject to manipulation around the threshold. If individuals or employers could strategically adjust the running variable to gain eligibility for extended unem-

ployment benefits, the identifying assumption that observations are as-good-as-randomly assigned around the threshold would be invalidated.

This concern is particularly relevant in the EI system, where there is evidence of manipulation around thresholds such as the minimum hours of work required to establish eligibility to receive *any* EI benefits.[11] If similar manipulation occurred around the focal payroll tax share threshold, it could bias the treatment effect estimates.

Figure 5 presents a histogram of the focal payroll tax share distribution.[12] The distribution is centered around the 30% threshold, with no apparent excess mass on the eligible side of the cutoff. Finally, I test for whether the density of claimants around the cutoff is smooth; the test fails to reject the null hypothesis of continuity (p-value: 0.809).

The absence of detectable manipulation is perhaps unsurprising, as the focal payroll tax share for each EI claimant is based on earnings that are also reported by employers in separate taxfilings. Furthermore, since this value is determined in a calendar year preceding the date an EI claim is submitted, individuals would be hard-pressed to retroactively adjust it to gain access to additional benefits.

Panel A of Table 2 examines whether any discontinuities exist in predetermined covariates around the 30% threshold for the focal payroll tax share. Reassuringly, there are no statistically significant differences in claimants' age, the share who are male, the number of prior EI claims, hours worked or earnings in the qualifying period, or the weekly benefit amount.

To interpret the ratio of the reduced form RD estimate to the first stage RD estimate as a local average treatment effect, the running variable (the focal payroll tax share) must affect outcomes of interest solely by increasing the number of weeks of entitlement associated

[11] An analysis of the distribution of insurable hours in the EISV dataset shows clear evidence of manipulation to meet the minimum hours threshold, particularly among seasonal workers who repeatedly claim benefits in the off-season. Strategic responses to EI eligibility criteria related to time worked prior to a layoff have also been documented by Christofides and McKenna (1996), Green and Riddell (1997), Baker and Rea (1998), and Friesen (2002).

[12] According to official government reports, the 30% threshold used in the definition of a long-tenured worker was chosen because it corresponded to the average annual payroll taxes paid by workers with 35-hour weeks earning minimum wage (Employment and Social Development Canada, 2014).

with long-tenured worker designation. This exclusion restriction cannot be tested directly. However, as described further in Section 4.8, in the 47 regions without any PBD extension, being designated as a long-tenured worker has no effect on any outcomes of interest. The absence of an effect in these regions, where benefit entitlement is unaffected by a claimant's designation, is evidence that the additional weeks of unemployment benefits granted to long-tenured workers in the 15 selected regions are solely responsible for the differences in outcomes observed at the threshold in those locations.

3.4 Main Analysis Sample

Among claimants who are on the margin of qualifying as long-tenured workers, I distinguish between individuals based on the timing of benefit receipt. I refer to individuals whose claims began on or after January 4, 2015, and before the implementation of the PBD extension on July 3, 2016, as "retroactively *eligible* claimants." Those retroactively eligible claimants who received unemployment benefits in at least one of the five weeks following the implementation date are also classified as "retroactively *extended* claimants" and are a subset of all retroactively eligible claimants. Individuals whose unemployment spells commenced on or after the extension's implementation date (July 3, 2016) and before its termination (July 9, 2017) are referred to as "new claimants"; for this group, the PBD extension was applied from the start of their claim.[13]

In the analysis that follows, I focus primarily on outcomes for retroactively extended claimants and for new claimants who initiated their benefit spells before October 30, 2016. I concentrate on these groups for several reasons. First, the magnitude of the treatment—the difference in the PBD extensions for long-tenured workers versus other EI claimants—is greatest at 20 weeks. Second, by excluding retroactively eligible claimants who exited to employment prior to the extension's implementation, I ensure that the analysis is restricted to individuals who were *meaningfully* treated. This latter point is particularly important: of

[13]If an individual has multiple EI claims during the extension period, I focus on a single claim, as subsequent claims may themselves be outcomes of the treatment. For individuals with one or more claims before the implementation date (including those who later file a new claim post-implementation), I select the last claim before implementation. If an individual has only post-implementation claims, I select the earliest such claim.

the 35,340 individuals on the margin of long-tenured worker designation due to their history of payroll tax contributions and prior benefit receipt, approximately half do *not* receive benefits immediately following implementation, likely because they had already found work.

A potential concern with restricting my primary analysis sample to exclude claimants who did not receive benefits post-implementation is the possibility of differential selection into continued unemployment following the March 2016 announcement of the PBD extension. Studies using variation in benefit levels have found that take-up of unemployment insurance rises with its generosity (Kroft, 2008). If long-tenured workers were more likely than other claimants to remain unemployed after the announcement in anticipation of receiving extra weeks of benefits, and if these workers systematically differed in observed or unobserved characteristics, this would threaten the "as-good-as-randomly assigned around the threshold" assumption underlying the regression discontinuity design.

To assess whether differential selection into ongoing unemployment occurred following the extension's announcement, I conduct several tests. Panel A of Table 3 reports the instrumental variable estimate of the effect of long-tenured worker status on the share of claimants receiving benefits in any of the five weeks after the extension was implemented. If long-tenured workers who had exhausted their standard entitlement were more likely to remain unemployed in anticipation of the extension, we would expect an immediate increase in benefit receipt following its implementation. Table 3 shows no statistically significant difference between long-tenured workers and other claimants in the share receiving benefits in any of the five weeks after July 3, 2016. In other words, there is no evidence that EI claimants designated as long-tenured workers were more likely to remain unemployed without benefits.

Figure 7 provides further evidence against differential selection into continued unemployment. Each point represents the instrumental variable estimate of the difference in weekly benefit receipt between long-tenured workers and other claimants who entered into unemployment in 2015, all of whom were retroactively *eligible* claimants. The figure starts in January 2016, with shaded areas indicating 90% robust bias-corrected confidence intervals. In none of the weeks between the time of the announcement and the extension's implemen-

tation are the differences statistically significant, nor is there any difference in receipt rates at the very start of the implementation period.

Both of these facts indicate that there is no differential selection into ongoing unemployment following the announcement of the extension. If long-tenured workers who had not yet exhausted their benefits were more likely to stay unemployed, we would expect to observe higher weekly receipt rates following the announcement. Similarly, if long-tenured workers who had already exhausted their standard entitlement remained unemployed without benefits in anticipation of the extension, we should observe a spike in the difference in weekly benefit receipt immediately after its implementation. However, a statistically significant difference in weekly benefit receipt rates emerges only toward the end of the five-week period following implementation; this timing aligns with when the additional five weeks of EI available to non-long-tenured workers would have been depleted by claimants who had previously exhausted their standard entitlement.

As a final check for differential selection into continued unemployment, Panel B of Table 2 tests whether there are any discontinuities in predetermined characteristics once I put in place the sample restrictions needed to produce the main analysis sample. If there was differential selection into ongoing unemployment, we might expect discontinuities to emerge as a result of conditioning on whether an individual remained unemployed until the time of implementation. As is evident in Panel B, however, for my main analysis sample there are no discontinuities in predetermined variables at the threshold.

Having found no evidence of differential selection into ongoing unemployment, the subsequent analysis using linked tax records mostly focuses on the main analysis sample. In the Appendix, I report estimates for two alternative samples. The first includes all retroactively eligible claimants and new claimants through the end of the extension period—that is, all claimants at the margin of long-tenured worker status, regardless of whether they were still unemployed at the time the extension was implemented. The second alternative sample consists of all retroactively extended claimants and new claimants through the end of the extension period. Both samples include long-tenured workers who were eligible for either 25, 17, or 10 total additional weeks of benefits, corresponding to 20, 12, or 5 extra weeks of

benefit entitlement relative to other claimants who received the 5-week PBD extension.

4 Results

4.1 Additional Entitlement and Benefits Received

Columns 1 and 2 of Table 4 present reduced-form, first-stage, and instrumental variable estimates of the impact of long-tenured worker designation on total weeks of benefit entitlement and total weeks used. The table further includes an estimate of $E[Y(0)|C]$, the expected value of the outcome variable for untreated compliers at the payroll tax share cut-off.[14] My instrumental variable estimates represent the difference in means for treated and untreated compliers at the 30% threshold.

Panel A reports estimates for my primary analysis sample. In column (1), the reduced-form estimate of the increase in total weeks of entitlement associated with crossing the 30% focal payroll tax share cutoff is 12.64 weeks. Dividing this by the first-stage estimate of 0.632 yields an instrumental variables estimate of 20 weeks, which aligns with the fact that all long-tenured workers in the primary analysis sample were entitled to a 25-week PBD extension versus 5 weeks for other claimants. The average total entitlement for untreated compliers (including their standard entitlement plus five weeks of extended benefits) was approximately 41 weeks.

Column (2) reports that the 20-week increase in PBD for long-tenured workers led to an additional 8.32 weeks of benefits being used. To visualize how the additional weeks of unemployment benefit entitlement affects the number of benefit weeks received, Figure

[14] Although individual compliers cannot be identified in the data, $E[Y(0)|C]$ can be estimated using a fuzzy regression discontinuity approach, where the dependent variable in the reduced-form equation is $(1 - LTW_i) \times Y_i$. Here, LTW_i is an indicator for being a long-tenured worker, and Y_i is the observed outcome. To the left of the 30% focal payroll tax share threshold, $(1 - LTW_i) = 1$ for never-takers and compliers, meaning that $(1 - LTW_i) \times Y_i = Y_i(0)$ for these groups and 0 for always-takers. To the right of the threshold, $(1 - LTW_i) = 0$ for compliers, so $Y_i(0)$ is observed only for never-takers, while compliers and always-takers have values of 0. Rescaling the change in the average value of $(1-LTW_i)\times Y_i$ at the threshold by the share of compliers (estimated using the first-stage increase in expected LTW_i) and multiplying by -1 yields $E[Y(0)|C]$.

9 plots the benefit spell survival curves for treated and untreated compliers. While there are initially no differences between the two groups, the treated group becomes statistically significantly more likely to receive at least 30 weeks or more of benefits.

Using my estimates of the change in the duration of the benefit spell (dB) resulting from a change in potential benefit duration (dP), I calculate both the marginal effect of an extension in potential benefit duration (i.e., the increase in weeks of benefits used per additional week of entitlement, $\frac{dB}{dP}$) and the elasticity of the benefit spell with respect to potential benefit duration. The marginal effect is $\frac{dB}{dP} = 0.416$, and the elasticity is $\frac{dB}{dP} \cdot \frac{P}{B} = 0.666$. These values are larger than most—but not all—of the marginal effects and elasticities reported in studies summarized by Schmieder and Von Wachter (2016).

One possible reason my estimates fall toward the higher end of the range observed in the literature is that my main analysis sample is predominantly composed of individuals who had already been unemployed for some time when they became retroactively eligible for an extension and had thus either exhausted or were close to exhausting their standard entitlements. In contrast, the studies summarized by Schmieder and Von Wachter (2016) estimate the effects of potential benefit duration extensions for *all* unemployed workers, including those who may exit to employment soon after they start receiving benefits.

The remaining panels of Table 4 provide estimates for different sets of claimants in my data. Instrumental variable estimates for all the different samples of claimants are also illustrated in Figure 8 to facilitate visual comparisons. Panel B of Table 4 features all retroactively-eligible claimants and new claimants. For all these individuals, the instrumental variables estimate for the additional weeks of entitlement for long-tenured workers is 18.15 weeks, which represents a weighted average of the 20, 12, or 5 extra weeks depending on the EI claims' start dates. Panel C presents estimates for the set of long-tenured workers who were eligible for 20 additional weeks of benefits compared to other claimants (a total of 25 versus 5 weeks of extended weeks), *regardless* of whether they received benefits following the extension's implementation (i.e., this includes retroactively eligible claimants who exited unemployment prior to the extension's implementation). Panels D and E report estimates for the groups of new claimants for whom long-tenured worker designation resulted in el-

igibility for 12 extra weeks (17 versus 5) and 5 extra weeks (10 versus 5) of benefits relative to other claimants, respectively, depend on the timing of their EI claims' start date.

For these other sets of claimants, the instrumental variable estimates of the effect of the PBD extension on weeks of benefits used are smaller than the estimate for my primary analysis sample. For the two samples covering shorter PBD extensions for new claimants who were designated long-tenured workers (Panels D and E, respectively), the estimates of benefits used are statistically insignificant, although these are rather imprecise due to the relatively smaller number of observations.

Finally, I estimate the effect of the PBD extension on both the total value of the additional weeks of entitlement and the total amount of benefits received during the EI claim of interest. The instrumental variables estimates are presented in Table 4, Panel A, columns 3 and 4. The additional 20 weeks of benefits available to long-tenured workers in my main analysis sample have a current CAD value of $9,765. Of this amount, treated compliers received an additional $3,842 in unemployment benefits during their EI claim. The value of the additional entitlement and the amount of benefits received are smaller in the remaining panels, reflecting the combined effect of reduced extra entitlement and fewer weeks of benefits claimed.

4.2 Impacts on Unemployment

The increase in the number of benefit weeks used reported in the previous section reflects a combination of effects. First, holding job search behaviour constant, there will be a mechanical increase in benefit weeks used for individuals who exhaust their standard entitlement, as extended benefits cover additional weeks of these still-ongoing unemployment spells. This is known as the *coverage effect* (Bell et al., 2024). Second, due to behavioural responses, such as reduced search effort, higher reservation wages, or increased selectivity in job applications and acceptance, an increase in PBD may also prolong the duration of unemployment—and, consequently, the total weeks of unemployment benefits received.

Separately estimating the extension's impact on benefit duration and unemployment duration would help disentangle coverage effects from behavioural responses. However, in Canada, administrative data on weekly employment status are only available while individuals are receiving unemployment benefits. Once benefits are exhausted, unemployment is no longer recorded in administrative data, preventing the measurement of total unemployment duration—including periods without benefits—using weeks as the unit of analysis.

Despite this limitation, I provide evidence that at least part of the increase in benefit usage reported in the previous section results from behavioural responses that prolong total unemployment duration. To examine this, I construct indicators for (i) whether claimants exhaust their standard entitlement (i.e., the number of benefit weeks to which they are entitled under the *Employment Insurance Act*) and (ii) whether they exhaust their standard entitlement plus five additional weeks of unemployment benefits. I refer to the latter as non-LTW entitlement. Both indicators are observed for all EI claimants in the 15 regions selected for an extension.

I also estimate the number of weeks of unemployment benefits received, provided those benefits are available to *all* claimants in the 15 selected regions. This is defined as the minimum of: 1) a claimant's actual weeks of benefits received or, 2) their standard entitlement plus five weeks. I term these benefit weeks "non-LTW" weeks, as it represents the total entitlement available to claimants who are not long-tenured workers. If a PBD extension affects job search earlier in the unemployment spell as a result of forward-looking behaviour, we would expect to observe an effect using these three variables.

As shown in columns 3 and 4 of Panel A, Table 4, treated compliers in my main sample are 5.8 percentage points more likely to exhaust their standard entitlement and 9.6 percentage points more likely to exhaust their non-LTW entitlement, although only the latter estimate is statistically significant. Given mean exhaustion rates of 48% for standard benefits and 38.5% for non-LTW benefits among untreated compliers at the margin (i.e., those whose payroll tax share falls just below the threshold for long-tenured worker designation), my instrumental variable estimates represent relative increases of 12% and 25%, respectively. Treated compliers are also estimated to use 1.65 more non-LTW weeks on average, but this

difference is not statistically significant.

In Panel C, which reports results for all claimants who were potentially eligible for either 25 or 5 weeks of extra benefits depending on their long-tenured worker designation—regardless of whether they received benefits following implementation—the differences in the rate of standard entitlement exhaustion and in the number of non-LTW weeks received *are* statistically significant, possibly due to the larger sample size.

4.3 Effects on Re-Employment and Earnings

The finding that eligibility for 20 additional weeks of unemployment benefits led to behavioural responses, in the form of higher exhaustion rates of those benefits available to all claimants, is important. Without such responses, we would not expect to observe any differences in subsequent re-employment outcomes. In this section, I examine how the PBD extension affected various employment outcomes.

I first examine whether becoming eligible for additional weeks of unemployment benefits affects the likelihood that an individual reports employment on an annual basis. Unlike weekly employment status, which cannot be observed for individuals who no longer receive EI, annual employment *is* observed for all individuals in my sample, as it is recorded in tax filings.[15] Panel A of Figure 10 displays employment rates among claimants in my main analysis, spanning the three years before and five years after the PBD extension. Estimates of $E[Y_t(1)|C]$ and $E[Y_t(0)|C]$—the treated and untreated complier means—are plotted separately. Because tax data are reported on a calendar-year basis, relative years are defined for retroactively eligible claimants as time relative to the extension's implementation in 2016, when the different PBD extensions for long-tenured workers and other claimants were first applied; for new claimants, relative time is defined with respect to the onset of their unemployment spell, as the differences in the PBD for different types would have been known from the start.

[15] I classify an individual as employed if they report earnings exceeding $1,000 in constant 2023 CAD.

As shown in Panel A of Figure 10, employment rates before the benefit extension were close to 100% for both treated and untreated compliers. This is expected, as individuals must have been employed before the initiation of an EI claim. Following job loss, overall employment rates decline for both groups and remain lower throughout the subsequent five years, with only about 90% of claimants reporting paid employment in their annual tax filings.

Panel A of Table 5 presents the instrumental variables estimate of the effect of 20 additional weeks of benefit entitlement available to long-tenured workers on annual employment rates. In none of the post-extension years is the difference between treated and untreated compliers statistically significant.

The absence of a statistically significant effect suggests that, despite evidence from Section 4.2 indicating that the benefit extension prolonged the duration of unemployment, the magnitude of this effect is insufficient to be detected in annual employment measures. In all the studies reviewed by Schmieder and Von Wachter (2016), the effects of PBD extensions on the duration of unemployment are smaller than the effect on the duration of benefit spells. As noted in Section 4.1, the 20 additional weeks of benefit entitlement for long-tenured workers resulted in an average of only 8.32 additional weeks of benefits used. Provided that my setting does not represent an exceptional departure from this pattern, it is unlikely that the increase in the duration of unemployment exceeded the 8.32 week increase in the duration of the benefit spell.

I next examine whether additional weeks of unemployment benefits influence the level of T4 employment earnings (i.e., income paid by employers in the form of wages and salaries). Although the behavioural responses that extend unemployment duration—evidence of which is reported in Section 4.2—would reduce earnings in the short run by delaying reemployment, this negative effect may be offset in the long run if jobseekers ultimately secure positions with higher wages or longer work hours.

Figure **??**, Panel A, presents trends in T4 employment earnings for treated and untreated compliers in the years before and after the PBD extension. For both groups, there is a sub-

stantial decline in earnings following job loss, which for claimants in my main sample occurs in either relative time $t = -1$ or $t = 0$ (recall that relative time is defined with respect to the extension's implementation date for retroactively eligible claimants or the start of the unemployment spell for new claimants). Relative to peak pre-unemployment earnings in year $t = -2$, treated and untreated compliers experience a drop in earnings of 40% to 44% two years later. These are large declines but align with the upper range of prior estimates on the effects of layoffs on earnings in Canada (Morissette et al., 2013).

The instrumental variable estimates of the impacts on T4 employment earnings are presented in Panel B of Table 5, and are illustrated in Figure 11, Panel B. The estimates reveal that in the first two full years after the extension's implementation, the effect of the additional twenty weeks benefits available to long-tenured workers was to *increase* earnings from paid employment, thereby attenuating the negative effects of job loss on longer-run earnings. One year after treatment treated compliers earned $7,021 more than their untreated counterparts; two years after, the difference was $7,270. Relative to the untreated complier means, these statistically significant effects represent increases in earnings of 16% and 15%, respectively. Employment earnings for treated and untreated compliers converge from relative year $t = 3$ onward.

The estimated effects on earnings are notable both for their magnitude and direction, yet they remain plausible given the characteristics of my primary analysis sample, which consists largely of unemployed individuals who had either exhausted or were approaching the exhaustion of their standard benefit entitlement at the time of the extension. Existing literature suggests that this group is precisely the one most likely to exhibit a strong response to a benefit extension. For instance, Nekoei and Weber (2017) find that the positive re-employment effects of the Austrian UI extension they study are entirely driven by displaced workers with the highest ex ante risk of benefit exhaustion. Similarly, Caliendo et al. (2013), combining regression discontinuity estimates with a structural model of unobserved heterogeneity, report that the positive re-employment effects on wages of a UI extension in Germany occur among individuals who exit unemployment around or after the original benefit exhaustion point.

Panels C and D of Table 5 present two additional employment-related outcomes. The first is earnings, conditional on reporting wage-sector employment. Although the differences in annual employment rates are not statistically significant, earnings levels may still vary due to differences in the share of claimants reporting *any* earnings—particularly in year $t = 1$, which shows an approximate 4 percentage point difference in employment rates (albeit not statistically significant). When restricting the sample to claimants with positive employment earnings, the estimated effects on earnings are similar: relative to the untreated complier means, treated compliers who are employed earn 11% and 15% more in years $t = 1$ and $t = 2$, respectively. Using log earnings, the estimated effects are 0.167 and 0.055 log points in those years, with the former being statistically significant (p-value < 0.10).

While the instrumental variable estimate of the increase in earnings is large, it falls within the range of estimates reported in prior studies. For example, Chao et al. (2024) report gains of 10% in quarterly earnings upon re-employment for workers who experience a job separation and whose prior earnings during the base period exceed the minimum threshold required to qualify for up to 26 weeks of UI in the United States. Furthermore, their estimate is an intent-to-treat, as they do not observe take-up decisions or other factors affecting categorical eligibility (such as whether the job loss was due to a layoff or a quit).

My results on the level of earnings are robust to various combinations of control variables. Appendix Figure A.1 presents point estimates and confidence intervals from specifications that include different combinations of the four covariates used in the regression discontinuity specification in the main text: age at the time of the claim, prior earnings, prior hours worked, and status quo entitlement weeks. Although the estimates for years $t = 1$ and $t = 2$ are not statistically significant when no controls are included, they become significant in nearly all other specifications that include any combination of covariates. Including these controls increases the precision of the estimates.

Appendix Table A.1 shows that the positive earnings effects are also evident for an alternative sample, namely all retroactively extended claimants and all new claimants (including claimants who received shorter PBD extensions for long-tenured workers because their claims started on or after October 30, 2016). For this group, the point estimates of earnings

increases in relative years $t = 1$ and $t = 2$ are smaller than the estimates for the main analysis sample but are nonetheless statistically signficant. When including all retroactively eligible and new claimants (in other words, all EI claimants on the margin of long-tenured worker designation, regardless of whether they exited unemployment prior to the extension's implementation) the point estimates remain positive but are attenuated towards zero and no longer statistically significant.

4.4 Potential Mechanisms

I next explore potential mechanisms underlying the positive earnings effects I estimate.

First, I examine whether treated compliers in my main analysis sample are more likely than untreated compliers to remain in the same industry in which they were previously employed. Prior research on job displacement suggests that earnings losses are smaller for workers who secure re-employment within the same industry. For instance, Addison and Portugal (1989) estimate a wage penalty of 16.1% to 19.8% for displaced workers who switch industries, a finding corroborated by Neal (1995).

To assess industry transitions, I compare the two-digit North American Industry Classification System (NAICS) codes reported on EI claims with those recorded in the T1 income tax database.

Panel A of Figure 12 illustrates trends in the share of treated and untreated compliers who report employment in the same industrial sector as their pre-layoff job. In the years preceding job loss, the share of claimants employed in the industry from which they are eventually laid off increases steadily. This pattern is unsurprising given that the data primarily cover younger workers who recently attended a post-secondary institution or participated in an apprenticeship program. Some individuals are likely still in the process of entering the industry most aligned with their skills. Following job loss, both treated and untreated compliers transition out of their previous industry. Among untreated compliers, this transition is abrupt, as reflected in the sharp decline in same-industry employment in

relative years $t = 0$ and $t = 1$. In contrast, the transition among treated compliers is more gradual.

Table 6 quantifies the extent to which the PBD extension for long-tenured workers preserved worker–industry matches. Instrumental variable estimates indicate that the extension increased the same-industry share by 8.8 percentage points in the initial post-treatment year ($t = 0$), representing an 11% gain over the untreated-complier mean of 76.2%. The effect nearly doubles to 16.9 percentage points in year $t = 1$ (p-value < 0.01), a 32% increase relative to a substantially lower counterfactual match rate of 53.4%. Statistically significant gains of approximately 12 to 14 percentage points persist through relative years 2 to 4, translating into 24–30% increases over untreated complier averages. The IV estimates are plotted in Panel B of Figure 12.

The effects of additional weeks of unemployment benefit entitlement on the same-industry share suggest that at least part of the positive earnings impacts reported in Panel B of Table 5 result from re-matching with employers in the same industry. These effects on industrial attachment are statistically significant and economically meaningful, mirroring the strong returns in earnings.

The finding that extended PBD increases re-employment earnings may be explained, in part, by a greater likelihood of re-matching within the same industry. This interpretation is supported, albeit in the opposite direction, by Schmieder et al. (2016), who find that each additional month of potential benefits reduces re-employment wages by 0.1 percent and decreases the probability of returning to the same industry by 0.12 percentage points. Their results suggest that reduced industry attachment may explain earnings declines; conversely, my findings highlight the reverse mechanism: maintaining industry attachment can enhance re-employment outcomes.

As another measure of job quality, I estimate whether the additional benefits made available to long-tenured workers had any effect on the likelihood of being employed in a unionized job in the future. Unionization status can be inferred from tax filing based on whether an individual reports paying union dues. There are, however, no statistically significant dif-

ferences in any of the post-treatment years. The IV and reduced-form estimates are included in Panel B of Table 6.

Next, I examine whether extending unemployment benefits for long-tenured workers influences their residential mobility. It is well-documented that regions experiencing higher unemployment rates typically see increased out-migration (Greenwood, 1997). Recent evidence further shows displaced workers relocate at higher rates compared to their non-displaced counterparts (Fackler and Rippe 2017; Huttunen et al. 2018). Theoretically, the impact of unemployment insurance on relocation decisions is ambiguous. Unemployment benefit recipients may be less likely to move if they exert less effort to apply for jobs that require relocation, or if they decide to stay put while awaiting a rebound in hiring in their current region of residence. Alternatively, by relaxing liquidity constraints, unemployment insurance may help finance moves to regions with better job prospects (Tatsiramos 2008; Nunn et al. 2018).

To understand whether the additional entitlement weeks available to long-tenured workers affected their decision to relocate, I construct an indicator variable that captures whether claimants reside in a different census subdivision in their tax filings relative to the subdivision in which they were living when the layoff occurred.[16] In the years preceding layoff, both treated and untreated compliers display in-migration into the census subdivision where their layoff subsequently occurs; afterward, both groups exhibit some out-migration. With regard to differences between the two groups, Panel C of Table 6 indicates that instrumental variable estimates do not significantly differ statistically in any year post-treatment.

Taken together, the results in this section offer suggestive explanations for the large, positive impact on T4 employment earnings discussed in Section 4.3. For the treated compliers in my sample (those designated as long-tenured workers because they had paid just enough in past payroll taxes to cross the cut-off), the effect of receiving 20 additional weeks of extended benefits was to increase the likelihood of becoming re-employed in the same industry, with no apparent effect on unionization status or residential location.

[16] A census subdivision is a statistical area comparable to a municipality.

The significance of these patterns of industrial attachment is particularly noteworthy, especially given contemporaneous news reports highlighting the challenges faced by some unemployed workers—most notably those with specialized expertise in oil-and-gas-related sectors. Prospective employers in other sectors that were hiring were reportedly reluctant to take on such workers, fearing they would leave once commodity prices recovered.[17]

The expectation of a recovery in the oil and gas sector also represents a key distinction that may help explain why the PBD extension led to increases in re-employment earnings: claimants designated as long-tenured workers could afford to wait longer for an economic rebound driven by gradually rising global prices. In Alberta, where more than 60% of individuals in my main analysis sample lived, the economic recovery began in 2017, with positive GDP growth of 4.2% following two years in which Alberta's GDP shrank by about 3.5% per year (in 2015 and 2016). This dynamic may also help explain why the PBD extension examined in this study increased the likelihood of returning to the same industry of prior employment, contrasting with the decline reported by Schmieder et al. (2016), who estimate an average effect for laid-off workers across the business cycle and where the change in PBD is based on workers' ages rather than a policy response to a sector-specific economic downturn.

4.5 Effects on Self-Employment

The literature on unemployment has noted that job loss may "push" some individuals toward self-employment, including "gig work" (Kuhn and Schuetze, 2001, Rissman, 2003, von Greiff, 2009, Jeon and Ostrovsky, 2025). More recent theoretical work has considered how self-employment can serve as a form of self-insurance in response to job loss, and notes that more generous unemployment insurance may crowd out this form of self-insurance (Choi, 2024). Other studies have noted that more generous UI may disuade individuals from pursuing entrepreneurship if they risk losing eligibility for benefits (Gaillard and Kankanamge,

[17] CBC News, January 14, 2016: "*An out of work specialized oil and gas accountant is hoping the federal government will look at some sort of emergency measures for extending employment insurance benefits.... She says she has tried to find work outside of oil and gas, but found employers were reluctant because they fear she will leave once the industry recovers.*"

2025).

While earlier observational studies report a negative statistical association between unemployment insurance generosity and the probability of transitioning into self-employment (Moore and Mueller, 2002), much of the recent empirical research investigating the impacts of unemployment insurance using regression discontinuities has omitted self-employment as an outcome. This gap primarily arises due to reliance on administrative data from social insurance programs, which typically exclude self-employed individuals and therefore do not capture data on earnings from self-employment. Such data limitations prevent researchers from distinguishing clearly between periods of non-employment and self-employment. In contrast, the personal income tax data available in this study uniquely enable me to directly observe both the occurrence and level of self-employment income, making it possible to clearly identify transitions into self-employment.

Panel A of Figure 13 illustrates trends in self-employment among treated and untreated compliers before and after the extension of potential benefit duration. The self-employment indicator equals one if an individual reports any non-zero self-employment income on their personal income tax return.[18] As shown in the figure, both treated and untreated compliers exhibit similarly low self-employment rates in the years preceding their layoffs and the PBD extension. Afterward, however, untreated compliers experience an increase in self-employment rates relative to treated compliers. The instrumental variable estimates presented in Table 7, Panel B confirm that this divergence is statistically significant. One year after the PBD extension, treated compliers are 6.4 percentage points less likely to report self-employment income; after two years, they are 7.8 percentage points less likely. Given that untreated compliers have mean self-employment rates of approximately 14%, individuals who become eligible for additional benefit weeks due to crossing the focal payroll tax contribution threshold are about half as likely to report self-employment income.

The decrease in rates of self-employment following the PBD extension is mirrored in the differences in the levels of self-employment income; to make possible the disclosure of self-employment following Statistics Canada's rules, self-employment income is Winsorized at

[18]Net self-employment income can be negative if an individual's business incurs a loss.

the 1st and 99th percentiles. In terms of differences between treated and untreated compliers, treated compliers have self-employment income that is lower by $731, $1,701, $975, and $952 in the four years following treatment (see Panel A of Table 7).

In addition to examining whether individuals report any self-employment income, I also analyze whether they report income above thresholds of $10,000 and $20,000. These thresholds help differentiate between occasional gig work used to supplement other income sources and more substantial engagement in self-employment as a primary income source. The trends in the share of claimants reporting more than $10,000 in self-employment income are also illustrated in Panel A of Figure 14.

On both measures, the increase in self-employment activity is greater among untreated compliers than among treated compliers. As is shown in Panels C and D of Table 7, treated compliers are significantly less likely to report self-employment income above either threshold for up to three years following treatment. That said, high levels of self-employment income are uncommon overall: in the five years following treatment, only about 5% of untreated compliers report more than $10,000 in self-employment, and just 2.5% to 4.1% report more than $20,000. Among treated compliers, virtually no one reports self-employment income exceeding $20,000. These results suggest that while self-employment serves as a margin of adjustment following job loss, relatively few laid-off workers pursue it in a substantial way. For claimants designated long-tenured workers, the extra PBD extension reduces movement both into self-employment overall and into higher-earning forms of self-employment.

4.6 Transfer Program Participation and Substitution

An important factor in assessing the impact of a PBD extension on total government expenditures is determining the extent to which other components of the social safety net can substitute for unemployment insurance. Evidence from the United States suggests that, following the exhaustion of unemployment benefits, many individuals enter in-kind income support programs (such as food stamps and Medicaid). There is also evidence that appli-

cants for unemployment insurance who are deemed ineligible subsequently apply for and receive welfare benefits (Rothstein and Valletta, 2017, Leung and O'Leary, 2020, Hamdi et al., 2025) . Evidence on substitution between unemployment insurance and disability insurance in the United States is mixed (Lindner, 2016, Mueller et al., 2016), whereas European studies indicate that among older workers, unemployment insurance interacts with both retirement benefits and disability programs (Inderbitzin et al., 2016).

Thanks to the availability of tax records for up to five years following exposure to the PBD extension, I am able to examine the impacts on transfer program participation in both the short and long run. The possibility that changes in unemployment insurance entitlements have persistent effects on future transfer receipt has been noted by Schmieder et al. (2012b), who observe that the effect of PBD extensions in Germany on total days of non-employment over five years is smaller than the effect on the duration of the initial non-employment spell. Schmieder et al. suggest that this reduction may reflect both a mechanical effect—less time remaining after the initial spell in which to be non-employed—and a behavioural response, in which recipients who benefited from the PBD extension found more stable jobs and faced a lower risk of future unemployment and subsequent unemployment insurance receipt.

In Canada, last-resort income support is provided through provincially administered Social Assistance programs, which are means- and asset-tested and impose no time limits on receipt.[19] Evidence on the interaction between EI and Social Assistance is limited, aside from descriptive studies documenting relatively low rates of Social Assistance receipt among EI exhaustees.[20]

I examine whether the additional PBD extension for long-tenured workers affects the likelihood of receiving Social Assistance, using an indicator variable equal to one if individuals report receiving Social Assistance income on their tax filings. Following unemployment

[19] Able-bodied recipients are, however, typically required to participate in assisted job searches or other employment services.

[20] Finnie et al. (2013) report that only 4 to 5% of Employment Insurance exhaustees receive Social Assistance within five years of layoff, while Grey (2002) notes that Social Assistance entry often occurs with a delay following exhaustion. This delay is attributed to asset holdings that initially render individuals ineligible for Social Assistance immediately after exhausting unemployment benefits; Social Assistance programs require individuals to have very low levels of liquid assets to qualify for welfare payments.

entry and the implementation of the benefit extension, untreated compliers are more likely to report receiving Social Assistance, although both treated and untreated compliers exhibit relatively low receipt rates overall: for untreated compliers, the Social Assistance receipt rate ranges between 1% and 6% in the post-treatment period. Panel A of Table 8 shows that a statistically significant negative difference of 3.2 percentage points (p-value < 0.05) emerges in year $t = 2$, with only 2% of treated compliers receiving Social Assistance compared to 5.6% of untreated compliers. In relative year $t = 4$, treated compliers are again significantly less likely to receive Social Assistance, with a difference of 2.3 percentage points (p-value < 0.10). These results suggest that the additional weeks of EI available to long-tenured workers may have reduced reliance on Social Assistance both in the near term following EI exhaustion and in the longer run.

In Panels C and D of Table 8, I examine whether there are differences in the rates at which transfer income is received through the Canada Pension Plan (CPP) and Workers' Compensation. CPP is a social insurance program that provides pension payments to eligible contributors upon retirement (at age 60 or older) and replaces earnings for younger workers in the event of a work-limiting disability. Workers' Compensation provides wage replacement and covers medical expenses following workplace injuries. I find no evidence of program substitution between EI and Workers' Compensation, which is perhaps unsurprising given the relatively low incidence of workplace injuries and my primary analysis sample's modest size.

With respect to CPP income, however, it is notable that in relative year $t = 4$, treated compliers are 2.7 percentage points more likely to receive CPP income than untreated compliers.

The longer-run impacts on program participation for Social Assistance and CPP—moving in opposite directions—merit further discussion. What might explain these differences in program participation five years after treatment? It is worth noting that, for my main analysis sample, relative time aligns with calendar years, with $t = 0$ corresponding to 2016.[21] Thus, for this sample, relative year $t = 4$ corresponds to 2020, the onset of the COVID-19

[21] When considering all individuals at the margin of long-tenured worker designation, $t = 0$ is 2017 for some new claimants.

pandemic.

One possible explanation for the decrease in Social Assistance receipt and the increase in CPP receipt relates to higher levels of earnings earlier in the post-treatment period. Individuals with higher earnings have higher CPP contributions, and may therefore have been eligible for higher benefit levels. Following the onset of the pandemic, those with sufficient contributions may have turned to CPP either in retirement or as an individual deemed to be disabled, while those without such contributions could have received emergency pandemic payments for out-of-work individuals, which are recorded as Social Assistance income in the tax data.

4.7 Total Income

Having examined the effects of the extended PBD on employment, self-employment, and transfers individually, I now consider their combined impact on total income and taxes paid. This broader perspective is essential for assessing the overall fiscal externality of the policy. To the extent that positive re-employment earnings effects increase tax revenue, the short-run cost of providing extended benefits through EI may be offset, suggesting that consumption smoothing benefits can be provided at a low social cost.

While in Table 4, I reported the additional amount of benefits paid during the focal benefit spell, I now consider the amount of EI income received in each year relative to treatment; estimates are found in Panel A of Table 9. In the tax records included in the ELMLP, EI income includes unemployment benefits as well has any special benefits (comprising maternity leave, parental leave, and short-term sickness leave). Prior to treatment, there is no difference in the annual amount of EI received; following the PBD extension, the amount of EI income is statistically significantly higher in $t = 0$, 1, and 2, increasing by \$2,293, \$2,324, and \$994, respectively. Relative to untreated complier means of \$10,927, \$4,361, and \$2,999, these represent increases of 21%, 53%, and 33%, respectively. There are statistically signficant differences in EI income received after this point.

The effects of the extra PBD extension for long-tenured workers are evident across three calendar years for two reasons. First, a single unemployment benefit claim may span adjacent calendar years, and some claimants who initiated spells later in year $t = 0$—particularly those with long standard entitlements topped up with 25 additional weeks—may have only exhausted their additional benefits in year $t = 2$. Second, some claimants in my sample may have initiated another unemployment claim before the extension period ended on July 9, 2017, and were again designated long-tenured workers based on the same focal payroll tax share that affected their designation during the prior claim.

I next examine the contribution of employment and self-employment income to total income. Panel B of Table 9 presents the instrumental variable estimates for what I term labor market income—the sum of earnings from paid employment, net self-employment income, and other employment-related income (such as tips and gratuities). The point estimates of the impact of the PBD extension on labor market income are smaller than those for employment earnings alone that are reported in Table 5. This is consistent with the fact that higher levels of self-employment income reported by untreated compliers would have partially offset differences in earnings from paid employment between treated and untreated compliers; these offsetting effects are illustrated in Figure 15. Although the instrumental variable point estimates for the impact of the PBD extension on labor market income for long-tenured workers are smaller than those for employment earnings, they remain statistically significant in the first two full calendar years following the implementation of the extension.

Panel C of Table 9 examines the effect on what Statistics Canada refers to as total income, though it is more accurately described as total after-transfer, before-tax income. This measure includes labor market income, investment income (such as dividends, excluding capital gains), and transfer income, including EI, CPP, Social Assistance, federal and provincial child benefits, and in-work refundable tax credits such as the Canada Workers Benefit. Here, the combined effects of increased labor market income and higher transfer payments are evident in the first year of exposure to the PBD extension ($t = 0$) and in the two subsequent years. The effects are large in magnitude and statistically significant in all three years. Relative to untreated complier means of \$47,805, \$50,723, and \$54,901, total before-tax in-

come among treated compliers is higher by 11%, 20%, and 13%, respectively.

A natural implication of higher before-tax income is that taxes paid also increase.[22] Using income tax data, I calculate net taxes paid as the sum of net taxes paid to the federal and provincial governments. Panel D of Table 9 shows that, consistent with the increase in total before-tax income, treated compliers pay more in taxes following the PBD extension, with the difference being statistically significant in years $t = 1$ and $t = 2$.

Notably, in the first three years following the extension, treated compliers paid $956, $2,232, and $2,022 more in taxes, respectively. The sum of these instrumental variable estimates is $5,219. In comparison, the corresponding estimates for the increase in EI income received over the same period are $2,294, $2,324, and $994, summing to $5,612. In other words, based on these point estimates, the increase in taxes paid as a result of higher before-tax income nearly fully offsets the additional EI expenditures.

Panel E of Table 9 reports the after-tax income; the instrumental variable point estimates are, predictably, lower than those for before-tax income. They are statistically significant in all three years following the extension. As noted by Stepner (2023), a progressive income tax system has the effect of attenuating income losses following layoffs and can effectively serve as a form of insurance against income losses. In my setting, the difference in before-tax income (inclusive of market income and transfer program income) arising from the extra PBD extension for long-tenured workers is attenuated by a progressive tax system.

4.8 Robustness

To what extent can the observed impacts described above be attributed to the additional weeks of PBD in the selected EI regions, rather than to other factors associated with claimants' designations? This question is particularly relevant given that the long-tenured worker designation was introduced in 2012 by the federal government, alongside the occasional and frequent claimant categories, to impose differentiated job search responsibilities.

[22] Employment Insurance income is taxable and is subject to a separate benefit repayment rate of 30% for repeat users whose annual income exceeds 1.5 times the maximum insurable earnings for that year.

For example, claimants classified as frequent users were expected to accept jobs in different occupations, at lower wages, or with longer commutes than those classified as occasional claimants, who, in turn, were expected to accept less favourable job offers than long-tenured workers.

Evidence from other jurisdictions indicates that job search requirements, when salient and enforced, can significantly affect benefit receipt (Chan et al., 2024). Internal reports from Employment and Social Development Canada—the department responsible for unemployment insurance policy—found, however, that claimant awareness of the job search obligations associated with their category was limited (Employment and Social Development Canada, 2018). Enforcement activity was similarly minimal: in 2015, only 1,200 claimants across all of Canada were penalized for failing to search for or accept suitable employment (Canada Employment Insurance Commission, 2016). Following a change in government in October 2015, these differentiated requirements were formally eliminated.

To confirm that, without being tied to extra weeks of entitlement, long-tenured worker designation alone had no effect, Table A.2 presents instrumental variable estimates of the impacts of long-tenured worker designation in the 47 EI economic regions that were not included in the federal response to the commodity price downturn.

The estimation sample for these placebo tests is constructed to closely mirror the main analysis sample. It includes claimants who began an EI benefit spell on or after January 4, 2015, and before October 30, 2016. Claimants who initiated their claim before July 3, 2016 must have either received benefits in one of the five subsequent weeks, or reported being unemployed during the week of May 16 in the Census, for which a linkage to the ELMLP exists.[23]

I specifically examine outcomes for which I find statistically significant impacts when using my main analysis sample: EI income, earnings from employment, self-employment, and industry of employment. Across these measures, the estimates are small and statistically

[23]Ideally, I would restrict the sample to claimants who remained unemployed at the start of July 2016. However, weekly unemployment status is not observable in administrative records once EI benefits are exhausted. Instead, I rely on the Census linkage to identify EI exhaustees who were likely still unemployed as of July 3, 2016.

insignificant, supporting the interpretation that the observed effects in treated regions are attributable to the additional weeks of EI entitlement rather than to the long-tenured worker designation alone.

5 Conclusion

This paper provides rigorous quasi-experimental evidence on the re-employment impacts of extending potential benefit duration during economic downturns, using detailed administrative data from Canada. I show that additional weeks of benefit entitlement, implemented in response to an economic downturn caused by a global oil price shock, yield significant long-term benefits. Specifically, longer UI durations substantially increase re-employment earnings, improve the probability of re-employment within the same industry, reduce entry into self-employment.

Moreover, these extensions generate positive impacts on public finances by increasing subsequent tax revenues and reducing reliance on other social safety net programs, such as Social Assistance. Consequently, the short-run fiscal cost of more generous unemployment benefits can be offset in the long-run.

These findings underline the important role that unemployment insurance design plays in mitigating the adverse impacts of economic downturns. This study contributes important evidence to ongoing policy discussions about optimal UI design and highlights the potential for counter-cyclical expansions in generosity to produce welfare-improving outcomes.

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6 Tables and Figures

Table 1: Summary Statistics

	LTW	Occasional Claimants	LTW–Occasional Claimants	Claimants Around Payroll Tax Cutoff
	(1)	(2)	(3)	(4)
LTW share	1.0	0.0	1.0***	0.504
Age of claimant	44.9	36.2	8.7***	34.1
Male	0.691	0.638	0.052***	0.717
Insured earnings (current CAD)	17,520	15,315	2,210***	17,625
Insured hours of work	1,636.3	1,445.2	191.1***	1,603.0
Number of prior EI claims	5.100	4.100	1.000***	3.900
Total unemployment benefits received in past 560 weeks	5.9	13.9	-8.0***	6.4
Total weeks of benefit entitlement	59.8	39.3	20.5***	50.1
Weekly benefit amount (current CAD)	505	455	50***	505
EI region				
Newfoundland-Labrador	0.051	0.080	-0.029***	0.044
Sudbury	0.015	0.018	-0.003***	0.012
Northern Ontario	0.053	0.078	-0.025***	0.037
Northern Manitoba	0.010	0.027	-0.016***	0.007
Saskatoon	0.034	0.033	0.001**	0.038
Southern Saskatchewan	0.034	0.031	0.003***	0.036
Northern Saskatchewan	0.029	0.040	-0.011***	0.026
Calgary	0.211	0.168	0.043***	0.202
Edmonton	0.214	0.179	0.035***	0.225
Northern Alberta	0.058	0.058	0.000	0.058
Southern Alberta	0.165	0.128	0.038***	0.150
Southern Interior British Columbia	0.076	0.094	-0.017***	0.098
Northern British Columbia	0.045	0.059	-0.014***	0.059
Whitehorse and Nunavut	0.004	0.007	-0.002***	0.007

Continued on next page

	LTW	Occasional Claimants	LTW–Occasional Claimants	Claimants Around Payroll Tax Cutoff
	(1)	(2)	(3)	(4)
Industry				
Agriculture, forestry, fishing and hunting	0.015	0.023	-0.008***	0.013
Mining, quarrying, and oil and gas extraction	0.104	0.051	0.052***	0.079
Utilities	0.005	0.003	0.002***	0.005
Construction	0.223	0.242	-0.018***	0.309
Manufacturing	0.115	0.081	0.035***	0.087
Wholesale trade	0.051	0.035	0.016***	0.039
Retail trade	0.054	0.073	-0.018***	0.047
Transportation and warehousing	0.052	0.045	0.007***	0.036
Information and cultural industries	0.009	0.005	0.003***	0.005
Finance and insurance	0.014	0.008	0.005***	0.009
Real estate and rental and leasing	0.022	0.019	0.003***	0.020
Professional, scientific and technical services	0.063	0.045	0.017***	0.065
Management of companies and enterprises	0.014	0.009	0.005***	0.011
Administrative and support, waste management and remediation services	0.047	0.058	-0.011***	0.044
Educational services	0.057	0.069	-0.011***	0.061
Health care and social assistance	0.029	0.036	-0.007***	0.032
Arts, entertainment and recreation	0.008	0.015	-0.006***	0.009
Accommodation and food services	0.028	0.057	-0.029***	0.026
Other services (except public administration)	0.038	0.043	-0.005***	0.052
Public administration	0.039	0.062	-0.023***	0.036
No industry code	0.013	0.022	-0.009***	0.014
Number of claimants	248,410	399,730		35,340

Notes: The sample in Column 1 consists of all long-tenured workers in the 15 EI regions selected for a PBD extension who started their claim on or after January 4, 2015 and before July 9, 2017. The sample in Column 2 consists of all occasional claimants in those 15 regions whose claim dates started over the same period. Column 3 provides estimates of the difference in means of Columns 1 and 2. The statistical significance of this difference is denoted by *p<0.10, **p<0.05, ***p<0.01. The sample in Column 4 consists of all EI claimants in the 15 regions who had received fewer than 36 weeks of benefits in the 560-week period preceding their claima and who had paid 30% of the annual maximum EI payroll tax in at least 6 but not more than 7 of the prior 10 calendar years.

Table 2: Smoothness of Predetermined Variables Around the Threshold

	Predetermined variables:								
	Age	Share male	Number of prior EI claims	Insured hours	Insured earnings (current CAD)	Weeks of standard entitlement	Weekly benefit amount (current CAD)	Weeks of benefits received prior to PBD implementation	Weeks of benefits received between announcement and implementation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: All retroactively eligible and new claimants									
	0.411	0.019	-0.061	-12.60	42.90	-0.350	-0.831	0.202	0.220
	[-0.257, 1.01]	[-0.013, 0.068]	[-0.311, 0.123]	[-35.54, 14.54]	[-197.97, 296.73]	[-0.812, 0.122]	[-5.85, 4.32]	[-0.576, 1.18]	[-0.101, 0.654]
Panel B: Main analysis sample									
	0.838	0.041	0.141	-37.14	-3.50	-0.801	-1.35	-0.868	0.237
	[-0.107, 1.83]	[-0.011, 0.107]	[-0.141, 0.475]	[-72.38, 1.70]	[-368.37, 328.68]	[-1.62, 0.028]	[-9.09, 5.15]	[-2.12, 0.366]	[-0.357, 1.09]

Notes: Table presents estimates of discontinuities in predetermined variables at the 30% focal payroll tax share cutoff. 90% robust bias-corrected confidence intervals are included under estimates in square parentheses. Sample composition is described by panel headings. Estimates statistically significant at *** 99 percent, ** 95 percent, and * 90 percent confidence levels, respectively.

Table 3: Selection into Ongoing Unemployment

	Share receiving benefits in first 5 weeks following July 3, 2016 (1)
Panel A: All claims eligible for 25- vs. 5-week extension	
IV estimate	0.038
	[-0.006, 0.111]
Reduced-form estimate	0.024
First-stage	0.633***
$E[Y(0)\|C]$	0.307
Panel B: All retroactively eligible claims from 2015	
IV estimate	0.005
	[-0.059, 0.068]
Reduced-form estimate	0.003
First-stage	0.648***
$E[Y(0)\|C]$	0.273

Notes: Table presents estimates of discontinuities in share of claimants who receive at least one week of benefits in the first 5 weeks following the implementation of the PBD extension on July 3, 2016. Sample composition is described by panel headings. Robust, bias-corrected 90% confidence intervals displayed in parentheses. Estimates statistically significant at *** 99 percent, ** 95 percent, and * 90 percent confidence levels, respectively.

Table 4: Additional Benefit Entitlement and Use

	Outcome variables:						
	Total weeks of entitlement	Benefit weeks used	Value of additional entitlement (current CAD)	Benefits received (current CAD)	Exhausted status quo entitlement	Exhausted non-LTW entitlement	Number of non-LTW benefit weeks used
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Primary analysis sample							
IV estimate	20.00***	8.32***	9,765***	3,842***	0.058	0.096**	1.65
	[20.00, 20.00]	[5.19, 11.54]	[9,632, 9,934]	[2,455, 5,494]	[-0.024, 0.153]	[0.018, 0.188]	[-0.614, 4.44]
Reduced-form estimate	12.64***	5.00***	6,006***	2,318***	0.035	0.058*	0.991
First-stage	0.632***	0.602***	0.615***	0.603***	0.603***	0.607***	0.601***
$E[Y(0)\|C]$	41.01	28.40	2,436	13,024	0.480	0.385	28.15
Panel B: All retroactively eligible claimants and new claimants							
IV estimate	18.15***	4.63***	8,937***	1,854***	0.063***	0.074***	1.30
	[17.83, 18.51]	[2.63, 6.75]	[8,774, 9,132]	[845.64, 2,852]	[0.026, 0.108]	[0.029, 0.125]	[-0.348, 3.13]
Reduced-form estimate	11.52***	2.91***	5,710***	1,170***	0.041***	0.047**	0.819
First-stage	0.635***	0.629***	0.639***	0.631***	0.653***	0.637***	0.630***
$E[Y(0)\|C]$	40.37	21.31	2,453	9,860	0.304	0.197	21.12
Panel C: All claims eligible for 25- vs. 5-week extension							
IV estimate	20.00***	5.48***	9,865***	2,275***	0.075**	0.095***	1.98*
	[20.00, 20.00]	[3.21, 7.92]	[9,760, 9,975]	[1,146, 3,403]	[0.024, 0.141]	[0.052, 0.159]	[0.224, 4.11]
Reduced-form estimate	13.14***	3.42***	6,227***	1,424***	0.047**	0.060***	1.24*
First-stage	0.657***	0.624***	0.631***	0.626***	0.630***	0.633***	0.625***
$E[Y(0)\|C]$	40.04	20.86	2,470	9,614	0.308	0.177	20.66

Continued on next page.

	Outcome variables:						
	Total weeks of entitlement	Benefit weeks used	Value of additional entitlement (current CAD)	Benefits received (current CAD)	Exhausted status quo entitlement	Exhausted non-LTW entitlement	Number of non-LTW benefit weeks used
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel D: All claims eligible for 17- vs. 5-week extension							
IV estimate	12.00***	2.36	5,790***	377.50	0.025	-0.056	-0.081
	[12.00, 12.00]	[-3.35, 7.57]	[5,610, 5,939]	[-2,382, 2,556]	[-0.172, 0.177]	[-0.232, 0.071]	[-4.67, 4.10]
Reduced-form estimate	7.75***	1.54	3,843***	250.98	0.016	-0.036	-0.054
First-stage	0.646***	0.651***	0.664***	0.665***	0.643***	0.645***	0.659***
$E[Y(0)\|C]$	42.67	22.79	2,478	10,659	0.318	0.273	22.73
Panel E: All claims eligible for 12- vs. 5-week extension							
IV estimate	5.00***	0.921	2,444***	277.11	0.056	0.062	-0.490
	[5.00, 5.00]	[-3.71, 5.55]	[2,360, 2,567]	[-1,778, 2,691]	[-0.067, 0.174]	[-0.063, 0.182]	[-4.63, 3.48]
Reduced-form estimate	3.55***	0.645	1,710***	194.43	0.040	0.044	-0.345
First-stage	0.710***	0.700***	0.700***	0.702***	0.703***	0.700***	0.705***
$E[Y(0)\|C]$	41.27	22.24	2,351	9,742	0.297	0.234	21.90

Notes: Table presents estimates of discontinuities in Employment Insurance-related outcomes. Sample composition is described by panel headings. Robust, bias-corrected 90% confidence intervals displayed in parentheses. Estimates statistically significant at *** 99 percent, ** 95 percent, and * 90 percent confidence levels, respectively.

Table 5: Employment Outcomes

	Pre-treatment		Post-treatment				
Relative year	-2	-1	0	1	2	3	4
Panel A: Employment indicator							
IV estimate	0.004	0.005	0.018	0.048	-0.005	-0.008	-0.012
	[-0.019, 0.027]	[-0.010, 0.030]	[-0.039, 0.083]	[-0.007, 0.121]	[-0.049, 0.057]	[-0.068, 0.060]	[-0.072, 0.036]
Reduced-form	0.002	0.003	0.011	0.028	-0.003	-0.005	-0.007
First-stage	0.602***	0.594***	0.586***	0.575***	0.599***	0.600***	0.631***
$E[Y(0) \mid C]$	0.993	0.995	0.903	0.871	0.910	0.892	0.837
Panel B: Employment earnings (constant 2023 CAD)							
IV estimate	3,700	3,101	3,507	7,022*	7,277**	-637.76	-1,746
	[-3,015, 11,703]	[-2,035, 10,269]	[-1,791, 9,887]	[1,348, 15,947]	[2,297, 17,533]	[-6,739, 8,811]	[-8,800, 6,290]
Reduced-form	2,208	1,845	2,056	4,064*	4,275**	-386.90	-1,069
First-stage	0.597***	0.595***	0.586***	0.579***	0.587***	0.607***	0.612***
$E[Y(0) \mid C]$	59,339	54,063	33,460	42,098	47,571	54,503	49,651
Panel C: Employment earnings conditional on any employment (constant 2023 CAD)							
IV estimate	3,237	2,488	2,659	5,562*	8,018**	-2,159	-1,825
	[-2,912, 10,852]	[-1,615, 8,989]	[-2,377, 8,742]	[791.11, 14,191]	[2,551, 18,193]	[-8,271, 5,333]	[-8,741, 6,656]
Reduced-form	1,966	1,525	1,587	3,276*	4,642**	-1,323	-1,099
First-stage	0.607***	0.613***	0.597***	0.589***	0.579***	0.613***	0.602***
$E[Y(0) \mid C]$	59,510	54,757	37,048	47,381	52,439	61,412	58,775
Panel D: Log employment earnings							
IV estimate	0.040	0.033	0.094	0.167*	0.055	-0.101	0.042
	[-0.070, 0.170]	[-0.041, 0.153]	[-0.084, 0.318]	[0.031, 0.377]	[-0.125, 0.287]	[-0.281, 0.103]	[-0.150, 0.290]
Reduced-form	0.024	0.020	0.056	0.099*	0.032	-0.060	0.025
First-stage	0.608***	0.615***	0.597***	0.593***	0.588***	0.597***	0.598***
$E[Y(0) \mid C]$	10.78	10.73	10.13	10.41	10.65	10.80	10.61

Notes: Table presents estimates of discontinuities for employment-related outcomes. The estimation sample comprises retroactively extended claimants and new claimants whose EI claims started before October 30, 2016. Robust, bias-corrected 90% confidence intervals displayed in parentheses. Estimates statistically significant at *** 99 percent, ** 95 percent, and * 90 percent confidence levels, respectively.

Table 6: Mechanisms

	Pre-treatment		Post-treatment				
Relative year	-2	-1	0	1	2	3	4
Panel A: Same industrial sector							
IV estimate	0.015	0.020	0.084**	0.169***	0.125**	0.136**	0.130**
	[-0.062, 0.082]	[-0.022, 0.076]	[0.021, 0.175]	[0.089, 0.299]	[0.031, 0.245]	[0.049, 0.253]	[0.046, 0.244]
Reduced-form	0.009	0.012	0.050**	0.098***	0.073**	0.081**	0.078**
First-stage	0.600***	0.599***	0.593***	0.580***	0.584***	0.599***	0.602***
$E[Y(0) \mid C]$	0.836	0.928	0.762	0.534	0.521	0.486	0.440
Panel B: Unionized position							
IV estimate	0.020	0.038	0.063	0.059	0.066	0.014	0.015
	[-0.072, 0.116]	[-0.039, 0.125]	[-0.028, 0.172]	[-0.045, 0.165]	[-0.019, 0.169]	[-0.079, 0.115]	[-0.091, 0.103]
Reduced-form	0.012	0.023	0.037	0.034	0.039	0.009	0.009
First-stage	0.598***	0.608***	0.587***	0.576***	0.596***	0.601***	0.598***
$E[Y(0) \mid C]$	0.307	0.312	0.354	0.348	0.324	0.341	0.327
Panel C: Resides in different census subdivision							
IV estimate	0.033	0.008	0.021	-0.037	-0.056	-0.065	-0.056
	[-0.041, 0.091]	[-0.033, 0.053]	[-0.058, 0.105]	[-0.137, 0.053]	[-0.151, 0.029]	[-0.164, 0.018]	[-0.149, 0.028]
Reduced-form	0.019	0.005	0.012	-0.022	-0.033	-0.039	-0.034
First-stage	0.592***	0.598***	0.587***	0.577***	0.594***	0.604***	0.608***
$E[Y(0)\|C]$	0.096	0.042	0.137	0.240	0.279	0.313	0.350

Notes: Table presents estimates of discontinuities for mechanisms related to employment and earnings. The estimation sample comprises retroactively extended claimants and new claimants whose EI claims started before October 30, 2016. Robust, bias-corrected 90% confidence intervals displayed in parentheses. Estimates statistically significant at *** 99 percent, ** 95 percent, and * 90 percent confidence levels, respectively.

Table 7: Self-Employment

	Pre-treatment		Post-treatment				
Relative year	-2	-1	0	1	2	3	4
Panel A: Self-employment income (constant 2023 CAD, Winsorized)							
IV estimate	-34.00	178.90	-404.72	-731.23*	-1,701***	-974.75*	-952.01*
	[-406.43, 390.76]	[-156.59, 581.10]	[-979.96, 73.72]	[-1,802, -96.39]	[-3,052, -1,143]	[-2,472, -30.46]	[-2,528, -34.04]
Reduced-form	-20.13	107.11	-236.68	-445.13*	-1,011***	-587.62*	-575.13*
First-stage	0.592***	0.599***	0.585***	0.609***	0.594***	0.603***	0.604***
$E[Y(0) \mid C]$	n/a	n/a	n/a	1,701	1,478	1,854	1,690
Panel B: Reports any self-employment income							
IV estimate	0.038	0.007	-0.022	-0.064**	-0.078***	-0.018	-0.017
	[-0.004, 0.074]	[-0.035, 0.043]	[-0.090, 0.026]	[-0.140, -0.018]	[-0.152, -0.040]	[-0.083, 0.039]	[-0.081, 0.035]
Reduced-form	0.023	0.004	-0.013	-0.037**	-0.046***	-0.010	-0.010
First-stage	0.607***	0.592***	0.586***	0.578***	0.594***	0.597***	0.604***
$E[Y(0) \mid C]$	0.023	0.034	0.093	0.143	0.142	0.120	0.136
Panel C: Reports $10,000 or more in self-employment income							
IV estimate	-0.002	0.004	-0.018*	-0.038**	-0.051***	-0.030*	-0.013
	[-0.018, 0.014]	[-0.012, 0.020]	[-0.043, -0.001]	[-0.082, -0.016]	[-0.095, -0.033]	[-0.074, -0.004]	[-0.052, 0.014]
Reduced-form	-0.001	0.003	-0.011*	-0.022**	-0.031***	-0.018*	-0.008
First-stage	0.594***	0.616***	0.597***	0.589***	0.605***	0.603***	0.608***
$E[Y(0) \mid C]$	-0.001	0.009	0.026	0.057	0.055	0.059	0.047
Panel D: Reports $20,000 or more in self-employment income							
IV estimate	-0.004	0.005	-0.015*	-0.016*	-0.027***	-0.021	-0.016
	[-0.018, 0.006]	[-0.007, 0.017]	[-0.033, -0.001]	[-0.048, -0.002]	[-0.056, -0.014]	[-0.053, 0.003]	[-0.047, 0.003]
Reduced-form	-0.003	0.003	-0.009*	-0.010*	-0.016**	-0.012	-0.010
First-stage	0.591***	0.608***	0.589***	0.592***	0.600***	0.605***	0.612***
$E[Y(0) \mid C]$	0.002	-0.000	0.017	0.029	0.025	0.041	0.028

Notes: Table presents estimates of discontinuities for self-employment-related outcomes. The estimation sample comprises retroactively extended claimants and new claimants whose EI claims started before October 30, 2016. Robust, bias-corrected 90% confidence intervals displayed in parentheses. Estimates statistically significant at *** 99 percent, ** 95 percent, and * 90 percent confidence levels, respectively.

Table 8: Transfer Programs

	Pre-treatment		Post-treatment				
Relative year	-2	-1	0	1	2	3	4
Panel A: Receives Employment Insurance income							
IV estimate	0.086**	0.044	-0.001	0.190***	0.075	0.035	0.013
	[0.019, 0.197]	[-0.047, 0.141]	[-0.047, 0.045]	[0.087, 0.291]	[-0.034, 0.176]	[-0.056, 0.116]	[-0.073, 0.111]
Reduced-form	0.052**	0.026	0.000	0.110***	0.044	0.021	0.008
First-stage	0.599***	0.599***	0.594***	0.580***	0.586***	0.614***	0.608***
$E[Y(0) \mid C]$	0.208	0.461	0.951	0.546	0.334	0.358	0.388
Panel B: Receives Social Assistance income							
IV estimate	0.007	0.008	0.010	0.014	-0.036**	-0.012	-0.023*
	[-0.013, 0.033]	[-0.009, 0.027]	[-0.020, 0.042]	[-0.026, 0.050]	[-0.078, -0.012]	[-0.058, 0.018]	[-0.065, -0.003]
Reduced-form	0.004	0.005	0.006	0.008	-0.021**	-0.007	-0.014*
First-stage	0.607***	0.599***	0.586***	0.579***	0.594***	0.598***	0.608***
$E[Y(0) \mid C]$	0.011	0.002	0.014	0.029	0.056	0.047	0.032
Panel C: Receives Canada Pension Plan income							
IV estimate	0.001	-0.004	0.012	0.016	0.021	0.023	0.027*
	[-0.020, 0.022]	[-0.031, 0.015]	[-0.012, 0.040]	[-0.010, 0.046]	[-0.006, 0.056]	[-0.004, 0.058]	[0.003, 0.065]
Reduced-form	0.001	-0.003	0.007	0.009	0.013	0.014	0.017*
First-stage	0.597***	0.597***	0.586***	0.576***	0.595***	0.602***	0.607***
$E[Y(0) \mid C]$	0.016	0.019	0.012	0.006	0.013	0.017	0.021
Panel D: Receives Workers' Compensation income							
IV estimate	0.002	-0.023	0.000	0.006	0.010	0.006	-0.001
	[-0.027, 0.029]	[-0.053, 0.003]	[-0.030, 0.026]	[-0.024, 0.036]	[-0.015, 0.041]	[-0.028, 0.034]	[-0.039, 0.023]
Reduced-form	0.001	-0.014	0.000	0.003	0.006	0.003	-0.001
First-stage	0.599***	0.601***	0.593***	0.584***	0.599***	0.604***	0.609***
$E[Y(0) \mid C]$	0.016	0.030	0.017	0.010	0.014	0.021	0.018

Notes: Table presents estimates of discontinuities for receipt of income from different transfer programs. The estimation sample comprises retroactively extended claimants and new claimants whose EI claims started before October 30, 2016. Robust, bias-corrected 90% confidence intervals displayed in parentheses. Estimates statistically significant at *** 99 percent, ** 95 percent, and * 90 percent confidence levels, respectively.

Table 9: Income and Taxes

	Pre-treatment		Post-treatment				
Relative year	-2	-1	0	1	2	3	4
Panel A: Employment Insurance income (constant 2023 CAD)							
IV estimate	-192.14	-120.84	2,293**	2,327**	991.45*	440.81	-105.81
	[-1,015, 749.12]	[-1,392, 1,059]	[796.55, 4,316]	[719.44, 3,787]	[6.49, 2,163]	[-763.91, 1,668]	[-1,148, 1,073]
Reduced-form	-115.59	-71.52	1,354**	1,344**	606.16	262.99	-64.50
First-stage	0.602***	0.592***	0.590***	0.577***	0.611***	0.597***	0.610***
$E[Y(0) \mid C]$	1,946	4,162	10,927	4,361	2,999	2,999	3,692
Panel B: Labour market income (constant 2023 CAD)							
IV estimate	3,876	3,271	3,264	6,477*	5,651*	-2,019	-2,778
	[-2,896, 11,931]	[-2,216, 10,530]	[-2,007, 9,701]	[512.86, 15,186]	[614.31, 15,693]	[-8,278, 7,027]	[-10,049, 4,878]
Reduced-form	2,311	1,935	1,915	3,743*	3,338*	-1,229	-1,699
First-stage	0.596***	0.592***	0.587***	0.578***	0.591***	0.608***	0.611***
$E[Y(0) \mid C]$	59,491	54,395	34,201	43,993	49,170	57,182	51,992
Panel C: Before tax, after transfer income (constant 2023 CAD)							
IV estimate	3,671	1,958	5,281*	10,388***	7,357**	-246.60	-1,665
	[-3,111, 11,701]	[-4,382, 8,821]	[427.88, 10,969]	[4,536, 18,735]	[2,790, 17,192]	[-6,788, 8,565]	[-8,207, 5,188]
Reduced-form	2,181	1,155	3,099*	5,962***	4,367**	-148.74	-1,015
First-stage	0.594***	0.590***	0.587***	0.574***	0.594***	0.603***	0.609***
$E[Y(0) \mid C]$	63,387	63,184	47,805	50,723	54,901	64,439	64,991
Panel D: Net taxes paid (constant 2023 CAD)							
IV estimate	809.70	382.09	955.57	2,232**	2,023**	-51.62	-592.31
	[-1,262, 3,149]	[-1,469, 2,419]	[-346.08, 2,428]	[576.89, 4,351]	[545.25, 4,480]	[-1,759, 2,368]	[-2,394, 1,392]
Reduced-form	480.11	226.29	561.94	1,282**	1,178**	-31.06	-360.46
First-stage	0.593***	0.592***	0.588***	0.575***	0.583***	0.602***	0.609***
$E[Y(0) \mid C]$	9,634	9,721	6,231	6,980	7,838	9,935	10,145

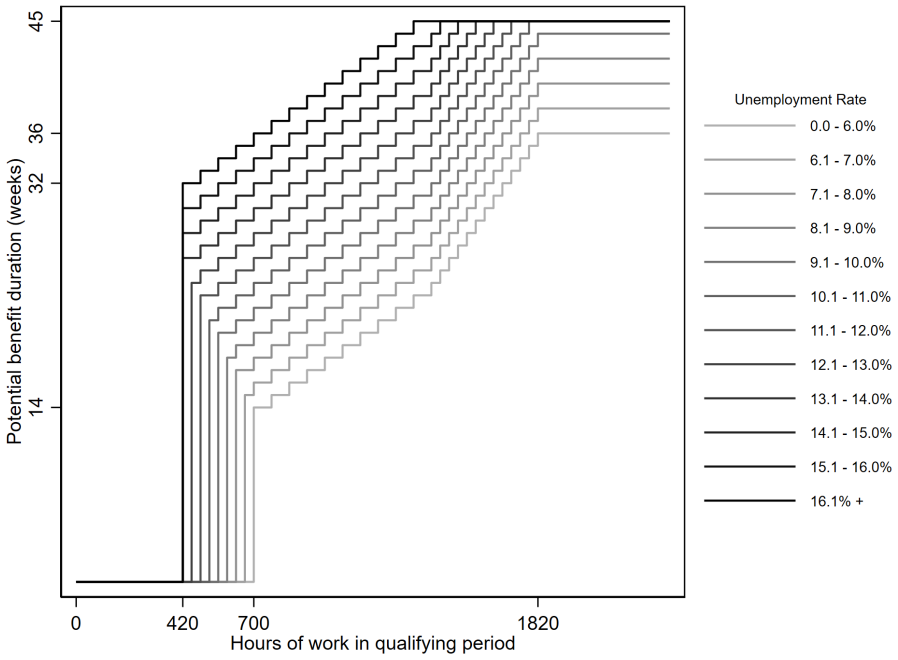
Continued on next page.

Table 9: Income and Taxes

	Pre-treatment		Post-treatment				
Relative year	-2	-1	0	1	2	3	4
Panel E: After-tax income (constant 2023 CAD)							
IV estimate	2,870	1,581	4,361*	8,172***	5,770**	-163.51	-1,037
	[-2,031, 8,733]	[-3,037, 6,566]	[599.18, 8,803]	[3,725, 14,615]	[2,031, 13,172]	[-5,358, 6,466]	[-6,011, 4,047]
Reduced-form	1,707	932.61	2,555*	4,684***	3,408**	-98.37	-631.58
First-stage	0.595***	0.590***	0.586***	0.573***	0.591***	0.602***	0.609***
$E[Y(0) \mid C]$	53,737	53,458	41,580	43,761	47,052	54,449	54,886

Notes: Table presents estimates of discontinuities for different sources of income. The estimation sample comprises retroactively extended claimants and new claimants whose EI claims started before October 30, 2016. Robust, bias-corrected 90% confidence intervals displayed in parentheses. Estimates statistically significant at *** 99 percent, ** 95 percent, and * 90 percent confidence levels, respectively.

Figure 1: Standard Potential Benefit Duration



Notes: Figure depicts the standard weeks of unemployment benefit entitlement as a function of hours worked in qualifying period and the regional unemployment rate.

Figure 2: Employment Insurance Regions

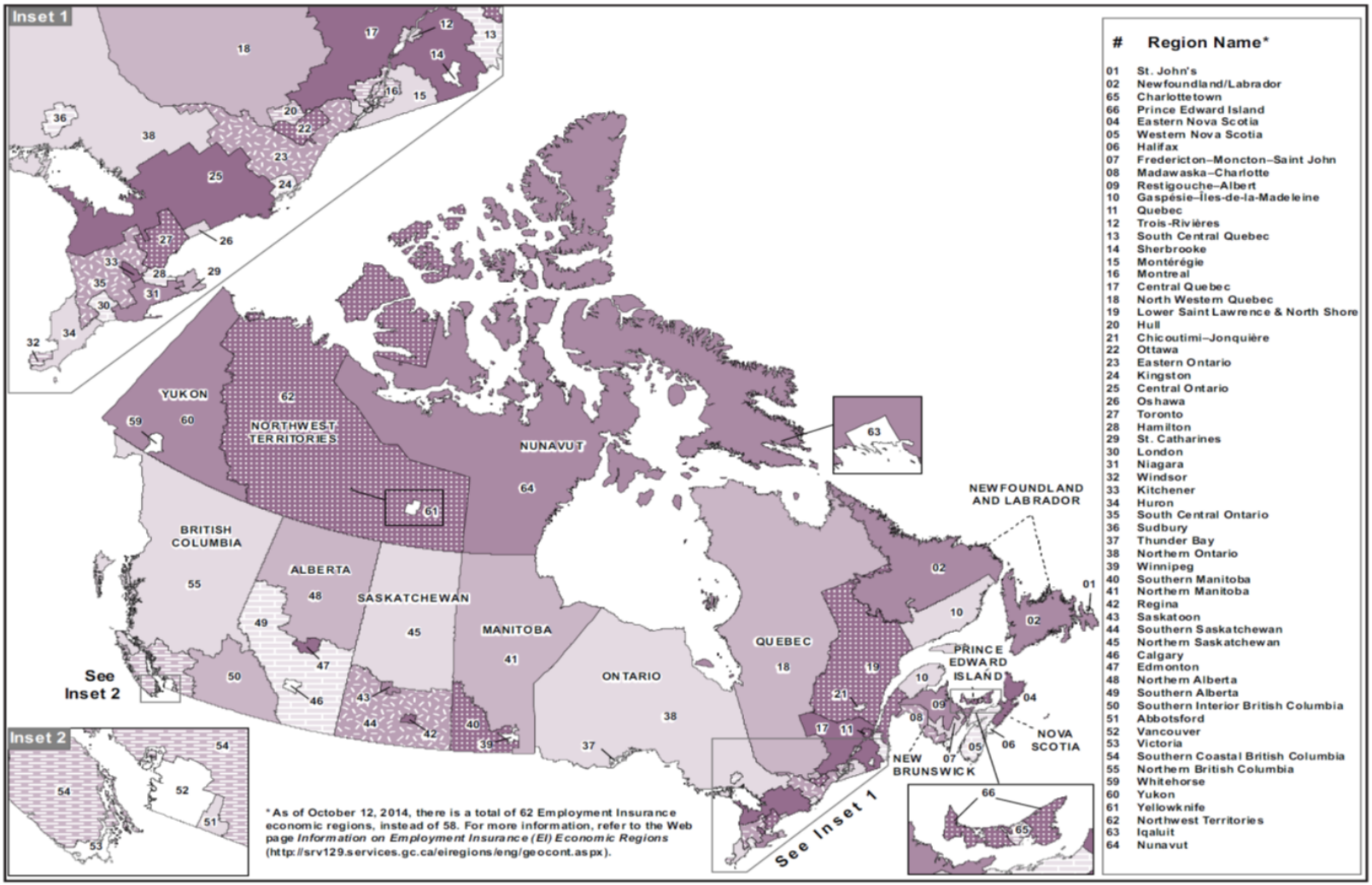
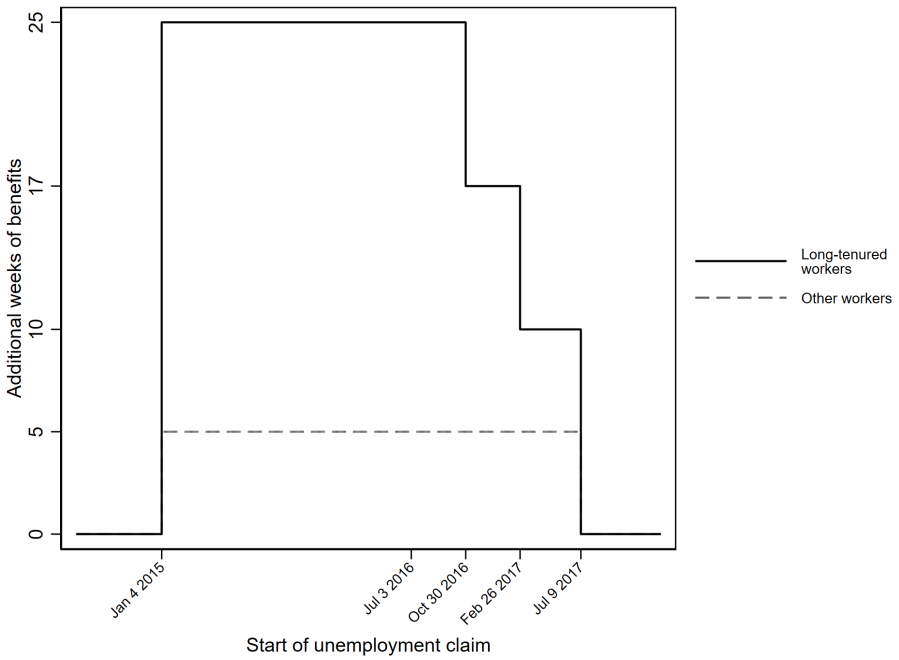


Figure 3: PBD Extension by Claim Date and Worker Type

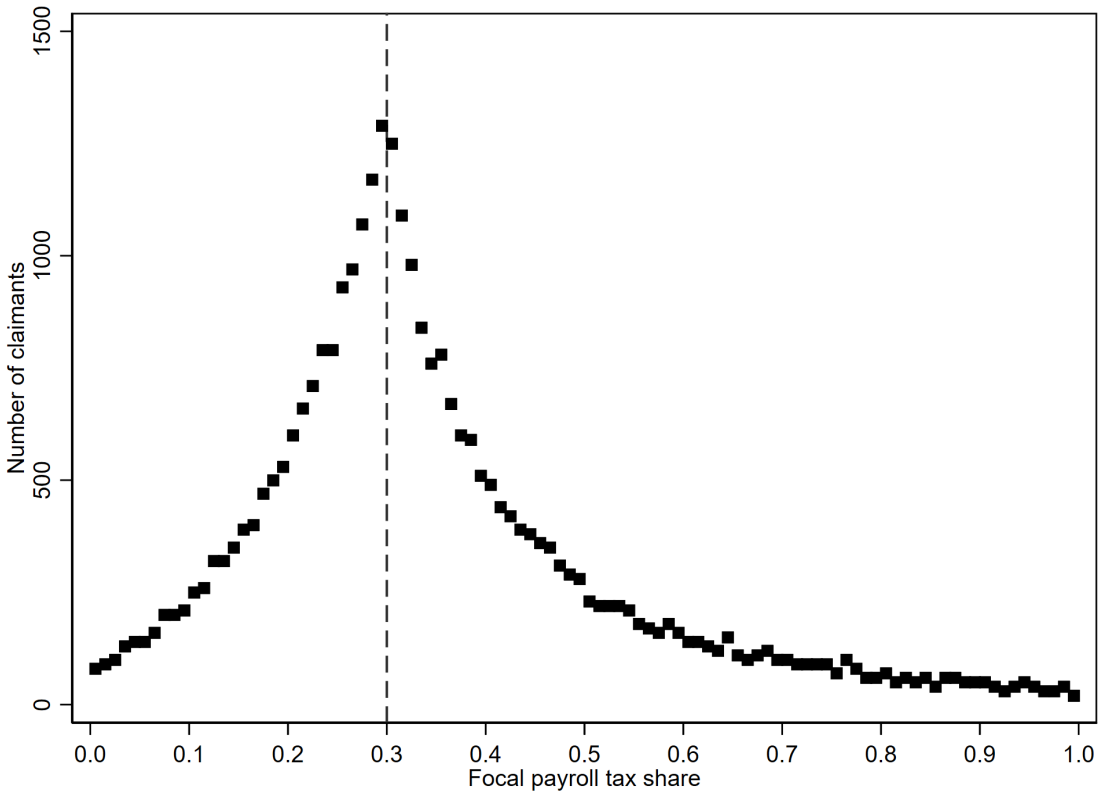


Notes: Figure depicts additional weeks of unemployment benefit entitlement for unemployed workers in 15 regions affected by commodity price downturn based on worker type and the start date of the unemployment claim.

Figure 4: Global Oil Prices

Notes: Figure depicts spot prices for West Texas Intermediate crude oil in current USD.

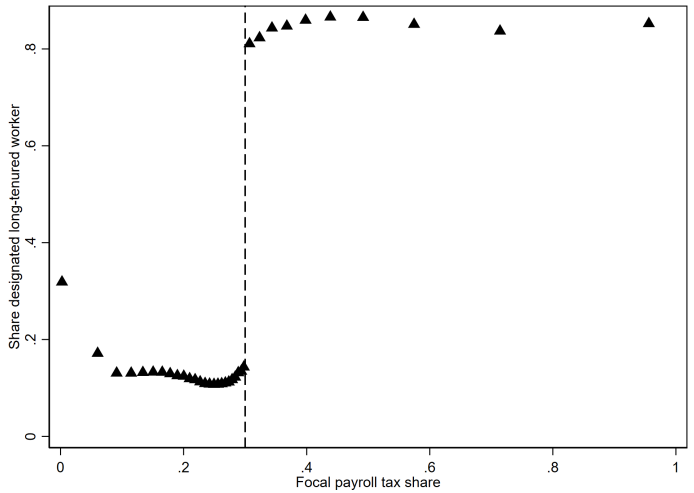
Figure 5: Distribution of Claimants Around Payroll Tax Threshold



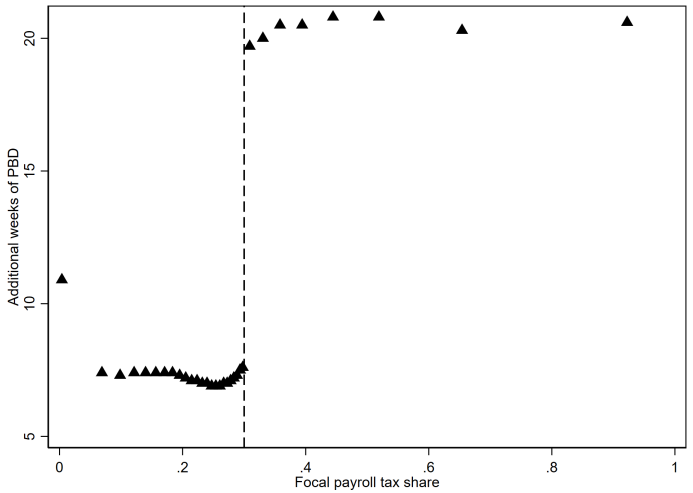
Notes: This figure depicts the number of claimants based on the value of their focal payroll tax share. The sample consists of EI claimants from the ELMLP database who initiated an unemployment claim between January 4, 2015 and July 9, 2017 in the 15 EI regions selected to receive a potential benefit duration extension and who had contributed at least 30% of the maximum EI payroll tax in more than 6 but fewer than 7 of the past 10 years and who had received fewer than 36 weeks of unemployment benefits in the 5 years leading up to their most recent unemployment claim.

Figure 6: Discontinuities

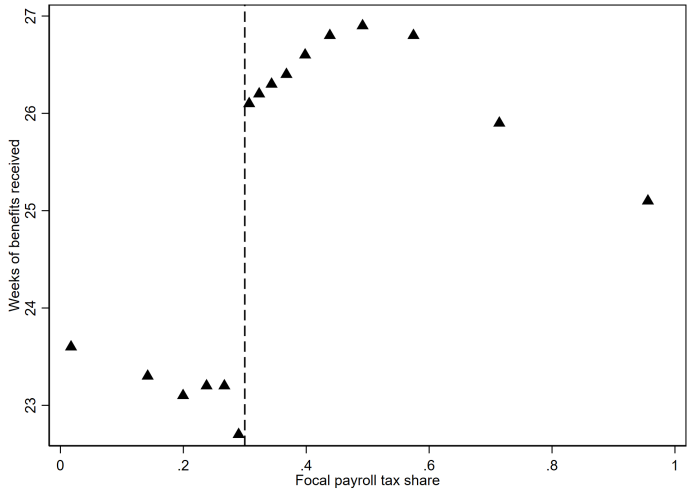
Panel A: Long-Tenured Worker Designation



Panel B: Additional Weeks of Entitlement

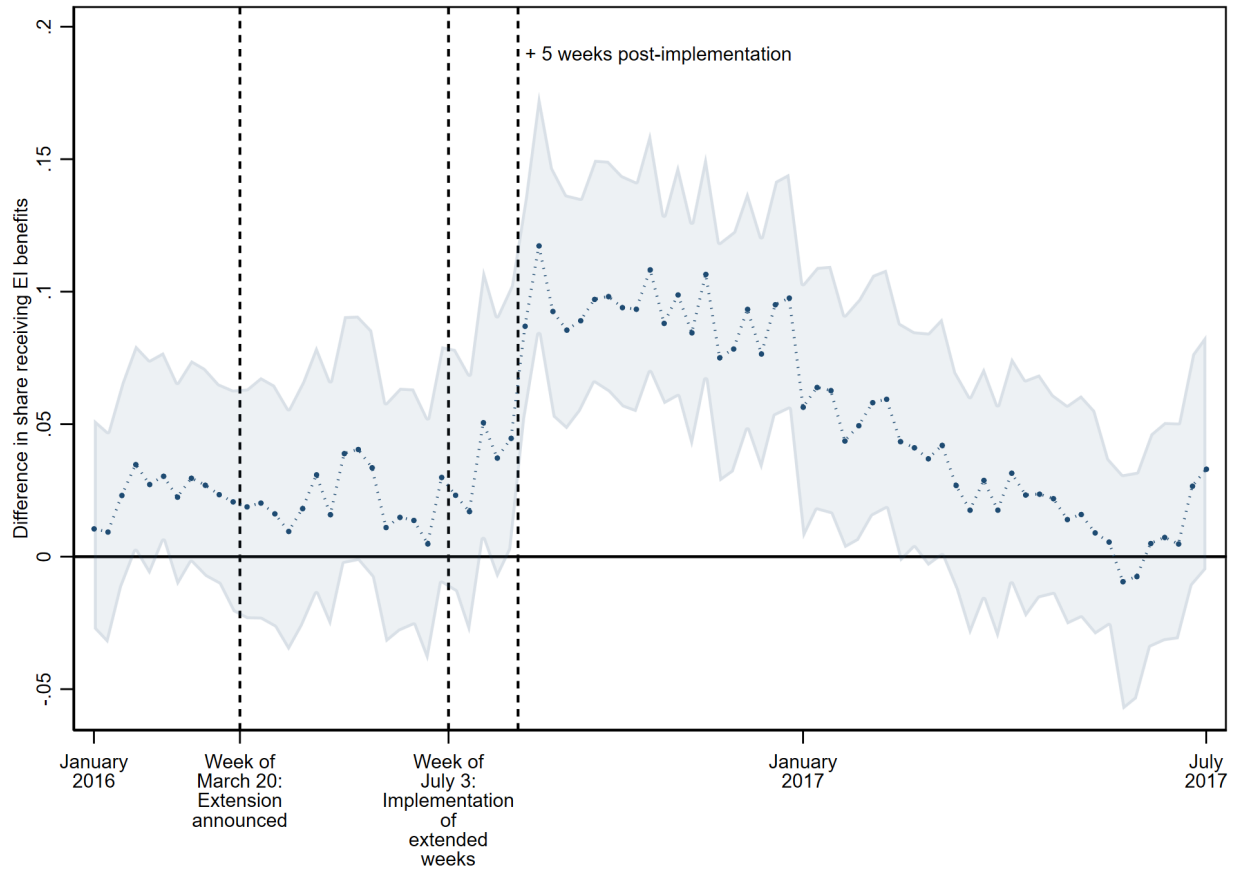


Panel C: Weeks of Benefits Used



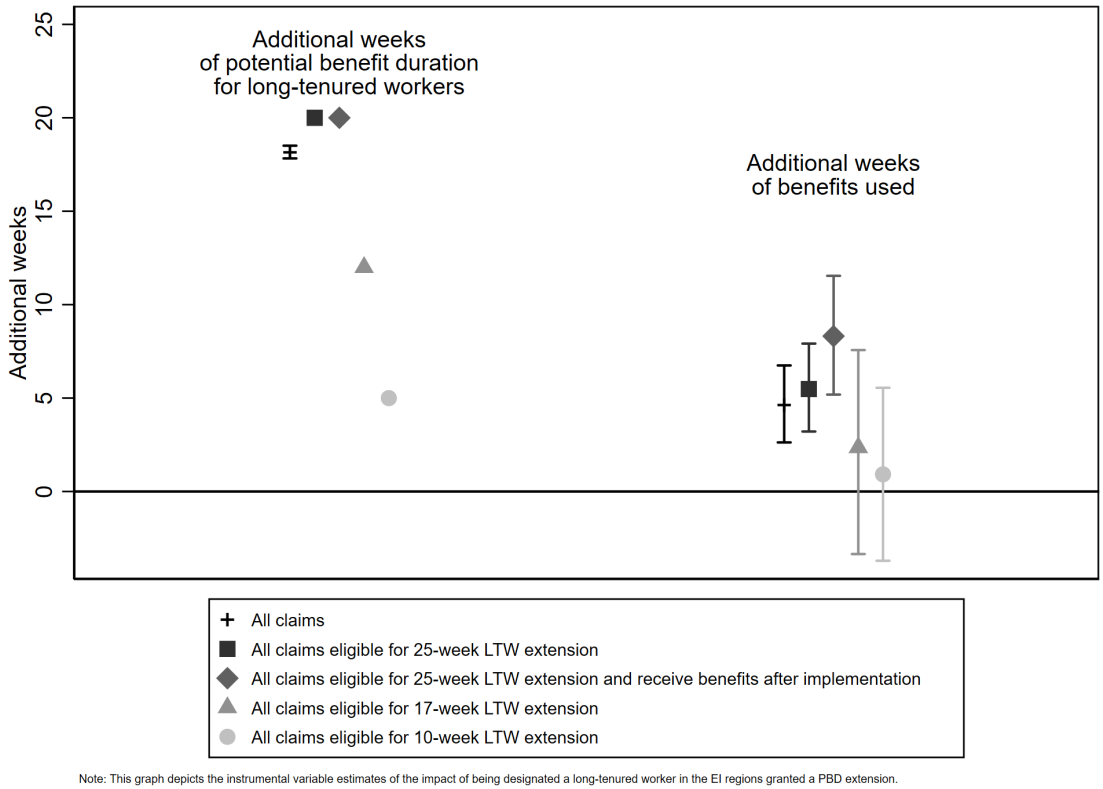
Notes: Figures illustrate the discontinuities in the outcome variable at the 30% focal payroll tax share threshold. The sample consists of EI claimants from the ELMLP database who initiated an unemployment claim between January 4, 2015 and July 9, 2017 in the 15 EI regions selected to receive a potential benefit duration extension and who had contributed at least 30% of the maximum EI payroll tax in more than 6 but fewer than 7 of the past 10 years and who had received fewer than 36 weeks of unemployment benefits in the 5 years leading up to their most recent unemployment claim.

Figure 7: Receipt of Weekly Unemployment Benefits Among EI Claimants from 2015



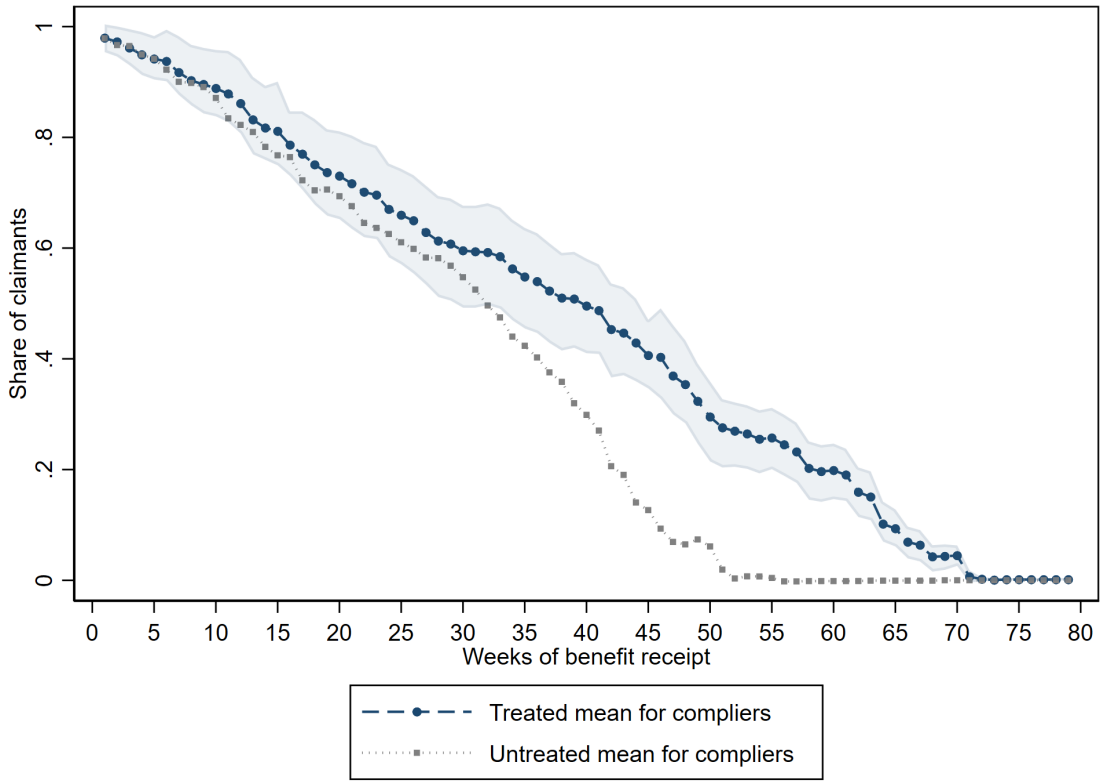
Notes: Figure depicts the difference in the share of EI claimants receiving unemployment benefits each week based on whether or not the claimant is designated a long-tenured worker. The sample consists of EI claimants from the ELMLP database who initiated an unemployment claim between January 4, 2015 and December 31, 2015 in the 15 EI regions selected to receive a potential benefit duration extension and who had contributed at least 30% of the maximum EI payroll tax in more than 6 but fewer than 7 of the past 10 years and who had received fewer than 36 weeks of unemployment benefits in the 5 years leading up to their most recent unemployment claim. The points represent the instrumental variable estimates of the effect of being designated a long-tenured worker; the shaded areas represent 90% confidence intervals generated using robust bias-corrected standard errors.

Figure 8: Additional Weeks of Benefit Entitlement and Usage



Notes: The figure depicts the instrumental variables estimates of the change in potential benefit duration and the change in benefit receipt as a result of being designated a long-tenured worker in the 15 regions granted an extension in the weeks of benefit entitlement. The sample consists of EI claimants from the ELMLP database who initiated an unemployment claim between January 4, 2015 and July 9, 2017 in the 15 EI regions selected to receive a potential benefit duration extension and who had contributed at least 30% of the maximum EI payroll tax in more than 6 but fewer than 7 of the past 10 years and who had received fewer than 36 weeks of unemployment benefits in the 5 years leading up to their most recent unemployment claim. The points represent the instrumental variable estimates of the effect of being designated a long-tenured worker; the capped spikes represent 90% confidence intervals generated using robust bias-corrected standard errors.

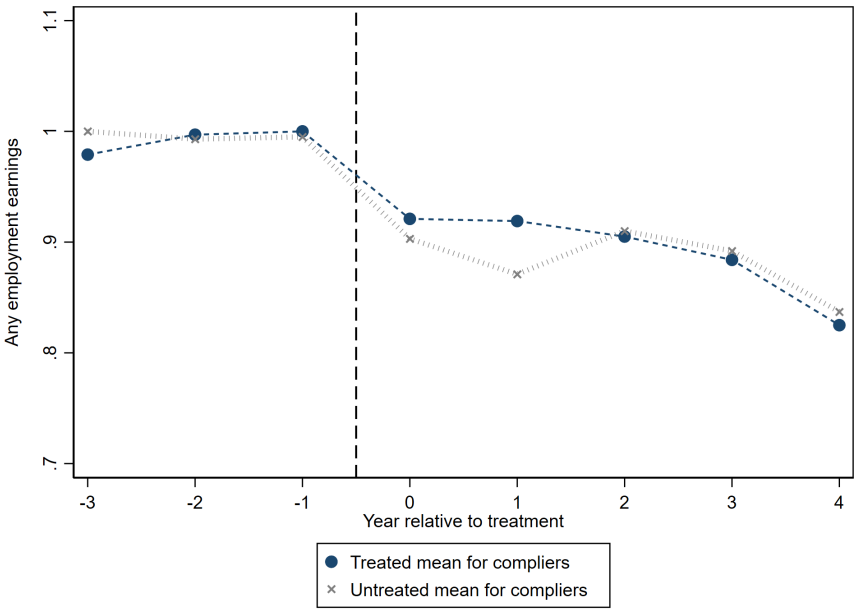
Figure 9: Weeks of Benefits Used



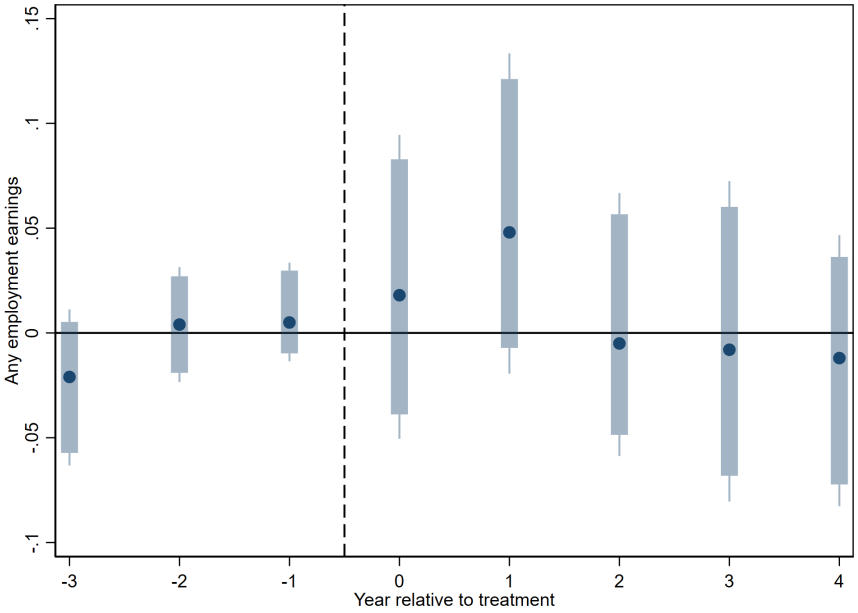
Notes: This figure depicts the share of claimants receiving at least a certain number of weeks of benefits during their focal unemployment spell. The shaded area represents the 90% confidence interval for the difference in means between the treated and untreated compliers. Confidence intervals are generated using robust bias-corrected standard errors.

Figure 10: Any Employment

Panel A: Levels



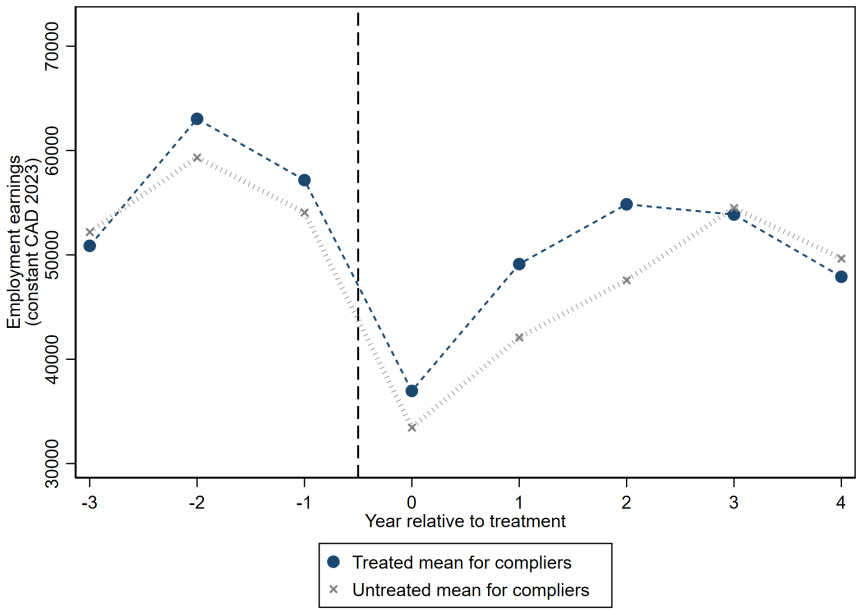
Panel B: IV estimates



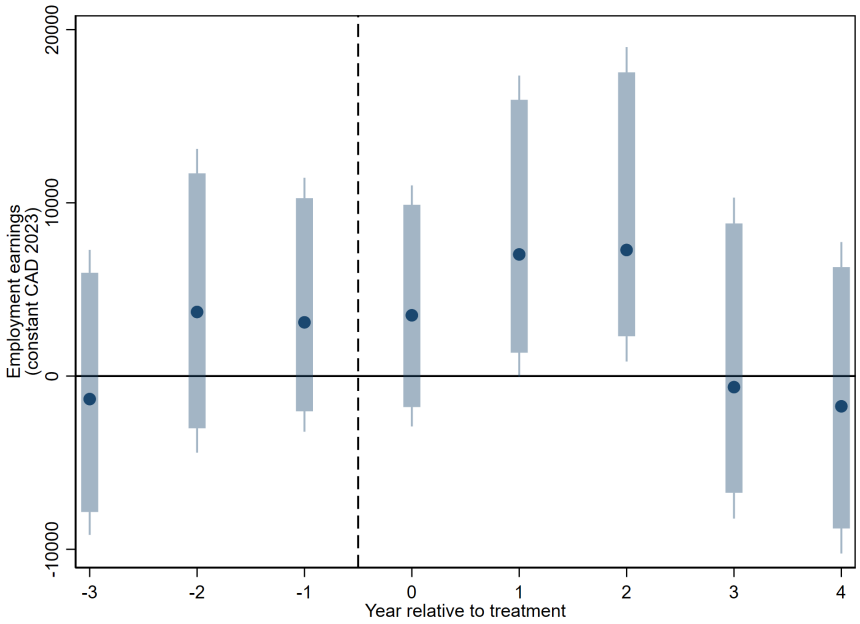
Notes: Panel A plots the mean outcomes for treated and untreated compliers, respectively. Panel B presents the instrumental variables estimate of the effect of additional weeks of benefit entitlement. The shaded bars represent the 90% robust bias-corrected confidence intervals for the instrumental variables estimate; the spikes indicate the 95% confidence intervals.

Figure 11: Earnings from Employment

Panel A: Levels



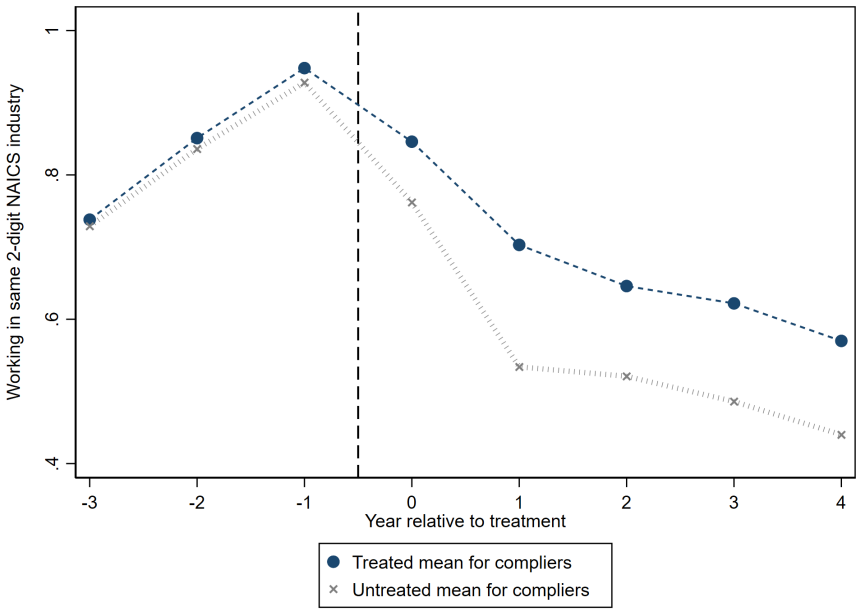
Panel B: IV estimates



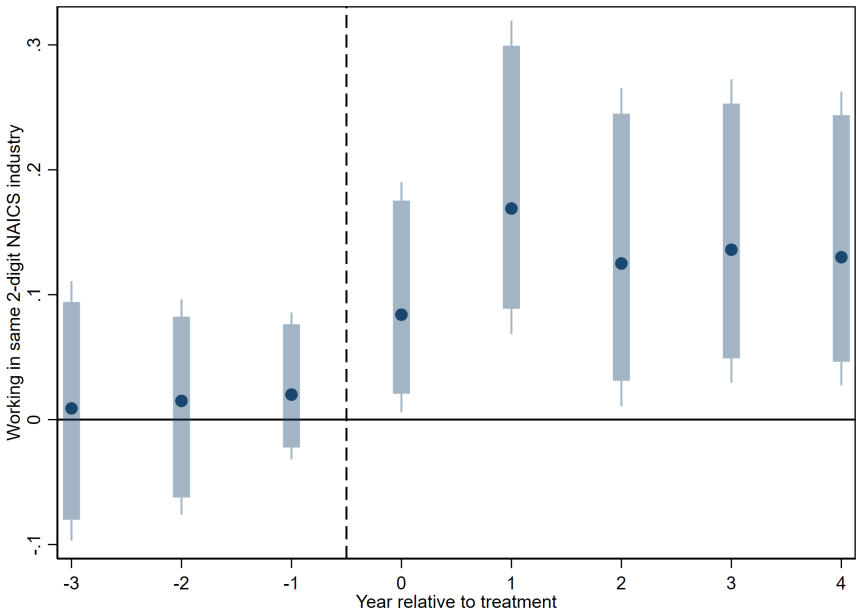
Notes: Panel A plots the mean outcomes for treated and untreated compliers, respectively. Panel B presents the instrumental variables estimate of the effect of additional weeks of benefit entitlement. The shaded bars represent the 90% robust bias-corrected confidence intervals for the instrumental variables estimate; the spikes indicate the 95% confidence intervals.

Figure 12: Industry Attachment

Panel A: Levels



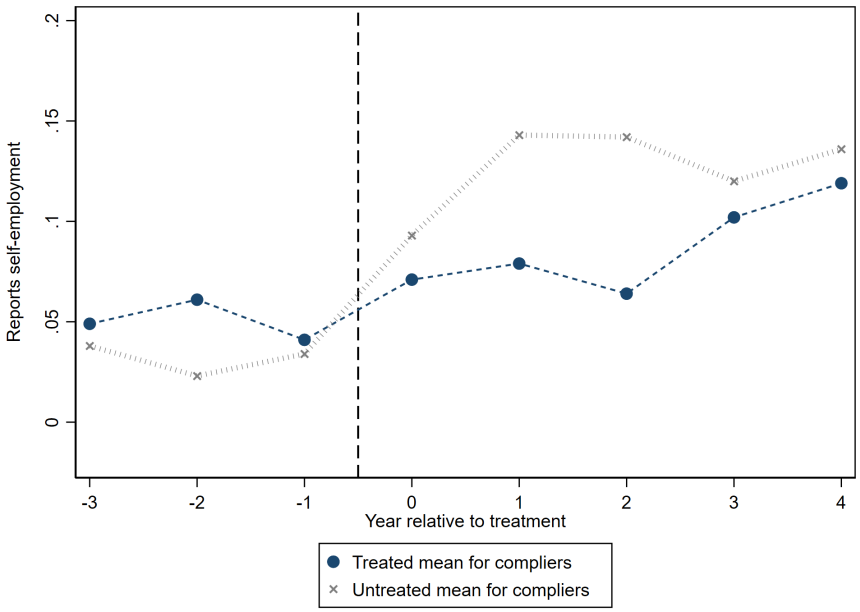
Panel B: IV estimates



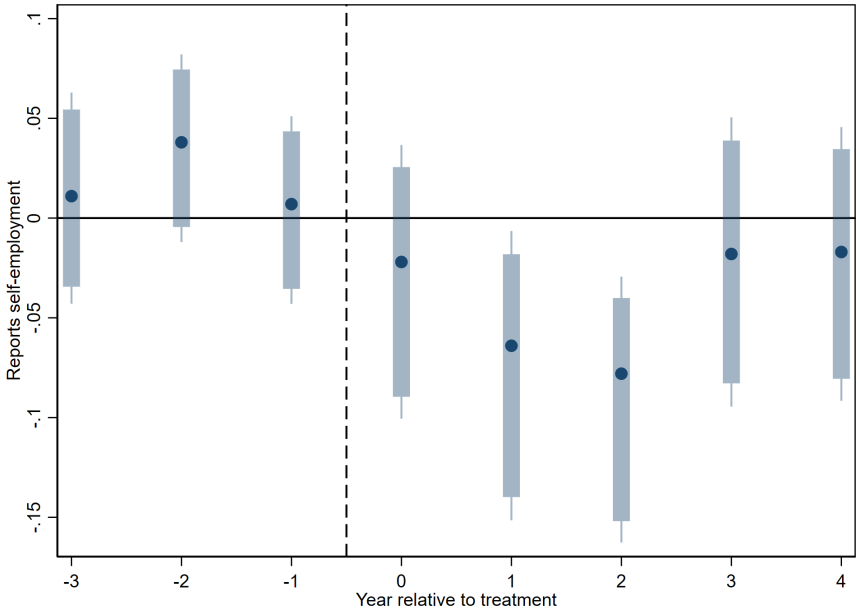
Notes: Panel A plots the mean outcomes for treated and untreated compliers, respectively. Panel B presents the instrumental variables estimate of the effect of additional weeks of benefit entitlement. The shaded bars represent the 90% robust bias-corrected confidence intervals for the instrumental variables estimate; the spikes indicate the 95% confidence intervals.

Figure 13: Reports Any Self-Employment

Panel A: Levels



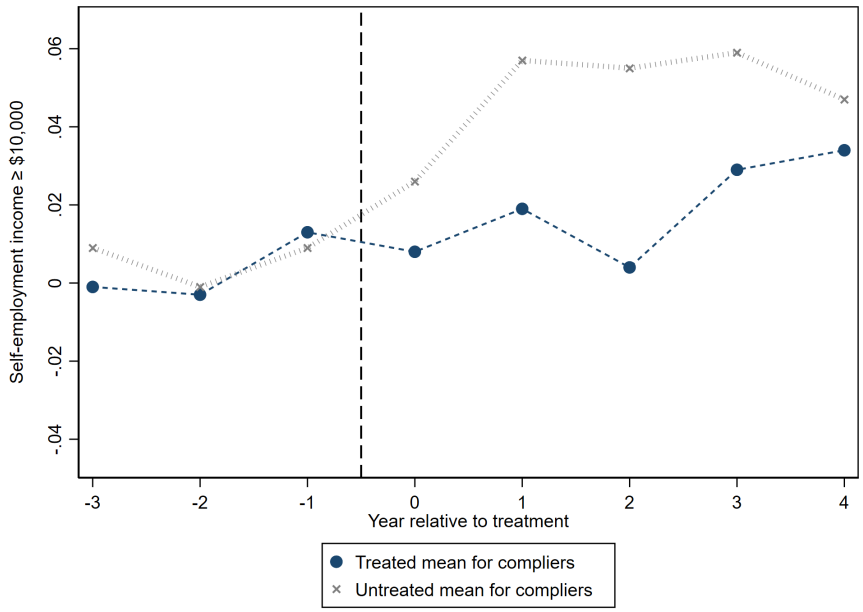
Panel B: IV estimates



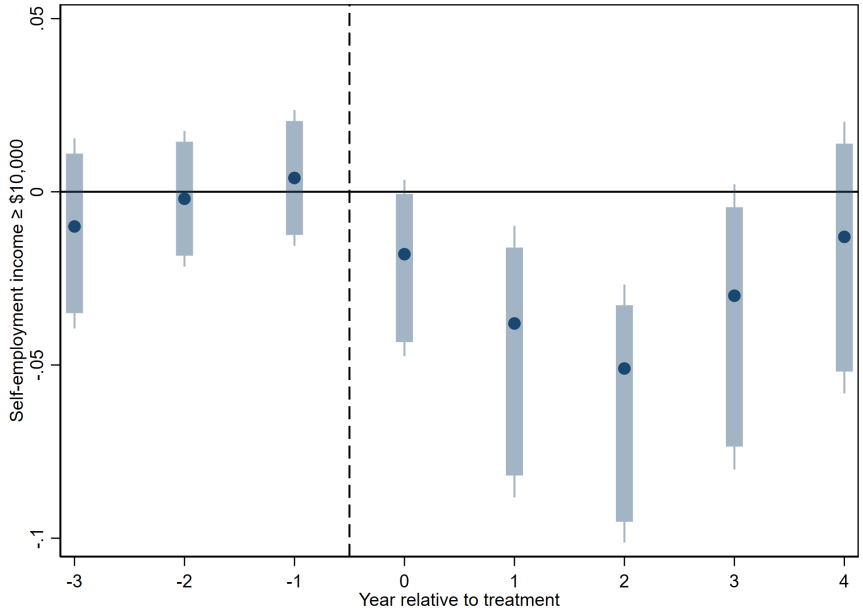
Notes: Panel A plots the mean outcomes for treated and untreated compliers, respectively. Panel B presents the instrumental variables estimate of the effect of additional weeks of benefit entitlement. The shaded bars represent the 90% robust bias-corrected confidence intervals for the instrumental variables estimate; the spikes indicate the 95% confidence intervals.

Figure 14: Self-Employment Income $\geq \$10,000$

Panel A: Levels

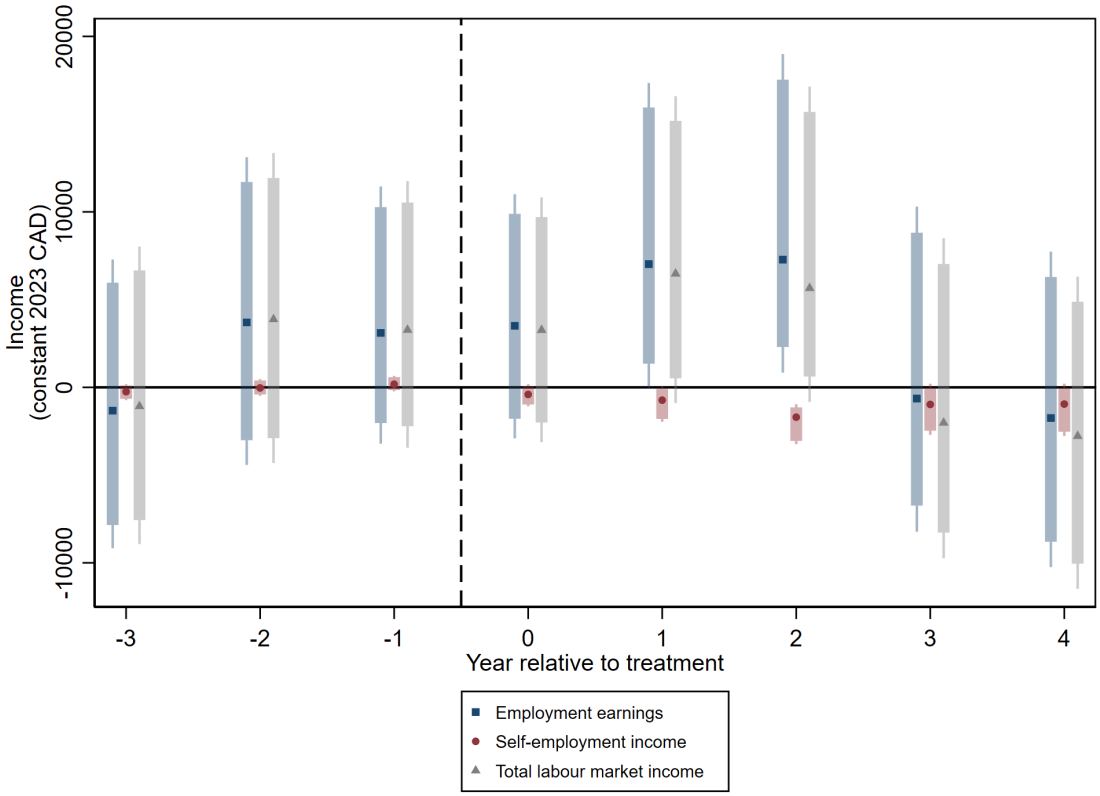


Panel B: IV estimates



Notes: Panel A plots the mean outcomes for treated and untreated compliers, respectively. Panel B presents the instrumental variables estimate of the effect of additional weeks of benefit entitlement. The shaded bars represent the 90% robust bias-corrected confidence intervals for the instrumental variables estimate; the spikes indicate the 95% confidence intervals.

Figure 15: Components of Labour Market Income



Notes: This figure depicts the instrumental variables estimates of the effect of additional weeks of benefit entitlement on employment earnings, self-employment income, and total labour market income. The shaded bars represent the 90% robust bias-corrected confidence intervals for the instrumental variables estimate; the spikes indicate the 95% confidence intervals.

Appendix Tables and Figures

Table A.1: Employment Earnings Impacts for Different Samples

	Pre-treatment		Post-treatment				
Relative year	-2	-1	0	1	2	3	4
Panel A: Primary analysis sample							
IV estimate	3,700	3,101	3,507	7,022*	7,277**	-637.76	-1,746
	[-3,015, 11,703]	[-2,035, 10,269]	[-1,791, 9,887]	[1,348, 15,947]	[2,297, 17,533]	[-6,739, 8,811]	[-8,800, 6,290]
Reduced-form	2,208	1,845	2,056	4,064*	4,275**	-386.90	-1,069
First-stage	0.597***	0.595***	0.586***	0.579***	0.587***	0.607***	0.612***
$E[Y(0) \mid C]$	59,339	54,063	33,460	42,098	47,571	54,503	49,651
Panel B: All retroactively extended claimants and new claimants							
IV estimate	5,099	2,561	3,244	5,293*	6,878**	2,880	2,503
	[-229.27, 12,296]	[-2,360, 8,479]	[-1,155, 8,555]	[111.27, 12,123]	[2,709, 14,740]	[-2,113, 9,778]	[-2,792, 9,323]
Reduced-form	3,095	1,550	1,972	3,171	4,238**	1,816	1,592
First-stage	0.607***	0.605***	0.608***	0.599***	0.616***	0.631***	0.636***
$E[Y(0) \mid C]$	57,088	54,663	37,644	42,160	46,871	51,523	48,427
Panel C: All retroactively eligible claimants and new claimants							
IV estimate	2,508	1,712	156.69	1,209	1,796	-1,152	-2,670
	[-1,614, 7,109]	[-2,026, 6,027]	[-3,701, 4,289]	[-3,381, 5,960]	[-1,365, 6,065]	[-6,050, 3,473]	[-8,401, 2,519]
Reduced-form	1,581	1,073	98.58	758.89	1,183	-741.22	-1,691
First-stage	0.630***	0.627***	0.629***	0.627***	0.659***	0.644***	0.633***
$E[Y(0) \mid C]$	60,692	56,472	49,218	53,673	56,424	60,028	58,656

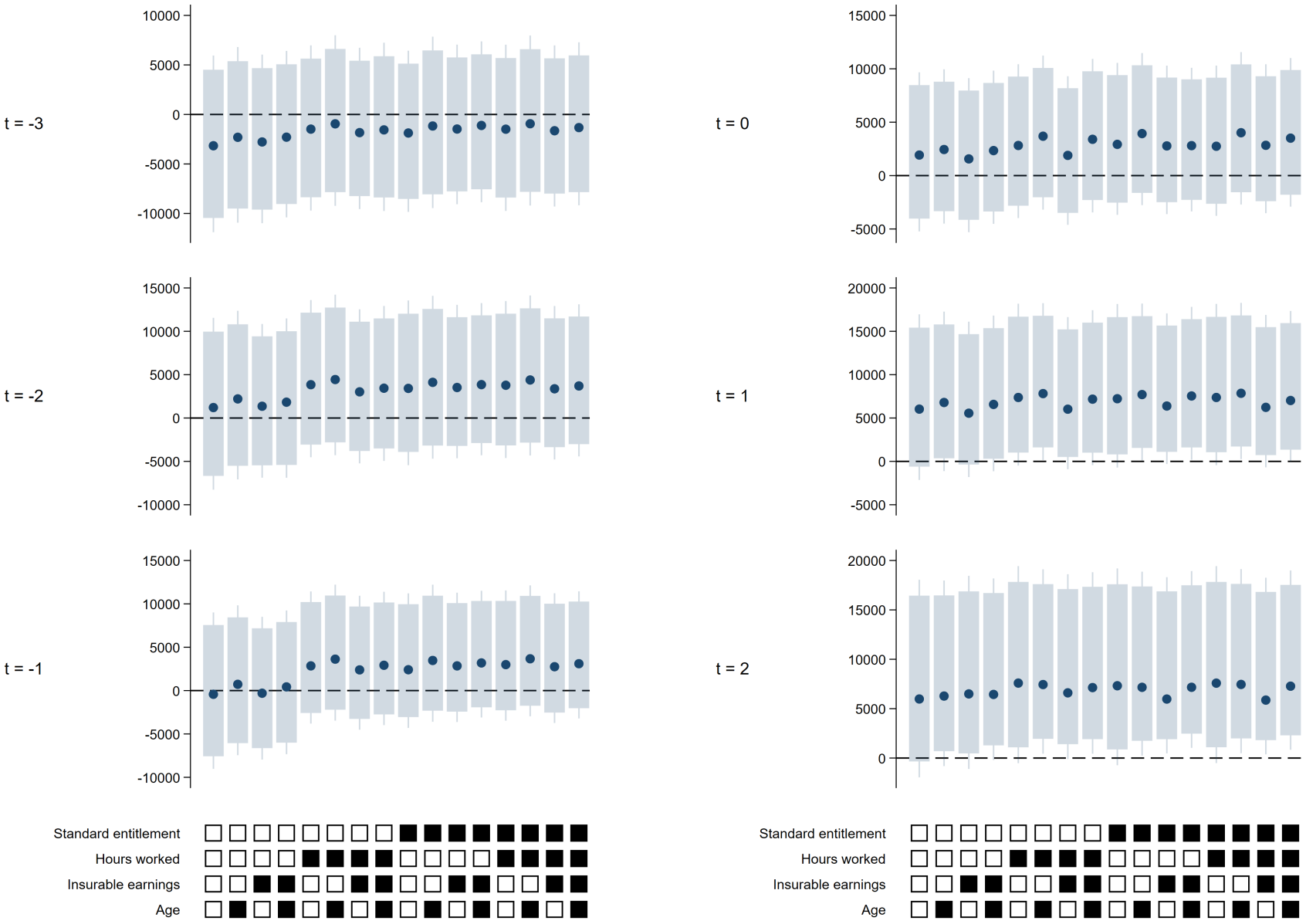
Notes: Table presents estimates of discontinuities for employment earnings in constant 2023 CAD. Robust, bias-corrected 90% confidence intervals displayed in parentheses. Estimates statistically significant at *** 99 percent, ** 95 percent, and * 90 percent confidence levels, respectively.

Table A.2: Estimates for Regions without PBD Extension

	Pre-treatment		Post-treatment				
Relative year	-2	-1	0	1	2	3	4
Panel A: Employment Insurance income (constant 2023 CAD)							
IV estimate	125.50	431.32	142.81	425.79	-286.04	-48.25	249.65
	[-480.93, 719.78]	[-95.16, 1,223]	[-520.23, 822.27]	[-179.55, 1,201]	[-1,141, 440.24]	[-741.20, 583.65]	[-425.45, 912.99]
Reduced-form	83.41	279.56	95.27	284.73	-185.42	-32.32	166.09
First-stage	0.665***	0.648***	0.667***	0.669***	0.648***	0.670***	0.665***
$E[Y(0) \mid C]$	2,705	3,173	7,285	5,004	4,165	3,543	3,107
Panel B: Employment earnings (constant 2023 CAD)							
IV estimate	449.72	1,432	-1,119	-683.95	826.15	1,622	973.05
	[-2,670, 3,857]	[-1,021, 4,256]	[-3,679, 1,347]	[-3,878, 2,204]	[-2,017, 5,145]	[-961.66, 5,954]	[-2,162, 5,231]
Reduced-form	293.40	931.18	-736.24	-454.11	545.35	1,086	650.87
First-stage	0.652***	0.650***	0.658***	0.664***	0.660***	0.669***	0.669***
$E[Y(0) \mid C]$	42,621	45,844	41,998	44,357	48,021	51,798	51,984
Panel C: Same industrial sector							
IV estimate	-0.036	-0.012	-0.020	-0.019	-0.022	-0.014	-0.007
	[-0.083, 0.005]	[-0.037, 0.015]	[-0.051, 0.009]	[-0.068, 0.028]	[-0.077, 0.021]	[-0.065, 0.037]	[-0.052, 0.040]
Reduced-form	-0.024	-0.008	-0.013	-0.012	-0.014	-0.009	-0.004
First-stage	0.660***	0.669***	0.671***	0.662***	0.663***	0.667***	0.675***
$E[Y(0) \mid C]$	0.853	0.938	0.939	0.758	0.744	0.709	0.667
Panel D: Reports self-employment income							
IV estimate	0.014	0.016	-0.018	0.003	-0.017	-0.024	-0.022
	[-0.007, 0.043]	[-0.020, 0.050]	[-0.058, 0.018]	[-0.043, 0.045]	[-0.063, 0.019]	[-0.071, 0.011]	[-0.066, 0.012]
Reduced-form	0.010	0.010	-0.011	0.002	-0.011	-0.016	-0.014
First-stage	0.681***	0.643***	0.651***	0.630***	0.645***	0.649***	0.653***
$E[Y(0) \mid C]$	0.064	0.063	0.109	0.101	0.112	0.106	0.119

Notes: Table presents estimates of discontinuities for outcomes in 47 EI regions that were not selected for a PBD extension. Estimates statistically significant at *** 99 percent, ** 95 percent, and * 90 percent confidence levels, respectively.

Figure A.1: Employment Earnings IV Estimates using Different Specifications



Notes: The figure presents the instrumental variables estimates for the impact of extra PBD for long-tenured workers on employment earnings in constant 2023 CAD using different combinations of control variables in the fuzzy regression discontinuity specification. Circles represent the point estimates. The shaded bars represent 90% confidence intervals generated using robust bias-corrected standard errors. Spikes represent 95% confidence intervals. Black boxes indicate that a variable is included as a covariate in the fuzzy regression discontinuity specification; white boxes indicate the variable is not included.