Manufacturing Investment and Employee Earnings: Evidence from Accelerated Depreciation*

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Abstract

This paper assesses the effects of a tax policy designed to induce manufacturing investment on employee earnings. The particular tax policy we examine is accelerated depreciation, which would lower the cost of capital by accelerating schedules for deducting capital costs from corporate taxes. Specifically, we examine the 2007 accelerated depreciation policy in Canada, which targeted firms acquiring machines and equipment for manufacturing and processing goods. Following the reform, treated firms increase investment in assets eligible for accelerated depreciation and experience increases in employment, sales, and after-tax profits. However, we find that incumbent workers at treated firms experience a decrease in their earnings, entirely driven by those displaced from treated firms and move to smaller firms with lower wage premiums after the reform. Our findings suggest that tax policies designed to promote manufacturing investment may negatively affect existing workers who get displaced, as treated firms increase the relative demand for high-skilled workers to complement capital investment.

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1 Introduction

A tax policy is a popular tool to promote investment in a particular sector, such manufacturing. Accelerated (or bonus) depreciation is a widely used tax policy designed to induce capital investment by allowing firms to deduct costs more quickly. While there exists extensive literature on how bonus depreciation affects investment and employment, surprisingly we have very limited empirical evidence on how such policies affect worker-level earnings.[1] Understanding how tax policies designed to promote capital investment affect workers is an important topic in policy debates, with theoretically opposing views. On the one hand, Harberger (1962) predicts that firm owners entirely bear the corporate tax burden associated with capital, assuming a closed economy with limited capital mobility. On the other hand, subsequent studies emphasize the importance of international capital mobility and predict that workers partly bear tax burdens related to capital costs. Therefore, it is important to accurately measure how tax policies designed to induce capital investment affect workers across different settings using a credible empirical design and employee-level data that can track the same workers over time.

Empirically evaluating the effects of tax polices designed to promote capital investment on worker earnings is challenging in part because it is difficult to isolate the main effects from other confounders, such as business cycles, demand shocks, or productivity. Prior studies exploit across-industry variation in the benefits of bonus depreciation to study the impacts on firm-level outcomes, such as investment, employment, and average payrolls (Zwick and Mahon 2017, Maffini et al. 2019, Curtis et al. 2022, and LaPoint and Sakabe 2022). However, without employee-level data, it is hard to conclude whether changes in firm-level average payrolls in response to bonus depreciation are driven by changes in existing workers' earnings or changes in worker compositions.

This paper studies the effects of an accelerated depreciation policy on worker-level outcomes using administrative employer-employee matched data from tax records. We exploit a 2007 reform in Canada, which accelerated the depreciation schedules for investment in machinery and equipment. To assess the policy impacts, we compare firms that had any positive manufacturing investment prior to the reform with firms that never invested in those assets. The intuition is that firms without any manufacturing investment before the reform would be least likely affected by the reform because such assets are unlikely to be part of their production. While the majority of treated firms is in the manufacturing sector, there are still firms in other sectors that invest in machinery or equipment. Furthermore, we have firms without any investment in machinery or equipment within the manufacturing sector. This allows us to additionally exploit within-sector variation.

[1] While there exists a few studies on how corporate taxes affect wages, including Arulampalam et al. (2012) Suárez Serrato and Zidar (2016), Fuest et al. (2018), Kennedy et al. (2023), and Duan and Moon (2023), there is no paper that examines the effects of an accelerated depreciation policy on wages using the employee-level data.

First, after the accelerated depreciation policy, we find that firms with any positive investment in machinery or equipment prior to the reform acquire new manufacturing assets and increase claims on capital cost allowance (for tax deductions) on these assets, while firms without any manufacturing investment do not respond to such tax incentives. On average, treated firms increase investment in assets eligible for accelerated depreciation by roughly 31,000 CAD on average per year. Since not all firms with positive manufacturing investment respond to the policy, our estimates capture the intent-to-treat. We estimate that 22 percent of the treated firms invest in these eligible assets after the reform on average.

To assess the policy impacts on worker-level outcomes, we compare annual earnings of workers at treated firms with those of workers at control firms before and after the reform. Surprisingly, we find that annual earnings of incumbent workers at treated firms decrease by 1.4 percent on average. Additionally, we find that annual earnings of workers that continue to stay at treated firms do not change much, while the decline in earnings of job movers from treated firms is large and significant. Furthermore, workers at treated firms are not more likely to leave their firms compared to workers at control firms after the reform, implying that the decline in job movers' earnings is not driven by differential displacements of treated workers relative to control workers. In other words, while workers at treated firms are equally likely to get displaced compared to those at control firms, treated workers experience a larger decline in their earnings relative to control workers after the accelerated depreciation policy.

To see whether the changes in incumbent workers' earnings are connected with changes in firm-level characteristics, we estimate the policy impacts on firm outcomes. We find that treated firms increase employment and average payrolls by 4.5 percent and 2.3 percent, respectively, on average per year after the reform. This suggests that the decline in existing workers' earnings is not driven by treated firms replacing workers with capital. We also find that average payrolls for new entrants increased by 1.8 percent on average, implying that the increase in average payrolls is almost entirely driven by treated firms paying higher wages for new workers without paying higher salaries for existing workers. The fact that treated firms raised new workers' wages without increasing incumbent workers' wages suggests that investing in new machinery or equipment may increase demand for operational skills, where the employers need to replace less experienced workers with more skilled workers after accelerated depreciation to increase efficiency.

Importantly, we find that treated firms experience increases in sales, after-tax profits, and EBITDA per worker, implying that these additional investments in capital and new workers result in increased profitability. In contrast to the findings in Kline et al. (2019) and Duan and Moon (2023), we do not find any pass-through of the increased surplus on incumbent workers' earnings. Instead of sharing rents with existing workers, treated firms continue to pay higher salaries

for entrants to recruit more skilled workers that complement capital investment and facilitate their expansion after the accelerated depreciation policy. Taken together, these results suggest that the decline in incumbent workers' earnings is not driven by treated firms substituting labor with capital or losing profits after the reform.

To explore potential mechanisms behind the decline in earnings of job movers from treated firms, we first examine whether these workers move to firms in different industries or become unemployed after leaving their firm. We find that 4 percent of job movers move to firms in different industries or sectors. While those that move to different industries may also switch occupations, the share of movers that switch industries is not large enough to explain the decline in earnings. Furthermore, we find that job movers from treated firms are not more likely to become unemployed relative to control workers, suggesting that unemployment is not the main explanation behind the decline in earnings of job movers either.

Given that most workers that leave treated firms after the reform move to other firms, we examine whether changes in characteristics of new employers may explain the decline in earnings for these workers. On average, new firms have about 53 log points lower employment and 1.7 log points lower firm fixed effects relative to treated firms. Therefore, we argue that the decline in earnings of job movers is likely explained by treated workers moving to smaller firms with lower wage premiums relative to control workers after the reform. We hypothesize that the reason behind job separations may be important in explaining why workers moving from treated firms end up at smaller firms relative to workers that leave control firms. The increased average payroll for new entrants implies that workers displaced from treated firms are more likely to be replaced by new workers with higher skills (i.e., those who can operate new machines and equipment) relative to workers that leave control firms after the reform. These displaced workers from treated firms are more likely to move to smaller firms with lower demand for particular skills to operate new machines or equipment. In other words, while accelerated depreciation does not result in treated firms substituting labor with capital in our setting, it may lead treated firms to increase demand for high-skilled workers to complement their capital investment. This shift in labor demand for different skills may be the key driver behind the decline in earnings of job movers after the reform.

If the shift in demand for high skills is the key channel behind our results, we would expect to see a larger decline in earnings of workers who are fired relative to those who move voluntarily. This is because low-skilled workers are less likely to voluntarily move to other firms during the time period where there is less demand for their skills. Indeed, we find that workers who are fired from treated firms experience much larger declines in earnings and wage premiums, relative to workers who move voluntarily. Furthermore, we find that workers at the lower quartile in the within-firm earnings distribution experience larger decreases in their earnings and wage premiums,

compared to workers at the top quartile of the distribution. These results are consistent with the idea that workers who are fired or with lower skills face larger search frictions to find a new firm when the relative demand for high-skilled workers increases to complement manufacturing investment.

We conduct several robustness tests to strengthen the internal validity of our results. To account for industry-specific or location-specific shocks coinciding with the reform that may drive our results, we additionally control for industry by year fixed effects and commuting zone by year fixed effects. Furthermore, we conduct placebo tests using assets ineligible for accelerated depreciation.

This paper's main contribution to the existing literature is three-fold. First, our paper contributes to growing literature on how tax policies designed to promote capital investment affect worker earnings. While an earlier study exploits across-industry variation in bonus depreciation benefits to assess the effects on firm-level average payrolls (Curtis et al. 2022), to the best of our knowledge, there is no paper that assesses the policy impacts on worker outcomes using the employee-level data. Relative to Curtis et al. (2022), our paper's unique contribution is that we additionally observe worker-level outcomes from tax records. The key advantage of using both firm-level and worker-level data is that we can link worker responses with firm responses to accelerated depreciation, allowing us to delve into potential mechanisms behind changes in worker earnings. In particular, we find that firms that respond to the accelerated depreciation policy significantly increased investment in both labor and capital. This increased investment in turn leads to higher after-tax profits. However, the increased profitability does not result in higher wages for incumbent workers, in contrast to findings in Kline et al. (2019) and Duan and Moon (2023) that show a large pass-through of the extra surplus on incumbent workers' wages. Instead, we find that existing workers suffer earnings losses, mostly driven by those displaced from treated firms after the reform. Leveraging detailed firm balance sheet data, we argue that the decline in earnings of job movers is likely driven by treated firms increasing demand for high-skilled workers, as these firms increase employment and average payrolls for new entrants. Displaced workers or those in the lower quartile in the within-firm earnings distribution suffer the greatest decline in their earnings when the relative demand for high-skilled workers increases after the policy change. Furthermore, we rule out that the decline in incumbent workers' earnings is driven by treated firms substituting labor with capital or losing profits after increasing capital investment.

Relatedly, our paper shows in which setting we may expect to see changes in workers' earnings and firm profits after a tax policy implementation. As firms hire more workers and increase average payrolls, it is unclear how profit margins may change. If the reduction in the cost of capital leads to misallocation of capital and labor to less efficient firms and workers, we may expect that these firms may suffer losses in their profits or productivity. By contrast, hiring more productive workers at higher salaries may increase firms' overall productivity, especially when combined with invest-

ment in productive capital, consistent with our overall findings. Furthermore, if the composition of workers changes after a tax cut (i.e., hiring workers from low socio-economic background), accelerated depreciation may not actually lead to an increase in average payrolls (Curtis et al. 2022). After flexibly controlling for worker fixed effects using our employer-employee matched data, we find that treated firms likely hired more high-skilled workers, as evidenced by the increase in new entrants' average payrolls after the reform. However, this change in demand for high-skilled workers resulted in earnings losses for incumbent workers that get displaced after the reform.

Second, our paper contributes to a literature on how job transitions impact wages. In particular, a large literature studies the role of firm-specific and match-specific components to explain changes in displaced workers' wages (Lachowska et al. 2020; Schmieder et al. 2020). Much of the recent work leverages mass layoffs to identify impacts of job transitions on workers earnings. In this paper, we find that while the accelerated depreciation policy does not lead to differential job displacement for workers at treated firms relative to workers at control firms, those that leave treated firms experience a larger decline in earnings relative to those that leave control firms. We find that workers that get displaced from treated firms are likely replaced by higher skilled workers, as treated firms need these new workers to operate new machines or equipment. Therefore, we find that the reason for job separations is an important factor in explaining persistent declines in earnings for displaced workers.

Third, besides contributing to the literature on accelerated depreciation, this paper complements an extensive literature that has found large effects of fiscal policies on real outcomes; for example, temporary reforms such as bonus depreciation (House and Shapiro 2008a; Zwick and Mahon 2017) have been shown to stimulate aggregate spending. Furthermore, our results are consistent with the findings from a growing empirical literature that has documented substantial investment responses to corporate tax incentives (Ohrn 2018; Chen et al. 2023; Giroud and Rauh 2019; Liu and Mao 2019; Maffini et al. 2019; Curtis et al. 2022; Duan and Moon 2023) and to payout taxes (Poterba and Summers 1983; Moon 2022), and large innovation responses to personal income taxes (Akcigit et al. 2022).

This paper is organized as follows. Section 2 provides institutional details on capital cost allowance system in Canada. Section 3 describes our empirical design and Section 4 shows our main results. Section 5 explores potential mechanisms and discusses economic interpretations of our findings.

2 Institutional Background

This section describes the institutional details on the Canadian tax policy regarding capital cost allowance and the reform that accelerated the schedule for capital cost allowance for a particular asset class relevant for our empirical design.

2.1 Tax Policy on Capital Cost Allowance

In general, firms cannot immediately deduct the full capital costs from their corporate taxes. In Canada, the capital cost allowance (CCA hereinafter) system determines how much of the cost of a capital asset a firm may deduct each year from corporate taxes. The CCA system is analogous to the Modified Accelerated Cost Recovery System (MACRS) in the United States, which determines the tax deduction schedule for each type of qualified capital, with the following differences besides the depreciation rates across different asset classes. Under the CCA system, firms can claim deductions for any given year using the declining balance method and the half-year rule, in which firms can claim CCA on one-half of their net additions in the first year. CCA rates are set so that firms can spread the deduction over the useful life of the asset, so that investment is allocated to its most productive use. For example, the CCA rate for an asset class 1 (buildings) is 4 per cent, and the CCA rate for an asset class 43 (machinery and equipment for manufacturing and processing goods) is 30 per cent. CCA rates are reviewed on a regular basis to ensure that they reflect current information on the useful life of assets.[2]

Based on the declining balance method and the half-year rule, the net-present value of one dollar of CCA for a particular asset class c is computed as:

$$z_c = \frac{1}{2}d_c + \frac{d_c(1-\frac{1}{2}d_c)}{1+r}\left(1 + \frac{1-d_c}{1+r} + \left(\frac{1-d_c}{1+r}\right)^2 + \left(\frac{1-d_c}{1+r}\right)^3 + ...\right) = \frac{d_c(1+\frac{1}{2}r)}{r+d_c},$$

where d_c is the CCA rate for Class c and r is the discount rate.[3] For example, assuming $r = 0.07$, the net-present value of one dollar of CCA on buildings is 0.376, while it is 0.839 for machinery and

[2]The most current information on statutory CCA rates can be found here: https://www.canada.ca/en/revenue-agency/services/tax/businesses/topics/sole-proprietorships-partnerships/report-business-income-expenses/claiming-capital-cost-allowance/classes.html.

[3]Note that without the half-year rule, the net-present value of CCA would be:

$$z_c = d_c\left(1 + \frac{1-d_c}{1+r} + \left(\frac{1-d_c}{1+r}\right)^2 + \left(\frac{1-d_c}{1+r}\right)^3 + ...\right) = \frac{d_c(1+r)}{r+d_c},$$

which is larger than the net-present value of CCA with the half-year rule.

equipment for manufacturing goods. Therefore, changing the depreciation schedule for a particular asset class (i.e., increasing the CCA rate for machinery and equipment) has different impacts on the net-present value of CCA across different firms, depending on their past reliance on that particular asset class prior to the policy change. For example, firms that often invest in machinery and equipment for producing goods would likely benefit more from a change in the CCA rate for machinery and equipment relative to firms that do not use those assets in their production.[4]

2.2 Accelerated Capital Cost Allowance

Accelerated capital cost allowance (ACCA hereinafter) is used to promote investment in certain industries. By accelerating the timing of capital cost deductions, ACCA allows firms to defer taxation and to improve the financial return from investment in particular assets. ACCA is analogous to a bonus depreciation policy in the United States, which accelerates the schedule by allowing firms to deduct a larger amount in the first year.

In 2007, the federal government proposed to re-target ACCA to reflect new economic challenges and provide tax incentives to increase investment in machinery and equipment. Specifically, the CCA rate for Class 43 is accelerated to a 50-per-cent straight-line rate. Taking into account the half-year rule, the assets in Class 43 may be written off on average over a two-year period, starting at the mid-point of the year in which the asset is acquired and ending at the mid-point of the second year after the acquisition, resulting in an effective deduction rate of up to 25 percent for the first year, up to 75 percent for the second year (less any deduction claimed for the previous year), and up to 100 per cent for the third and subsequent years (less any deductions claimed for previous years). The increased rate applies to eligible machinery and equipment purchased on or after March 19, 2007. In particular, this new investment will be categorized under a new asset class (29) of Schedule II. Assuming that firms claim the full eligible CCA each year, the net-present value of one dollar of CCA is now given by: $z_{29} = 0.5 * 0.5 + \frac{(1-0.5*0.5)}{1+r} = \frac{1}{4} + \frac{1}{1.07} * \frac{3}{4} = 0.951$, which is about a 11 percent increase in z. Since 2007, the federal government continued to extend this accelerated depreciation schedule until 2015.

In 2015, the federal government maintained the 50 per cent CCA rate for Class 29, but using the declining balance method, instead of the straight-line rule. Eligible assets are machinery and

[4]Moreover, firms across different sectors and industries typically have a different mix of asset classes. For example, firms in the retail sector tend to have a larger share of assets (i.e., buildings) with longer depreciation schedules due to lower CCA rates, compared to firms in the information technology sector that have a larger share of assets (i.e., computers) that depreciate much more quickly. Therefore, changing depreciation schedules across all asset classes has different impacts on the net-present value of CCA across different industries. In the United States, bonus depreciation policies yield a higher net present value of deductions for industries that have a larger share of assets with longer depreciation schedules (House and Shapiro 2008b; Zwick and Mahon 2017; Curtis et al. 2022).

equipment investment that would otherwise be included in Class 29, but now will be categorized under a new asset class (53). The net-present value of one dollar of CCA based on the declining balance method is 0.908, which is still higher than the original z_{43} prior to the reform in 2007.

Based on the value of z, we can compute the cost of capital based on a measure of user cost widely used in the literature (Zwick and Mahon 2017; Maffini et al. 2019; Curtis et al. 2022):

$$c = \frac{1 - \tau z}{1 - \tau}(r + \delta)$$

where τz represents the net present value of tax deductions due to capital cost allowances for one dollar increase in investment, τ is the marginal corporate tax rate, r is the interest rate, and δ is the economic rate of depreciation. While the top marginal corporate income tax rate changed during our sample period both at the federal level and across different provinces, changes in corporate income tax rates would equally affect both treated firms and control firms, so their impact on the cost of capital is minimal. For treated firms that invest in machinery and equipment for manufacturing goods, the cost of capital decreases from 0.291 to 0.277, which is about a 5 percent decrease on average after the reform.

While the 2007 reform (and the subsequent extensions, including the one in 2015) aimed to induce investment in machinery and equipment for manufacturing by targeting one asset class (initially labeled as 43, but re-labeled as 29 and then 53), the depreciation schedules for other asset classes were not affected by the reform.[5] Firms that typically invest in Class 43 can now claim larger deductions in earlier years if they invest in machinery and equipment for manufacturing and processing goods after the reform; therefore, companies that typically invest in Class 43 (mostly concentrated in the manufacturing and processing sector)are the ones that would now face greater tax incentives to increase investment after the reform. By contrast, firms that never invested in Class 43 prior to the reform would be least affected by the reform, because these firms would not invest in assets related to manufacturing and processing goods regardless of the tax incentives. This distinction forms the basis of our empirical design, which we explain in more details in Section 3.

In practice, firms may not claim any CCA in a given year if they have non-positive taxable income. The net-present value of CCA would be lower if firms do not claim full eligible deductions in each year. Since our estimates capture the effects on all eligible firms, including those not claiming full possible CCA in each period, the estimated reduction in the cost of capital after the reform assuming that firms claim full eligible CCA in each period would be an upper bound.

[5]The two exceptions are (1) Class 43.1 (green technology adoptions) where the CCA rate increased from 30 per cent to 50 per cent in 2007, and (2) Class 50 (computers) where the CCA rate increased from 45 per cent to 55 per cent in 2007 and to 100 per cent in 2009. However, investment responses in these asset classes are small after the reform, suggesting that changes in CCA rates for other asset classes are not important confounders in our setting.

3 Empirical Strategy

This section describes our empirical strategy and data to identify the effects of the accelerated depreciation policy on both firm-level and worker-level outcomes.

3.1 Estimating Policy Impacts on Firm Outcomes

Since the reform accelerated the depreciation rate for Class 43 from 30 per cent to 50 per cent based on a straight-line rune (instead of the declining balance method), any firm that invests in machinery and equipment for manufacturing and processing goods would benefit from this change. However, predicting which firm would invest in machinery and equipment (and therefore would benefit from the increased CCA rate) after the reform is difficult in practice. To identify the effects of the accelerated depreciation policy, we compare the outcomes of firms that invested in Class 43 prior to the reform (from 2002 to 2006), relative to the outcomes of firms that never invested in Class 43 prior to the reform, before and after the policy change in 2007. The intuition is that while firms that invested in Class 43 may or may not invest in machinery and equipment again after the reform, firms that *never* invested in Class 43 throughout the entire pre-reform period are likely the type of firms that do not use machinery or equipment for their production, and thus would be very unlikely to buy those assets just because of the accelerated depreciation. In fact, we verify that firms that never invested in Class 43 do not invest in machinery or equipment even after the reform.

Since not all firms which invested in Class 43 prior to the reform buy machinery or equipment again after 2007, our estimates capture the intent-to-treat (ITT) on the effects of the accelerated depreciation on firm outcomes. One can re-scale our estimates by the estimated change in in the cost of capital (5 percent) to interpret and compare our findings with those from prior studies.

To validate our empirical design and graphically show the reform's effects on firm outcomes, we estimate the following model:

$$Y_{jt} = \sum_{\tau=2002}^{2019} \theta_\tau \cdot \mathbb{1}_{\{t=\tau\}} \cdot Treated_j + \alpha_j + \alpha_t + u_{jt}, \tag{1}$$

where Y_{jt} is an outcome variable for firm j in year t, $Treated_j$ is an indicator for a firm had any positive investment in Class 43 prior to 2007, α_j are firm fixed effects, and α_t are year fixed effects. Each coefficient θ_τ measures the change in the outcome variable Y_{jt} for treated firms relative to control firms in the τ-th year before or after the reform became effective in 2007. θ_{2006} is normalized to be zero. We cluster standard errors at the firm-level.[6]

[6]Our main results are robust to clustering standard errors at the industry by commuting zone level (Appendix A).

One potential concern is that firms that typically invest in machinery or equipment are concentrated in a particular sector compared to firms that never invest in machinery or equipment. While the majority of treated firms is concentrated in the manufacturing sector, we still have a non-trivial share of control firms in the manufacturing sector, as we also have a decent share of treated firms in non-manufacturing sectors, allowing us to exploit the within-sector variation by controlling for sector by year fixed effects. It is also possible that our results are driven by other shocks particular to a certain industry within our treated group. To account for the potential concern regarding industry-specific coinciding with the reform, we additionally control for industry by year fixed effects in our main specification.

We compute and summarize the main estimates of the average tax effects on firm outcomes by estimating the following difference-in-differences model:

$$Y_{jt} = \theta \cdot Post_t \cdot Treated_j + \alpha_j + \alpha_t + u_{jt}, \tag{2}$$

where $Post_t$ is a dummy equal to 1 if it is after the reform year of 2007 and all the other variables are defined in equation (1). We report the estimates from this equation (2), as well as those from equation (1) in Section 4.

For our analysis sample, we impose the following restrictions. First, we focus on Canadian-Controlled Private Companies (CCPCs hereinafter), which account for roughly 98 percent of all firms in our dataset, and use non-CCPCs only as a placebo group in our robustness check. Second, we drop firms that move across provinces or switch industries during our sample period. Third, we focus on the four major provinces (Alberta, British Columbia, Ontario, and Quebec), which make up for almost 90 per cent of all firms in Canada.[7] Finally, we exclude firms in agriculture, finance and real estate, and public administration sectors, mainly because these sectors appear least comparable to the manufacturing sector. In Appendix A, we show that our main results are robust to including firms in the excluded provinces and sectors.

The main identifying assumption behind our empirical design is not the random assignment of firms into treated or control groups. Instead, it is that the affected and unaffected firms' outcomes would have trended similarly in the absence of the policy change. The key threat to this design is that time-varying shocks may coincide with the reform. We present two pieces of evidence that this threat is minimal. First, affected and unaffected firms exhibit parallel trends for the key outcomes prior to the reform. Second, we conduct placebo tests using asset classes ineligible for the accelerated depreciation policy in 2007 (results waiting for approval from the RDC).

[7] Duan and Moon (2023) shows shares based on the number of firms, assets, revenue, and employment across all provinces. Alberta, British Columbia, Ontario, and Quebec make up about 90 percent of all firms in Canada, in terms of shares based on the number of firms, total revenue, assets, and employment.

3.2 Estimating Policy Impacts on Worker Outcomes

To assess the effects of the accelerated depreciation policy on worker outcomes, we estimate a similar model of the following form:

$$Y_{ijt} = \sum_{\tau=2002}^{2019} \theta_\tau \cdot \mathbb{1}_{\{t=\tau\}} \cdot Treated_{ij} + \alpha_i + \alpha_t + u_{ijt}, \tag{3}$$

where Y_{ijt} is an outcome variable for an employee i at firm j in year t, $Treated_{ij}$ is an indicator for an employee i working at a treated firm j defined above, α_i are worker fixed effects, and α_t are year fixed effects. We additionally control for industry by year fixed effects. Each coefficient θ_τ measures the change in the outcome variable Y_{ijt} for treated workers relative to control workers in the τ-th year before or after the reform became effective in 2007. θ_{2006} is normalized to be zero. Standard errors are two-way clustered at both firm and worker levels.

We compute and summarize the main estimates of the average tax effects on workers' outcomes by estimating the following difference-in-differences model:

$$Y_{ijt} = \theta \cdot Post_t \cdot Treated_{ij} + \alpha_i + \alpha_t + u_{ijt}, \tag{4}$$

where $Post_t$ is a dummy equal to 1 if it is after the reform year of 2007 and all the other variables are defined in equation (3). We report the estimates from this equation (4), as well as those from equation (3) in Section 4.

For our analysis sample, we impose the following restrictions after assigning workers into the treatment group or control group based on whether they worked at treated firms or control firms in 2006 (one year before the reform). First, we drop workers with multiple jobs in a given year ("multiple-job holders") so that we focus on full-time workers. Second, we restrict workers to have at least 4,000 CAD in annual earnings to ensure that we study workers with stable income and attached to their firms (Card et al., 2013; Sorkin, 2018). Finally, we impose that all workers were continuously employed in the treated or control firms during the entire pre-event period (2002 to 2006). This tenure restriction is chosen to obtain a sample of workers with attachment to our analysis firms and is similar to tenure restrictions used in the mass layoff literature (Jacobson et al., 1993; Von Wachter et al., 2009; Lachowska et al., 2020). While these restrictions allow us to focus on workers that have stable jobs at either treated or control firms in our analysis sample, we do a robustness check by including multiple-job holders or those making below 4,000 CAD in annual earnings without the tenure restriction, and find qualitatively similar results (see Appendix A).

3.3 Data and Outcome Variables

For empirical analysis, we use the Canadian Employer Employee Dynamics Database (CEEDD), a matched employee-employer dataset that covers the universe of workers and companies in Canada from 2002 to 2019. To prevent the disclosure of confidential information, Statistics Canada requires researchers to round estimates and observation counts. The CEEDD draws information from both individual (T1) and corporate (T2) tax return records, merged with job-level information using T4 employee tax records and Record of Employment (ROE) data, and with firm-level information from the National Accounts Longitudinal Micro-data File (NALMF). This database has rich information on the universe of firms and workers in Canada.

The main outcome variables used in the firm-level analysis are employment, average payroll, net investment rate, total revenue, after-tax profits, and EBITDA per worker. Employment is defined as the average number of employees reported from the T4s. We observe expenditures in different asset classes in a given year from a dataset on capital cost allowance for depreciated capital. Additionally, we define the net investment rate as $\frac{I_t}{K_{t-1}} = \frac{K_t - K_{t-1}}{K_{t-1}} + \delta_t$, where K_t is the book value of tangible assets, net of depreciation, in year t and δ_t is the depreciation rate estimated from our data.[8] After-tax profits are defined as net income after taxes and extraordinary items. EBITDA is defined as a company's earnings before interest, taxes, depreciation, and amortization.

At the worker-level, the key outcome is annual earnings which are aggregated across all employers in a given year. While we include earnings across all employers, we associate workers with the "dominant" employer (i.e., the employer from which the employee receives the highest pay in the year). We use information on workers' gender and age from T1 for heterogeneity analyses.

All outcome variables are winsorized at the first and ninety-ninth percent levels, except for after-tax profits that are winsorized at the fifth and ninety-fifth percent levels, due to disclosure rules set by Statistics Canada.

3.4 Descriptive Statistics

We close this section with descriptive statistics of our data. Panel A of Table 1 shows the means for key outcome variables measured during the pre-reform period (2002 – 2006), separately by treated firms and control firms. On average, treated firms are larger than control firms, in terms of tangible assets, revenue, employment, and average payrolls. However, treated firms and control firms are similar in terms of EBITDA per worker and profit margins. The majority of treated firms

[8]The depreciation rate, δ_t is computed by dividing the amount of depreciated tangible assets in each year based on yearly changes in accumulated depreciation by the lagged tangible assets.

is in the manufacturing sector (69 percent), while the rest of treated firms is in the construction (4 percent), whole sale (9 percent), retail (5 percent), and other services (9 percent), which include administrative support, education, accommodation, and food services. By contrast, the majority of control firms is in the construction (14 percent), retail (19 percent), and other services (30 percent) sectors. Given the difference in the sectoral compositions between treated firms and control firms, we control for industry by year fixed effects to absorb any shocks specific to a particular industry in a given year potentially driving our results.

Panel B of Table 1 shows the means for key variables from the worker sample measured in the year before the reform, separately by the treated group and control group. On average, workers in treated firms earn a bit more relative to workers in control firms although the age composition is similar between the two groups.

4 Results

This section reports estimation results from the difference-in-differences models in Section 3, and presents additional tests supporting the interpretations of the results.

4.1 Investment in Eligible Assets and CCA Claims

Figure 2 plots estimates of θ_τ from equation (1) on investment and CCA claims in eligible assets using our analysis sample. As explained in Section 2, the asset classes eligible for the accelerated depreciation are 29 (from 2007 to 2015) and 53 (from 2016). We also observe how much firms claim CCA for a particular asset class in a given year. Panel (a) shows that investment in Class 29 or 53 followed similar pattern between treated firms and control firms, as Class 29 (and 53) was introduced after 2007 (and after 2016) and investment in these assets was mechanically zero prior to the reform. After 2007, investment in Class 29 or 53 increased significantly for treated firms, compared to control firms, suggesting that firms that had any positive investment in machinery or equipment responded to the accelerated depreciation policy by investing in these eligible assets. Panel (b) shows the share of firms that invest in Class 29 or 53 increased significantly among treated firms relative to control firms after the reform. Panel (c) shows CCA claims for Class 29 or 53 followed a similar pattern between the treated firms and control firms. After 2007, CCA claims for the eligible assets increase significantly for treated firms relative to control firms. Panel (d) shows that the share of firms claiming CCA in Class 29 or 53 increase for the treated group, relative to the control group after the reform.

Columns (1) – (4) of Table 2 present the difference-in-differences estimates on investment in eligible assets and CCA claims. Column (1) shows that treated firms' investment in Class 29 or 53 increased by 30,928.4 CAD after the reform. Column (2) shows that the share of firms investing in Class 29 or 53 increased by 22 percentage points. Column (3) shows that treated firms' CCA claims for Class 29 or 53 increased by 24,164.4 CAD on average per year after the reform. Column (4) shows that the share of firms claiming CCA for Class 29 or 53 increased by 26 percentage points. Taken together, these results imply that firms with any positive investment in machinery or equipment prior to the reform responded significantly to the accelerated depreciation policy by investing in eligible assets and claiming CCA accordingly.

4.2 Worker-level Earnings and Job Transitions

To estimate how the accelerated depreciation policy affects employee earnings, we use the worker-level data which allows us to control for compositions by tracking the same workers over time. In Panel (a) of Figure 3, we plot $\hat{\theta}_\tau$ from estimating equation (3) on workers' annual earnings. As the figure shows, earnings of treated workers trended similarly with those of the control workers in the years prior to 2007, but decreased significantly after 2007. Column (1) of Table 3 shows that workers' annual earnings in treated firms decreased by 1.35 percent on average after 2007.

Next, we examine whether workers move to other firms after their original employers benefit from the accelerated deprecation policy. Panel (b) of Figure 3 and Column (2) of Table 3 shows that the probability of moving to another firm for treated workers remained unchanged after 2007, relative to control workers. Panel(c) of Figure 3 shows that while the annual earnings initially dipped in the first few years for workers that continued to stay at treated firms, relative to workers who stayed at control firms, the annual earnings eventually go back to the pre-reform level after 2013. Column (3) of Table 3 shows that annual earnings for stayers in treated firms did not change much on average after the reform.

By contrast, Panel (d) of Figure 3 shows that annual earnings for workers that leave treated firms after the reform experience a significant decline in their annual earnings. Column (4) of Table 4 shows that annual earnings decrease by 3 percent on average for job movers from treated firms after the reform. This result, in conjunction with the result for stayers, suggests that the decline in the incumbent workers' earnings at treated firms was almost entirely driven by the job movers. In Section 5, we explore potential mechanisms behind the decline in job movers' earnings.

4.3 Employment, Average Payrolls, and Net Investment

Next, we turn to firm-level responses to the accelerated depreciation policy to help understand responses in worker-level earnings. Figure 4 plots estimates of θ_τ from equation (1) on employment, average payrolls separately for all workers and for new entrants, and net investment rate using our analysis sample. Panels (a) – (d) show that treated firms followed a similar pattern as control firms before 2007 for each of these outcomes. After 2007, we observe significant increases in employment and average payrolls for treated firms, relative to control firms, suggesting that the accelerated depreciation policy led treated firms to hire more workers. Furthermore, the increase in average payrolls for new entrants is consistent with the increase in overall average payrolls, without much changes in incumbent workers' earnings on average. The increase in net investment rate in Panel (d) is consistent with the increase in capital investment after the reform.

Table 4 presents the difference-in-differences estimates on these firm-level outcomes. Columns (1) and (2) show that treated firms' employment and average payrolls increased by 4.5 percent and 2.3 percent, respectively, implying a significant response in demand for labor. Columns (3) shows that treated firms' average payrolls for new entrants increased by 2.1 percent. Column (4) shows that net investment rate increased by 1.4 percentage points on average, as treated firms invest in new machinery and equipment after the reform. Overall, these firm-level results are consistent with the idea that treated firms responded positively to the reform by increasing manufacturing investment and that the decline in incumbent workers' earnings is not driven by treated firms substituting labor with capital, as both employment and average payrolls increased after the reform.

4.4 Sales and Profitability

To delve into a potential mechanism behind the responses in existing workers' earnings, we assess changes in firms' sales and profitability after the reform. Specifically, we examine whether the decrease in worker earnings is driven by decreases in firms' profitability, in the form of rent-sharing. Figure 5 plots estimates of θ_τ from equation (1) on total revenue, after-tax profits, and EBITDA per worker using our analysis sample. Panels (a) – (c) show that treated firms followed a similar pattern as control firms before 2007 for each of these outcomes. After 2007, we observe significant increases in these outcomes for treated firms, suggesting that the accelerated depreciation policy led these firms to experience increases in sales, after-tax profits, and EBITDA per worker. While increases in after-tax profits partly reflect a mechanical change from the tax deductions, the increased EBITDA per worker (albeit not statistically significant) suggests that treated firms became more profitable after increasing investment in response to the accelerated depreciation. Note that the responses in profitability and productivity happen more gradually compared to investment re-

sponses as shown in Figure 3, which occur immediately after the reform. The differences in the timing of these responses suggest that the increase in investment is connected with the increased profitability.

Table 5 presents the difference-in-differences estimates on these firm-level outcomes. Columns (1) and (2) show that treated firms' total revenue and after-tax profits increased by 3.3 percent and 30,351.3 CAD, respectively, on average per year, implying large and significant responses in firm growth and profitability. Furthermore, EBITDA increased by 66 CAD per worker, suggesting that firms became productive after the reform. Overall, these results suggest that the decline in incumbent workers' earnings is not driven by treated firms becoming less profitable after the reform, leading to fewer rents to be shared with workers.

4.5 Robustness and Internal Validity

We conduct several robustness checks to strengthen the internal validity of our results. First, we include sector by year fixed effects (instead of industry by year fixed effects) to control for sector-specific shock potentially coinciding with the reform that might drive our results. Second, we additionally include commuting zone by year fixed effects to absorb any local labor market specific shock in a given year potentially driving our findings. Third, we include previously excluded sectors (agriculture, finance, and public administration), and previously excluded provinces. Finally, in the preferred specification of equations (3) and (4), we impose tenure restrictions and drop multiple-job holders or those making below 4,000 CAD in annual earnings to focus on full-time workers with stable jobs. We relax these restrictions and include the previously excluded workers. Finally, we conduct placebo tests using assets that are ineligible for the accelerated depreciation policy.[9] Results from these robustness tests are included in Appendix A.

5 Potential Mechanisms and Economic Interpretations

The previous section showed that while the accelerated depreciation policy led to positive firm outcomes, it also resulted in the decline in incumbent workers' earnings, mostly driven by those that leave treated firms after the reform. This section discusses potential explanations behind the decline in earnings of job movers after the reform. Furthermore, we use our estimates to conduct a cost-per-job calculation for welfare implications.

[9] The results from the placebo tests are waiting for approval from the RDC.

5.1 Changes in Employer Characteristics via Job Transitions

As shown in Section 4, the decline in earnings of workers at treated firms is almost entirely driven by those who move to other firms after the reform. Since the majority of these workers leave their firms involuntarily, we follow the job displacement literature to see whether these workers move to firms with lower wage premiums (employer fixed effects).

Panels (a) and (b) of Figure 6 show that a significant share of workers moves to firms in different industries or sectors. Columns (1) and (2) of Table 6 shows that the share of workers that switch industries or sectors increases by 4 percentage points after the reform among the job movers. Panel (c) of Figure 6 and Column (3) of Table 6 show that the job movers from treated firms are not more likely to become unemployed relative to job movers from control firms. This result is important as it suggests that the decline in earnings of job movers from treated firms is not driven by these workers losing jobs after leaving their firm.

Panel (a) of Figure 7 shows that workers that leave treated firms after the reform move to smaller firms on average. Because of the tenure restriction, any change in pre-event firm characteristics is driven by yearly changes in target firms' characteristics (i.e., changes in firm size). The change in firm characteristics in event year ($t = 0$) still reflects the change in target firms' characteristics, given that the first job transition happens one year after the event. Starting from one year after the event ($t = 1$), changes in firm characteristics reflect both changes in new employers where treated workers moved, and changes in treated firms of workers who had not left yet. Column (1) of Table 7 shows that these workers move to firms with 52.9 log points lower employment on average. Next, we estimate firm-specific wage premiums to see whether these firms have lower wage premiums on average.

5.1.1 Employer Fixed Effects

Following a growing body of literature that assesses the importance of firm-specific premiums in determining wages, we examine whether there is any change in employer fixed effects for treated workers who move to other firms after the reform. Tracking changes in firm fixed effects provides us one way to characterize the transitions that workers at treated firms make after the reform and put their earnings losses into context. For example, if workers are moving to firms with lower wage premiums, then the loss of the employer wage premium from the old firm would explain the decline in workers' earnings (Lachowska et al. 2020).

Using our matched data, we estimate an employer fixed effect for each firm. We then characterize a firm-specific wage premium of the old and new employer for each worker who undergoes

a separation after the reform to understand the decline in earnings of workers who move to other firms. Our implementation of the AKM model regresses log earnings observed for individual i working at firm j in year t (y_{ijt}) on employer-specific fixed effects which reflect firm characteristics that result in above- or below-average earnings for all workers at firm j ($\phi_{j(i,t)}$), individual fixed effects (ω_i), and year effects (τ_t):

$$y_{ijt} = \phi_{j(i,t)} + \omega_i + \tau_t + u_{ijt} \tag{5}$$

We can then assess the role played by employer fixed effects by estimating an analogue to equation (1), substituting in as the outcome variable the estimated firm fixed effects $\hat{\phi}_j$. The goal is to estimate the share of earnings losses following job transitions that can be attributed to a mover's reemployment by an employer with a different $\hat{\phi}_j$ than the employer from which the mover left.

Panel (b) of Figure 7 shows that workers that leave treated firms after the reform move to firms with lower wage premiums on average. Column (2) of Table 7 shows that these workers move to firms with 1.7 log points lower firm-specific wage premiums on average. To summarize, we find that workers that transition jobs after the accelerated depreciation policy move to smaller firms with lower wage premiums. Next, we explore potential mechanisms behind the decline in worker earnings and wage premiums for job movers after the reform by looking at the type of job transitions and worker characteristics.

5.1.2 Heterogeneity by Type of Job Separations

After the reform, some workers voluntarily exit the firm while others are displaced. In our sample, roughly three quarters of these workers leave treated and control firms involuntarily after the M&A event.[10] If the decline in earnings of job movers from treated firms is driven by their skills being incompatible with new machinery and equipment, we would expect displaced workers to experience larger declines in earnings and wage premiums, relative to workers that voluntarily leave their firm. Note that displaced workers may experience a larger decline in earnings relative to those that voluntarily leave their firm in general due to search frictions. However, since control workers are also displaced from their firm after the reform, it suggests that job movers from treated firms may have even larger search frictions relative to displaced workers from control firms. Panel (a) of Figure 8 show that earnings of workers at treated firms that move to other firms, separately for those that move voluntarily and for those that move involuntarily. Relative to their control workers, those that move involuntarily show a larger decline in earnings compared to those that move voluntarily

[10] In some cases, the reason for exit is missing. We omit these individuals from this calculation, although the effects on earnings for these individuals are much smaller and closer to zero.

after the event. Panel (b) shows that these workers move to firms with lower wage premiums on average. Table 8 confirms that the decline in worker earnings and firm fixed effects is larger for workers that get displaced from treated firms. Therefore, these results are consistent with the idea that job movers from treated firms have particularly larger search frictions, likely due to their skills incompatible with new machinery and equipment for manufacturing goods.

5.1.3 Heterogeneity by Worker Characteristics

Relatedly, we consider heterogeneity by the type of workers. Intuitively, lower-skilled workers that get displaced after the reform may suffer greater losses in their earnings and firm-specific wage premiums, likely due to the lack of skills necessary to operate new machines or equipment. We exploit variation by the within-firm earnings distribution. Intuitively, workers at the upper end of the earnings distribution may have skills that are valued more by their original employers that invest in new machinery and equipment after the accelerated depreciation. In Figure 9, we plot the impacts for job movers across different quartiles of the within-firm earnings distribution. We find that the decline in earnings is concentrated among individuals in the bottom 75 percentile, and we find no impacts on earnings for workers in the top 25 percentile.

The differences are also due to bottom quartile workers moving to worse-paying firms on average. As can be seen in Panel (b) of Figure 9, job movers at the bottom 75 percentile are moving to firms with lower levels of AKM effects. Table 9 shows that job movers at the bottom quartile of the within-firm earnings distribution move to firms with 1.8 log points lower wage premiums, and experience 5.2 log points in their annual earnings after the reform. Our worker heterogeneity results are consistent with the idea that those in the lower within-firm income distribution suffer the largest earnings losses after the reform, likely due to their skills incompatible with the increased manufacturing investment.

5.2 Cost-per-Job Estimate

While our findings suggest that the accelerated depreciation policy had negative impacts on incumbent workers at treated firms, especially those that get displaced after the reform, the policy had positive impacts on firm outcomes, particularly on employment. To compare the fiscal cost of the reform to the number of jobs it created, we estimate a cost-per-job following Garrett et al. (2020):

$$\text{Cost-per-Job} = \frac{\sum_{t=1}^{T} \frac{\tau_t}{(1+r)^t} \cdot (CCA_t^{accelerated} - CCA_t^{normal}) \cdot \mathbb{1}[Taxable_t]}{\text{Number of Jobs Created}}.$$

where we estimate the fiscal cost (from 2007 to 2019) using corporate income tax rates (τ_t) and constant interest rate ($r = 0.07$). The difference between $CCA_t^{accelerated}$ and CCA_t^{normal} represents the difference in tax benefits per dollar of eligible investment accruing to treated firms during the accelerated depreciation period relative to the tax benefits before the reform. From the data on CCA claims, we can directly compute the lost corporate income tax revenue using the observed stream of CCA claims both under the accelerated depreciation and during the normal time. Finally, whether or not a firm is taxable in a given year affects the fiscal cost, which increases if a firm depreciates investment under the accelerated depreciation period, but experiences losses or shuts down in the future year, since the government would collect less tax revenue now without recouping additional revenue in the future. These estimates yield an estimate of the cost equal to X CAD per job.[11] Our estimated cost-per-job is comparable to the estimate of 20,000 USD per job from Garrett et al. (2020), which assesses the bonus depreciation policy in the United States.

6 Conclusion

This paper exploits firm-level variation in exposure to manufacturing investment and administrative data from tax records to estimate the effects of accelerated depreciation on worker earnings as well as firm outcomes. In 2007, the federal government in Canada accelerated depreciation schedules for assets related to machinery or equipment for manufacturing goods and services. We compare the outcomes of workers and firms with any positive manufacturing investment prior to the reform with workers and firms without any such investment. We find that incumbent workers' earnings decrease significantly after the reform, entirely driven by those displaced from treated firms. By contrast, treated firms increase investment in eligible assets, hire more workers, and pay hire salaries for new workers. Taken together, our findings suggest that a tax policy designed to promote manufacturing investment may result in changes in labor demand for high-skilled workers to complement capital investment, leading to earnings losses of existing workers being replaced by high-skilled workers.

[11] The estimates that go under this calculation are still waiting for approval from the RDC.

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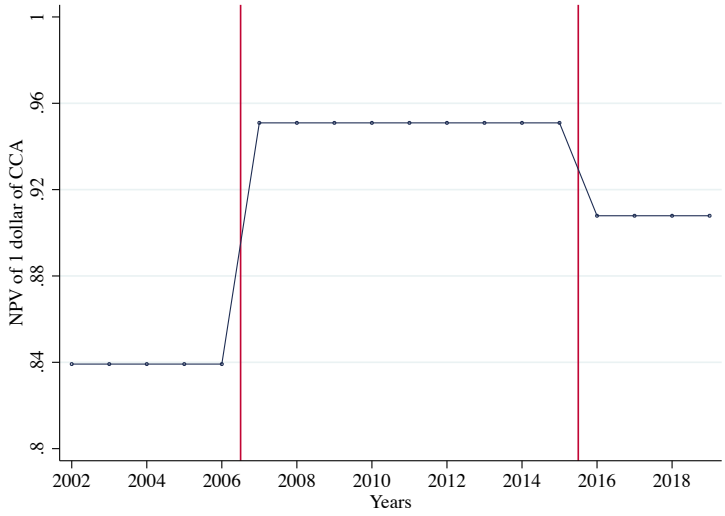
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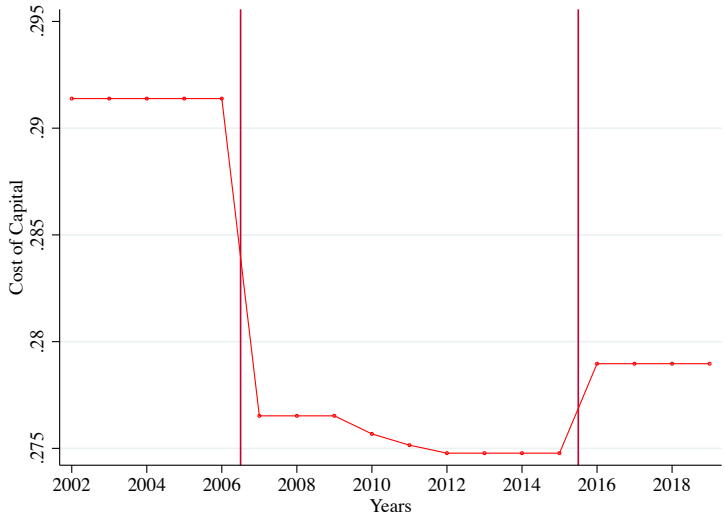
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Figure 1: Changes in NPV of CCA and Cost of Capital

(a) Changes in NPV of CCA



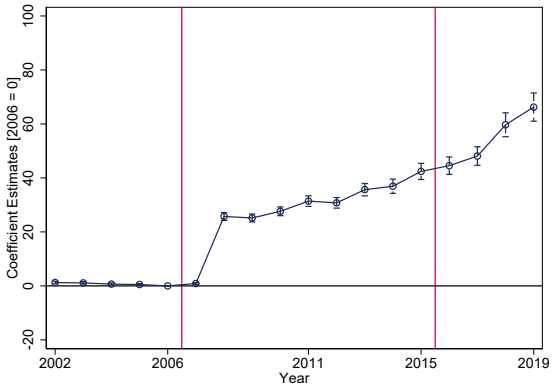
(b) Changes in Cost of Capital



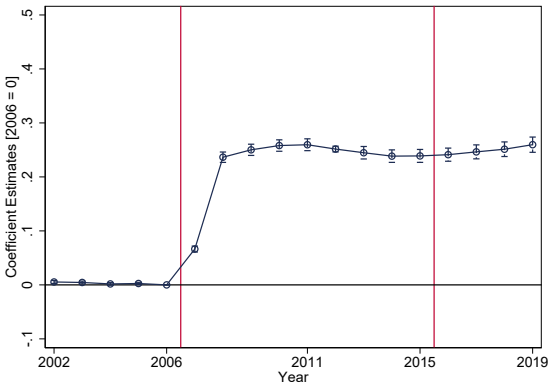
Notes: Panel (a) in this figure shows changes in net-present value (NPV) of one dollar of capital cost allowance (CCA) on assets related to machinery and equipment for manufacturing goods and services across years during our sample period. In 2007, the federal government increased the CCA rate for machinery and equipment from 30 per cent to 50 per cent and switched from the declining balance method to a straight-line rule. In 2016, the CCA rate remained at 50 per cent, but switched back to the declining-balance method. Panel (b) of this figure shows changes in the cost of capital for assets related to machinery and equipment. While we reflect changes in corporate income tax rates (both at the federal level and across provinces), such changes equally affect both treated and control firms, so the impacts of changes in corporate tax rates on the cost of capital is minimal in our setting.

Figure 2: Investment in Eligible Assets and CCA Claims

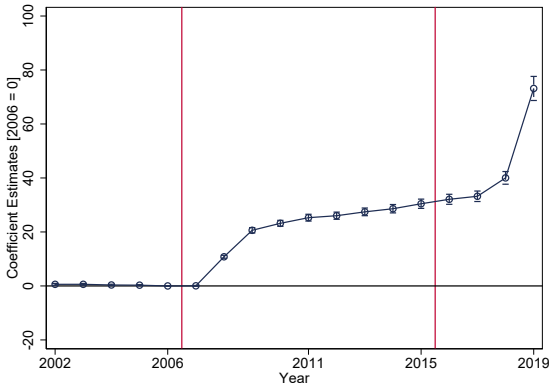
(a) Investment in Eligible Assets



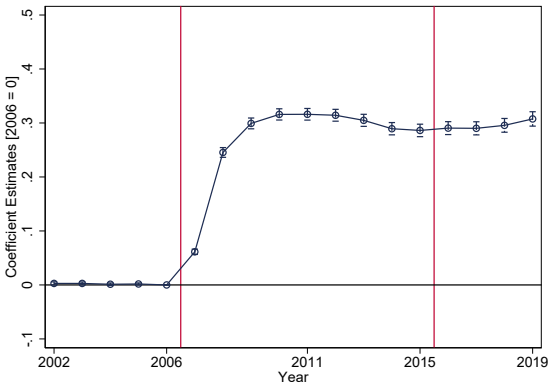
(b) Share of Firms Investing in Eligible Assets



(c) CCA Claims in Eligible Assets



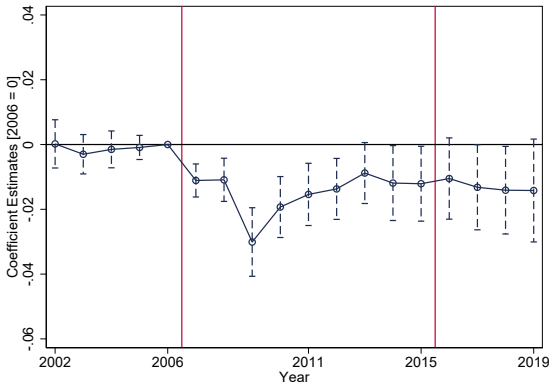
(d) Share of Firms Claiming CCA in Eligible Assets



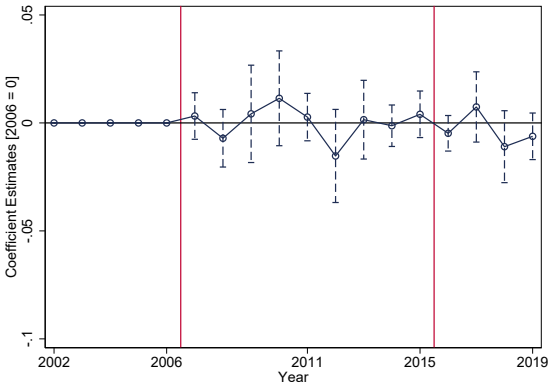
Notes: Panels (a) and (c) of this figure show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (1) for firms' investment and CCA claims, respectively, in assets eligible for the accelerated depreciation after 2007 (Class 29) and after 2016 (Class 53). Panels (b) and (d) of this figure show the estimates on the share of firms that invest claim CCA in these eligible assets, respectively. Investment and CCA claims are measured in thousand CAD. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 3: Worker-level Outcomes

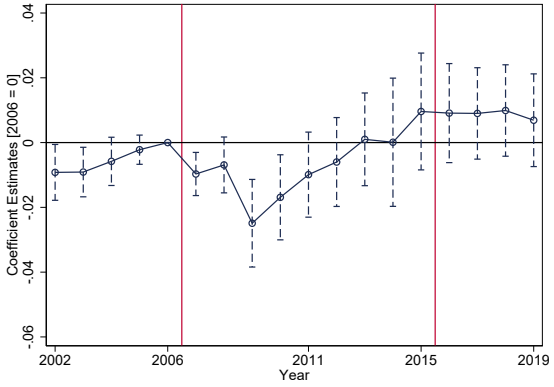
(a) log(Annual Earnings)



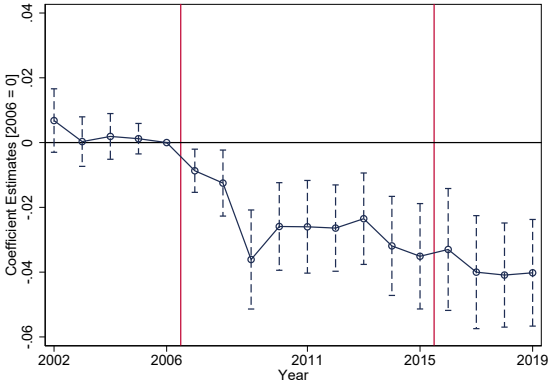
(b) Job Transition



(c) log(Annual Earnings) for Stayers



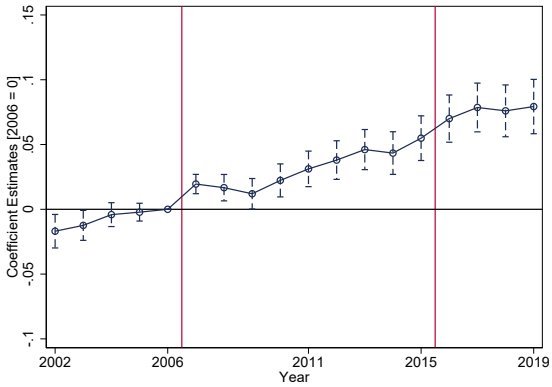
(d) log(Annual Earnings) for Movers



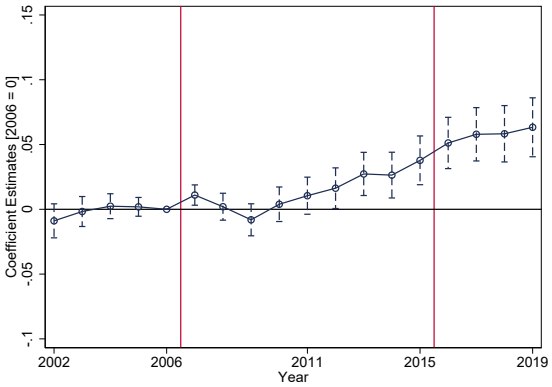
Notes: Panels (a) and (b) of this figure show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for workers' log(annual earnings) and job transition probabilities, respectively. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. Panel (c) shows the estimates on log(annual earnings) for workers who stayed with their original employer after 2006 and tracks them until they move to other firms. Panel (d) shows the estimates on earnings for workers who moved to other firms after 2006. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 4: Employment, Average Payrolls, and Investment Rate

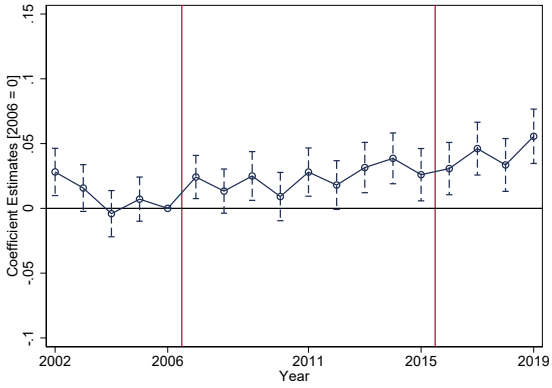
(a) log(Employment)



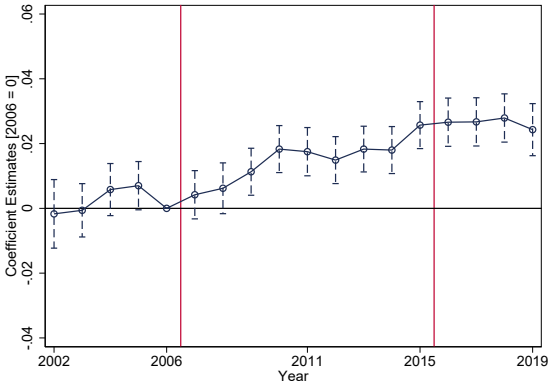
(b) log(Average Payrolls)



(c) log(Average Payrolls) of New Entrants



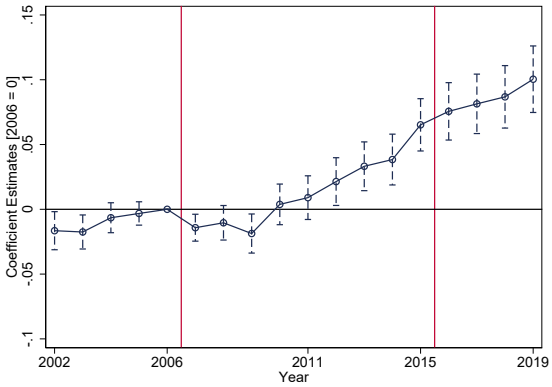
(d) Investment Rate



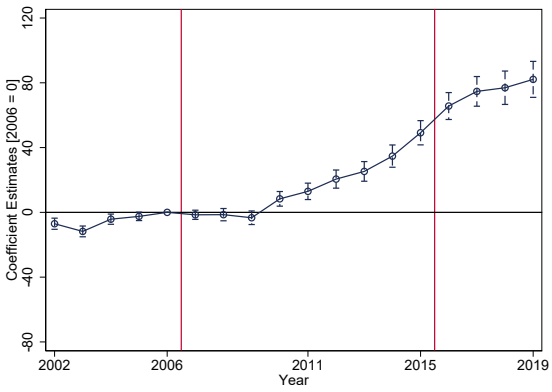
Notes: Panels (a) – (d) of this figure show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (1) for firms' log(employment), log(average payrolls), log(average payrolls) of new entrants, and investment rate in tangible assets, respectively. The log(average payrolls) of new entrants is calculated by averaging entrants' annual earnings at the firm level. The investment rate in tangible assets is define as $\frac{tangible\,assets_t - tangible\,assets_{t-1} + depreciation_t}{tangible\,assets_{t-1}}$. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 5: Sales and Profitability

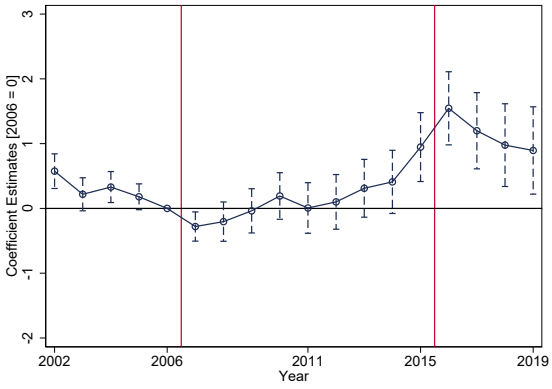
(a) log(Revenue)



(b) After-tax Profits



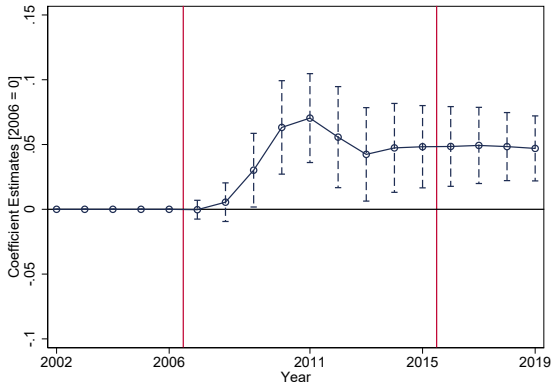
(c) EBITDA per Worker



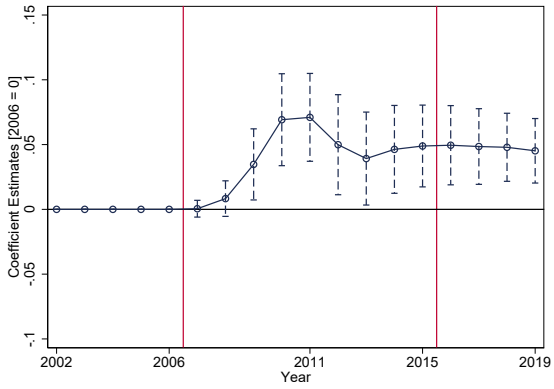
Notes: Panels (a) – (c) of this figure show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times \textit{Treated}$ in equation (1) for firms' log(revenue), after-tax profits, and EBITDA per worker, respectively. After-tax profits are defined as firms' net income after taxes and extraordinary items. EBITDA is earnings before interest, taxes, depreciation, and amortization. After-tax profits and EBITDA per worker are measured in thousand CAD. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 6: Share of Job Movers Switching Industries or Sectors or Becoming Unemployed

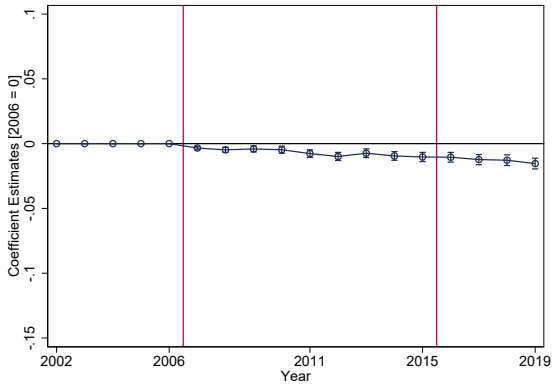
(a) Switching Industries



(b) Switching Sectors



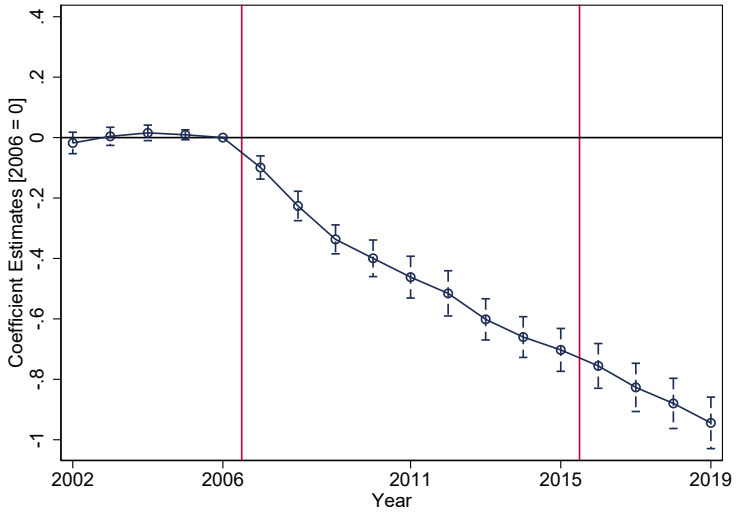
(c) Unemployed



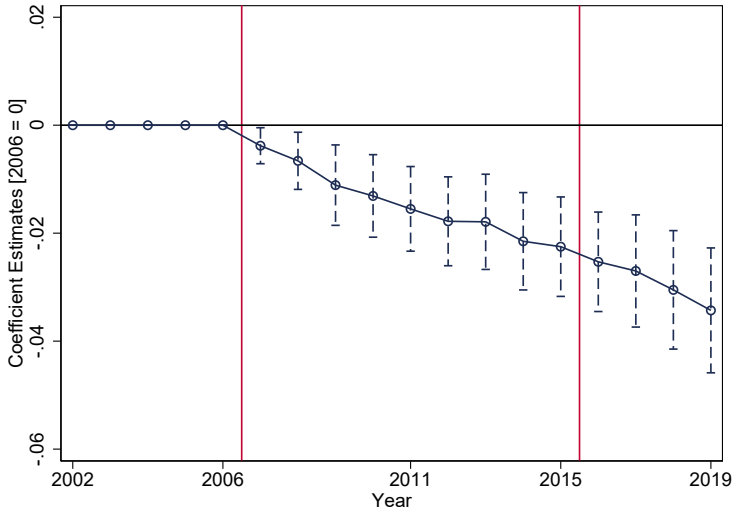
Notes: Panels (a) – (c) of this figure show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for the share of workers that move to different 4-digit NAICS industries and 2-digit NAICS sectors (from the one in 2006) and being unemployed, respectively. Panels (a) and (b) focus on years when the workers are employed, whereas panel (c) includes unemployed years. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. For panel (c), the sample consists of a balanced panel of workers. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 7: Firm Characteristics of Job Movers

(a) log(Employment)



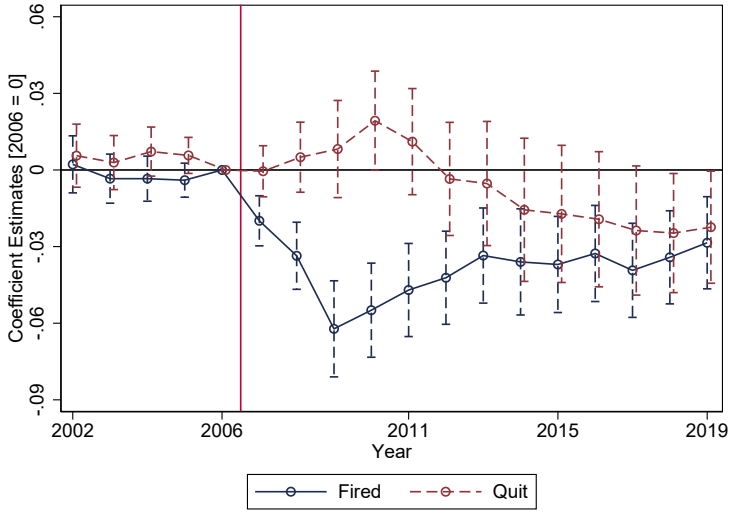
(b) AKM Firm Effects



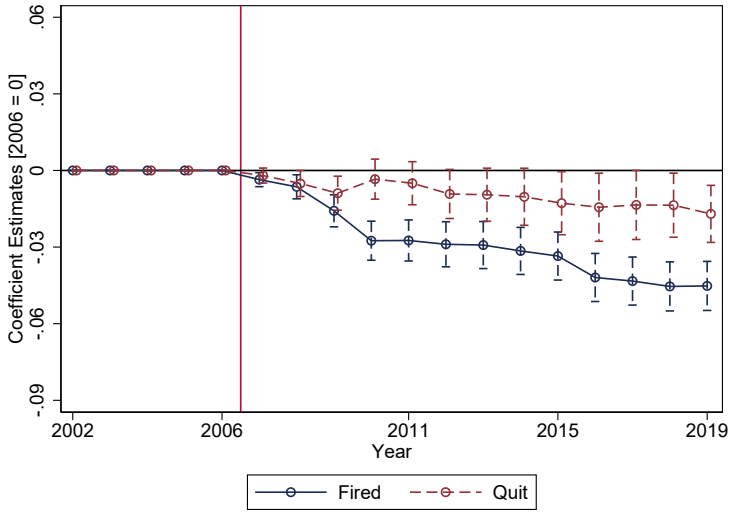
Notes: Panels (a) and (b) of this figure show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for log(employment) and AKM firm effects of firms of job movers. The sample consists of workers that leave their original firm after 2007. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 8: Heterogeneity by Type of Job Separations

(a) log(Annual Earnings)



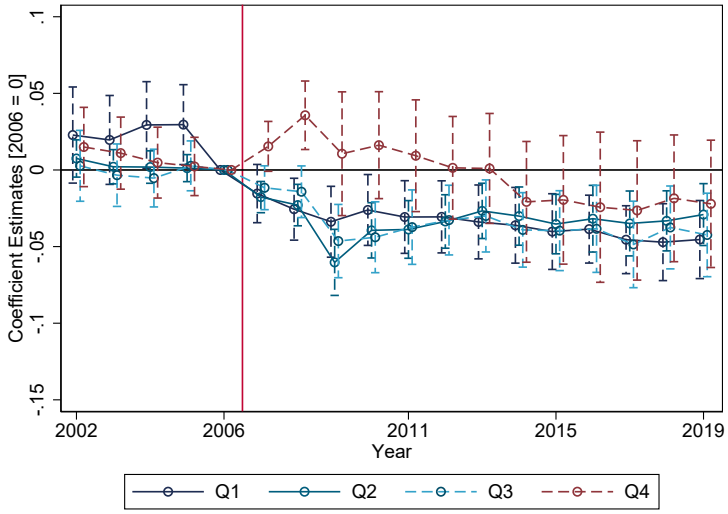
(b) AKM Firm Effects



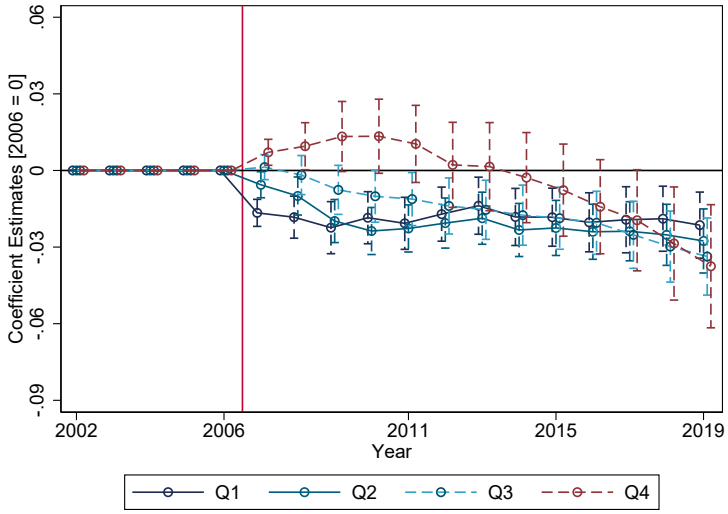
Notes: Panels (a) and (b) of this figure show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for workers' log(annual earnings) and AKM firm effects, separately for workers that move involuntarily to other firms and for workers that move voluntarily. The sample consists of workers that leave their original firm after 2007. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 9: Heterogeneity by Within-firm Earnings Distribution

(a) log(Annual Earnings)



(b) AKM Firm Effects



Notes: Panels (a) and (b) of this figure show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for workers' log(annual earnings) and AKM firm effects, separately for job movers in each quartile of the within-firm earnings distribution (measured in 2006). The sample consists of workers that leave their original firm after 2007. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals.

Table 1: Descriptive Statistics on Firms and Workers

	(1) Treated	(2) Control
A. Firm Characteristics		
Tangible Assets ('000)	2552.6	764.2
Revenue ('000)	7825.4	3170.9
Employment	40.1	18.4
Average Payrolls ('000)	125.3	51.2
After-tax Profits ('000)	110.5	61.5
EBITDA per Worker ('000)	6.9	6.0
Profit Margins	0.037	0.039
Firm Age	11.5	9.9
Sectors:		
Mining/Utility	0.007	0.009
Construction	0.042	0.139
Manufacturing	0.694	0.053
Wholesale Trade	0.088	0.079
Retail Trade	0.050	0.192
Transportation	0.004	0.044
Information	0.007	0.011
Professional Services	0.016	0.076
Education/Healthcare	0.006	0.098
Other Services	0.087	0.299
Observations	71,780	628,320
Firms	15,545	147,450
B. Worker Characteristics		
Annual Earnings ('000)	52.7	47.9
Male	0.720	0.581
Age	43.9	44.8
Sectors:		
Mining/Utility	0.062	0.022
Construction	0.016	0.113
Manufacturing	0.762	0.047
Wholesale Trade	0.069	0.092
Retail Trade	0.044	0.159
Transportation	0.003	0.156
Information	0.012	0.018
Professional Services	0.004	0.059
Education/Healthcare	0.007	0.145
Other Services	0.023	0.188
Observations	1,899,680	4,227,975
Workers	379,935	845,595

Notes: Panels A and B report summary statistics for firms and workers in our analysis sample, respectively. The firm sample consists of domestic firms in Alberta, British Columbia, Ontario, and Quebec with at least five workers in 2006. Treated and control firms are defined, respectively, as those with and without any investment in machinery or equipment for manufacturing goods in 2006. The worker sample consists of workers at the treated and control firms in 2006. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. All variables are measured as averages of years 2002 – 2006. Other services include real estate, rental and leasing, management, administrative support, arts and entertainment, accommodation and food services.

Table 2: Investment in Eligible Assts and CCA Claims

	(1) Investment in Eligible Assets	(2) Share of Firms Investing in Eligible Assets	(3) CCA Claims in Eligible Assets	(4) Share of Firms Claiming CCA in Eligible Assets
Post × Treated	30.9284***	0.2199***	24.1644***	0.2625***
	0.7244	(0.0035)	(0.5282)	(0.0038)
Observations	1,410,990	1,410,990	1,775,950	1,775,950
Mean Dep. Var.	0.0	0.003	0.0	0.006
Treated Firms	15,470	15,470	15,235	15,235
Control Firms	125,045	125,045	128,205	128,205
Adjusted R^2	0.416	0.501	0.469	0.565

Notes: Columns (1) and (3) report coefficient estimates on *Post* × *Treated* in equation (2) for firms' investment and CCA claims in eligible assets, respectively. Columns (2) and (4) report the same estimates for whether firms invest and claim CCA, respectively, in asset classes 29 and 53. The mean for each dependent variable is based on years 2002 – 2006, measured in thousand CAD for columns (1) and (3), and in level for columns (2) and (4). All specifications include firm fixed effects and industry by year fixed effects. Standard errors are clustered at the firm level and reported in parentheses.

Table 3: Worker-level Outcomes

	(1) log(Annual Earnings)	(2) Job Transition	(3) log(Annual Earnings) for Stayers	(4) log(Annual Earnings) for Movers
Post × Treated	-0.0135***	-0.0005	-0.0015	-0.0302***
	(0.0039)	(0.0041)	(0.0044)	(0.0053)
Observations	18,194,205	18,194,205	9,448,715	9,390,265
Mean Dep. Var.	52.7	0.0	54.5	50.9
Treated Workers	379,935	379,935	268,090	186,915
Control Workers	845,595	845,595	537,025	405,340
Adjusted R^2	0.698	0.123	0.782	0.634

Notes: Columns (1) and (2) report coefficient estimates on *Post* × *Treated* in equation (4) for workers' log(annual earnings) and job transition probabilities, respectively. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. Columns (3) shows estimates on earnings for workers who stayed with their pre-reform employer after 2006 and tracks them until they moved to other firms. Columns (4) shows the estimates on earnings for workers who moved to other firms after 2006. The mean for each dependent variable is based on years 2002 – 2006, measured in thousand CAD for columns (1), (3) and (4), and in level for column (2). All specifications include worker fixed effects and industry by year fixed effects. Standard errors are two-way clustered at the firm level and worker level and reported in parentheses.

Table 4: Employment, Average Payrolls, and Investment Rate

	(1) log(Employment)	(2) log(Average Payrolls)	(3) log(Average Payrolls) of Entrants'	(4) Investment Rate
Post × Treated	0.0452***	0.0225***	0.0179***	0.0140***
	(0.0062)	(0.0065)	(0.0044)	(0.0020)
Observations	2,005,910	2,005,910	1,607,990	1,882,890
Mean Dep. Var.	40.1	125.3	16.8	0.112
Treated Firms	15,545	15,545	13,900	14,995
Control Firms	147,450	147,450	140,200	130,470
Adjusted R^2	0.823	0.852	0.483	0.144

Notes: Columns (1) – (4) report coefficient estimates on *Post* × *Treated* in equation (2) for firms' log(employment), log(average payrolls), log(average payrolls) of entrants, and investment rate in tangible assets, respectively. The log(average payrolls) of entrants is calculated by averaging entrants' annual earnings at the firm level. The investment rate in tangible assets is define as $\frac{tangible\,assets_t - tangible\,assets_{t-1} + depreciation_t}{tangible\,assets_{t-1}}$. The mean for each dependent variable is based on years 2002 – 2006, measured in level for column (1), (4), and (6), and in thousand CAD for columns (2), (3), and (5). All specifications include firm fixed effects and industry by year fixed effects. Standard errors are clustered at the firm level and reported in parentheses.

Table 5: Sales and Profitability

	(1) log(Revenue)	(2) After-tax Profits	(3) EBITDA per Worker
Post × Treated	0.0329***	30.3513***	0.0662
	(0.0070)	(1.9051)	(0.1280)
Observations	2,005,910	2,005,910	2,005,855
Mean Dep. Var.	7825.4	110.5	6.9
Treated Firms	15,545	15,545	15,545
Control Firms	147,450	147,450	147,435
Adjusted R^2	0.878	0.528	0.495

Columns (1) – (3) report coefficient estimates on *Post* × *Treated* in equation (2) for firms' log(revenue), after-tax profits, and EBITDA per worker, respectively. After-tax profits are defined as firms' net income after taxes and extraordinary items. EBITDA is earnings before interest, taxes, depreciation, and amortization. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD. All specifications include firm fixed effects and industry by year fixed effects. Standard errors are clustered at the firm level and reported in parentheses.

Table 6: Share of Job Movers Switching Industries or Sectors or Becoming Unemployed

	(1) Switching Industries	(2) Switching Sectors	(3) Unemployed
Post × Treated	0.0414***	0.0418***	-0.0087***
	(0.0119)	(0.0117)	(0.0011)
Observations	9,390,265	9,390,265	10,660,570
Mean Dep. Var.	0.000	0.000	0.000
Treated Workers	186,915	186,915	186,915
Control Workers	405,340	405,340	405,340
Adjusted R^2	0.529	0.448	0.369

Notes: Columns (1) – (3) report coefficient estimates on *Post* × *Treated* in equation (4) for workers' probabilities of moving to different 4-digit NAICS industries and 2-digit NAICS sectors (relative to the one in 2006) and being unemployed, respectively. For columns (1) and (2), the sample consists of workers that leave firms after 2007. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. For column (3), the sample consists of a balanced panel of workers. The mean for each dependent variable is based on years 2002 – 2006 and measured in level. All specifications include worker fixed effects and industry by year fixed effects. Standard errors are two-way clustered at the firm level and worker level and reported in parentheses.

Table 7: Firm Characteristics of Job Movers

	(1) log(Employment)	(2) AKM Firm Effects
Post × Treated	-0.5289***	-0.0174***
	(0.0281)	(0.0033)
Observations	8,551,635	8,782,815
Mean Dep. Var.	3,082.2	0.0765
Treated Workers	186,915	186,895
Control Workers	405,340	404,070
Adjusted R^2	0.758	0.811

Notes: Columns (1) and (2) report coefficient estimates on *Post* × *Treated* in equation (4) for log(employment) and AKM firm effects of firms of job movers. The sample consists of workers that leave firms after 2007. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. The mean for each dependent variable is based on years 2002 – 2006, measured in thousand CAD for column (1) and in level for column (2). All specifications include worker fixed effects and industry by year fixed effects. Standard errors are two-way clustered at the firm level and worker level and reported in parentheses.

Table 8: Heterogeneity by Type of Job Separations

	(1) log(Annual Earnings)	(2) AKM Firm Effects
Post × Treated (Quit)	-0.0094	-0.0083**
	(0.0091)	(0.0039)
Post × Treated (Fired)	-0.0375***	-0.0270***
	(0.0074)	(0.0033)
Difference	-0.0280***	-0.0186***
	(0.0094)	(0.0037)
Observations	4,630,305	4,335,365
Mean Dep. Var. (Quit)	44.3	0.015
Mean Dep. Var. (Fired)	48.0	0.103
Treated Workers	104,745	104,745
Control Workers	190,285	183,410
Adjusted R^2	0.597	0.775

Notes: Columns (1) and (2) report coefficient estimates on *Post* × *Treated* in equation (4) for workers' log(annual earnings) and AKM firm effects, separately for workers that leave involuntarily and for workers that leave voluntarily after the reform. The sample consists of workers that leave firms after 2007. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded.The mean for each dependent variable is based on years 2002 – 2006, measured in thousand CAD for column (1), and in level for column (2). All specifications include worker fixed effects and industry by year fixed effects. Standard errors are two-way clustered at the firm level and worker level and reported in parentheses.

Table 9: Heterogeneity by Within-firm Earnings Distribution

	(1) log(Annual Earnings)	(2) AKM Firm Effects
Post × Treated (1st Quantile)	-0.0523***	-0.0183***
	(0.0073)	(0.0020)
Post × Treated (2nd Quantile)	-0.0359***	-0.0196***
	(0.0067)	(0.0017)
Post × Treated (3rd Quantile)	-0.0348***	-0.0142***
	(0.0075)	(0.0018)
Post × Treated (4th Quantile)	-0.0073	-0.0016
	(0.0143)	(0.0017)
Observations	7,236,635	6,808,490
Mean Dep. Var. (1st Quantile)	41.2	0.016
Mean Dep. Var. (2nd Quantile)	51.1	0.070
Mean Dep. Var. (3rd Quantile)	55.2	0.120
Mean Dep. Var. (4th Quantile)	56.5	0.100
Treated Workers	173,795	173,795
Control Workers	280,640	280,620
Adjusted R^2	0.639	0.810

Notes: Columns (1) and (2) report coefficient estimates on *Post* × *Treated* in equation (4) for workers' log(annual earnings) and AKM firm effects, separately for job movers across different quartiles in the within-firm earnings distribution. The sample consists of workers that leave firms after 2007. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. The mean for each dependent variable is based on years 2002 – 2006, measured in thousand CAD for column (1), and in level for column (2). All specifications include worker fixed effects and industry by year fixed effects. Standard errors are two-way clustered at the firm level and worker level and reported in parentheses.

ONLINE APPENDIX:

Manufacturing Investment and Employee Earnings: Evidence from Accelerated Depreciation

Yige Duan (yigeduan@mail.ubc.ca) Terry S. Moon (tsmoon@mail.ubc.ca)

A Robustness Checks and Internal Validity

In Appendix A, we provide results from robustness tests discussed in Sections 3 and 4.

A.1 Different Specifications

To account for any 4-digit industry specific shock in a given year that might drive our results, we include 4-digit industry by year fixed effects in our main specification. Analogously, we include sector by year fixed effects (instead of industry by year fixed effects) to control for any sector-specific shock potentially coinciding with the reform that might drive our results. Figure A.1 and Table A.1 show that our key results are robust to controlling for sector by year fixed effects.

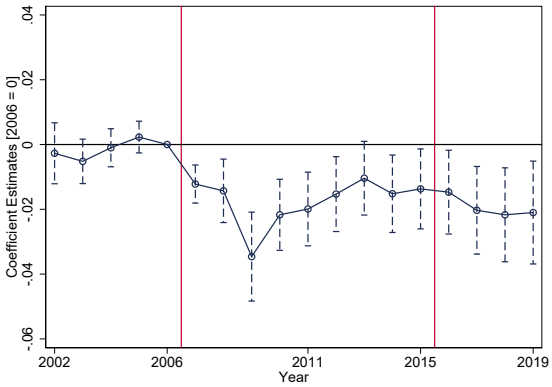
Moreover, we additionally include commuting zone by year fixed effects to absorb any local labor market specific shock in a given year potentially driving our findings (Figure A.2 and Table A.2). Here, we cluster standard errors at the industry by commuting zone level. The results are qualitatively similar to our main findings.

Furthermore, Figure A.3 and Table A.3 include previously excluded sectors (agriculture, finance, and public administration), and Figure A.3 and Table A.3 include previously excluded provinces. Results from including these previously excluded sectors and provinces are similar to our main results.

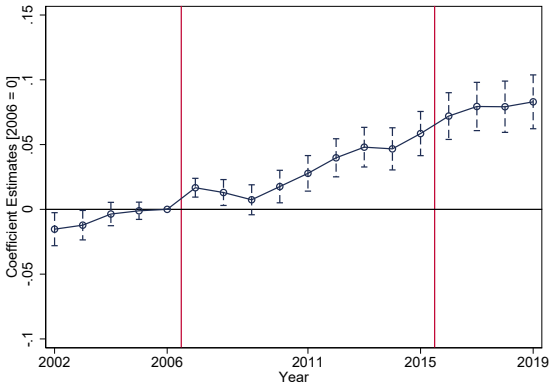
In the preferred specification of equations (3) and (4), we impose tenure restrictions and drop multiple-job holders or those making below 4,000 CAD in annual earnings to focus on full-time workers with stable jobs. In Figure A.5 and Table A.5, we relax these restrictions and include the previously excluded workers. While this greatly increases the sample size, the estimated effect on workers' earnings is qualitatively similar to (if anything, larger in magnitude than) our main estimate.

Figure A.1: Controlling for Sector by Year Fixed Effects

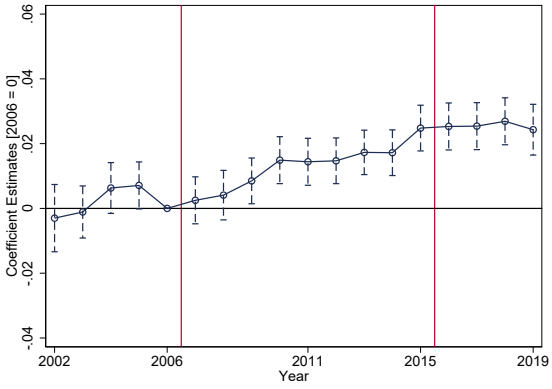
(a) log(Annual Earnings)



(b) log(Employment)



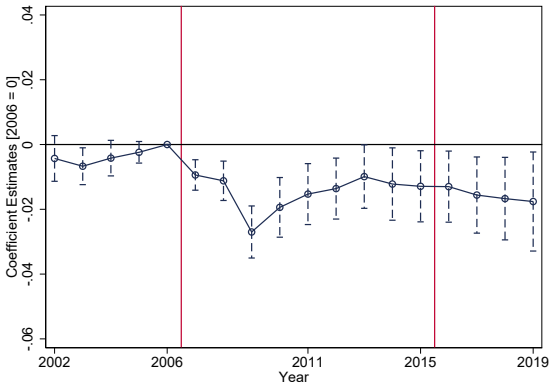
(c) Investment Rate



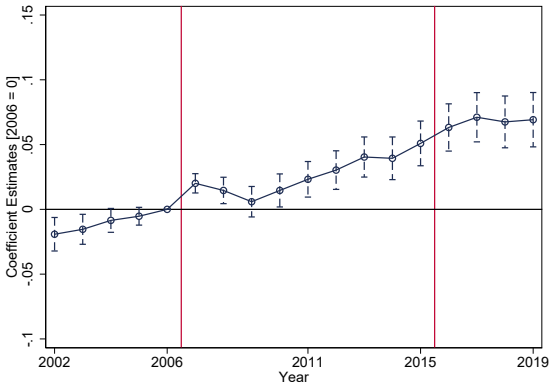
Notes: Panels (a) shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (3) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. Panels (b) and (c) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (1) for firms' log(employment) and investment rate respectively. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals. Instead of 4-digit NAICS industry by year fixed effects, we control for 2-digit NAICS sector by year fixed effects.

Figure A.2: Controlling for Commuting Zone by Year Fixed Effects

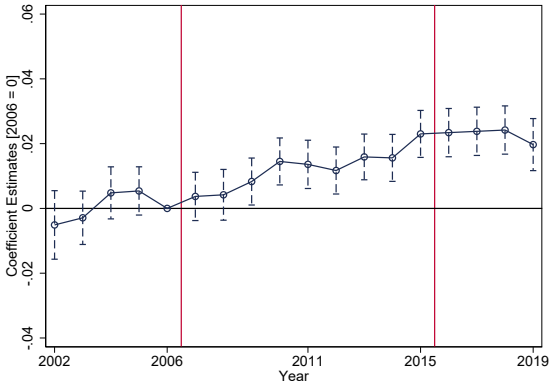
(a) log(Annual Earnings)



(b) log(Employment)



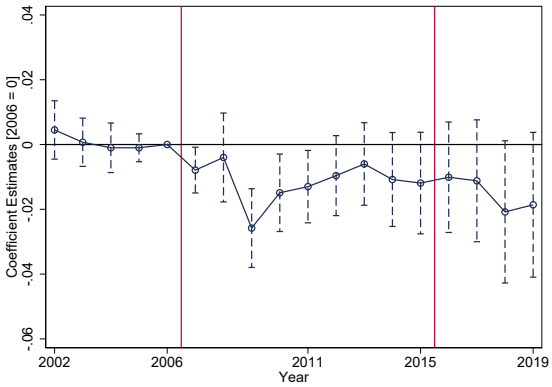
(c) Investment Rate



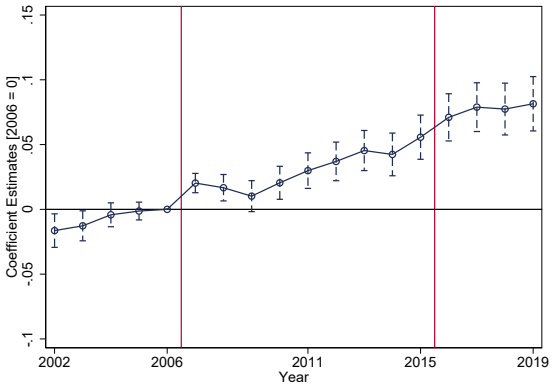
Notes: Panels (a) shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (3) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. Panels (b) and (c) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (1) for firms' log(employment) and investment rate respectively. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals. We additionally control for commuting zone by year fixed effects.

Figure A.3: Including All Industries

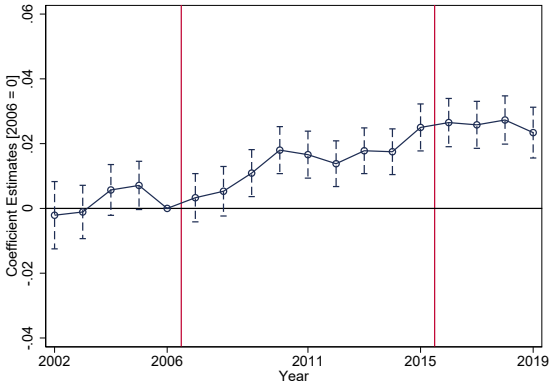
(a) log(Annual Earnings)



(b) log(Employment)



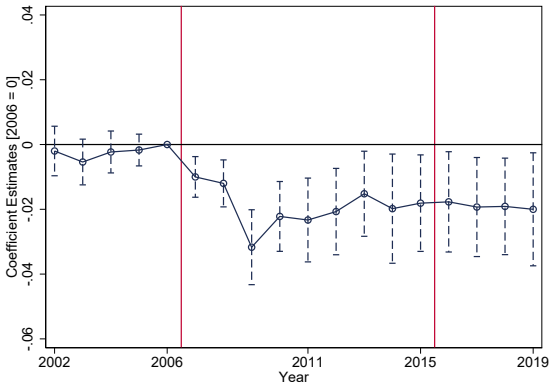
(c) Investment Rate



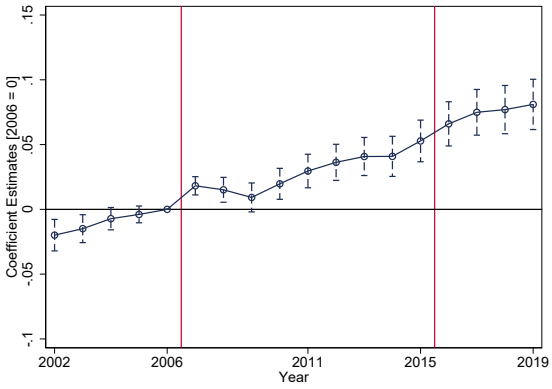
Notes: PPanels (a) shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (3) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. Panels (b) and (c) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (1) for firms' log(employment) and investment rate respectively. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals. The samples consist of workers and firms in all provinces.

Figure A.4: Including All Provinces

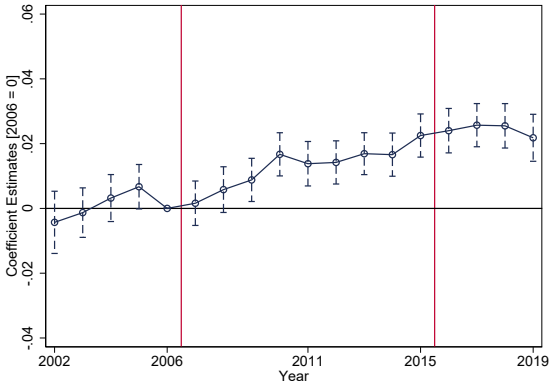
(a) log(Annual Earnings)



(b) log(Employment)

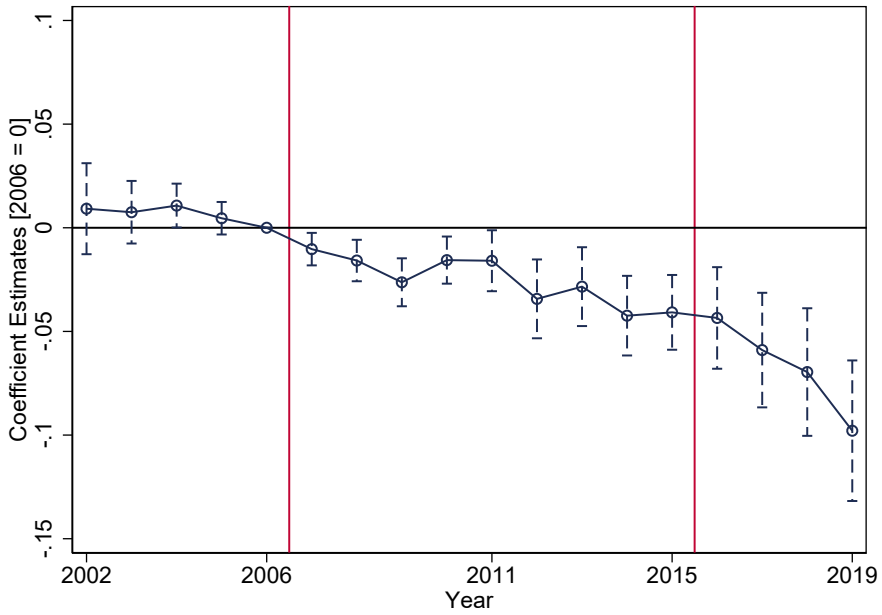


(c) Investment Rate



Notes: Panels (a) shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (3) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. Panels (b) and (c) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (1) for firms' log(employment) and investment rate respectively. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals. The samples consist of workers and firms in all sectors.

Figure A.5: Including Part-timers, Moonlighters, and Removing Tenure Restrictions



Notes: The figure shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for workers' log(annual earnings). The sample includes part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006. The solid vertical line indicates the reform years. The dashed lines represent 95 percent confidence intervals.

Table A.1: Controlling for Sector by Year Fixed Effects

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
Post × Treated	-0.0167***	0.0439***	0.0126***
	(0.0055)	(0.0061)	(0.0020)
Observations	18,194,205	2,005,910	1,882,890
Mean Dep. Var.	52.7	40.1	0.112
Treated Workers/Firms	379,935	15,545	14,995
Control Workers/Firms	845,595	147,450	130,470
Adjusted R^2	0.696	0.819	0.139

Notes: Column (1) reports coefficient estimates on *Post* × *Treated* in equation (4) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. Columns (2) and (3) report coefficient estimates on *Post* × *Treated* in equation (2) for firms' log(employment) and investment rate, respectively. The mean for each dependent variable is based on years 2002 – 2006, measured in thousand CAD for columns (1) and (4), and in level for columns (2) and (3). Column (1) includes worker fixed effects and 2-digit NAICS sector by year fixed effects. Columns (2) and (3) include firm fixed effects and 2-digit NAICS sector by year fixed effects. Standard errors are two-way clustered at the firm level and worker level in column (1), clustered at the firm level in columns (2) and (3), and reported in parentheses.

Table A.2: Controlling for Commuting Zone by Year Fixed Effects

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
Post × Treated	-0.0113***	0.0424***	0.0129***
	(0.0038)	(0.0062)	(0.0020)
Observations	18,194,205	2,005,910	1,882,890
Mean Dep. Var.	52.7	40.1	0.112
Treated Workers/Firms	379,935	15,545	14,995
Control Workers/Firms	845,595	147,450	130,470
Adjusted R^2	0.698	0.823	0.146

Notes: Column (1) reports coefficient estimates on *Post* × *Treated* in equation (4) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. Columns (2) and (3) report coefficient estimates on *Post* × *Treated* in equation (2) for firms' log(employment) and investment rate, respectively. The mean for each dependent variable is based on years 2002 – 2006, measured in thousand CAD for columns (1) and (4), and in level for columns (2) and (3). Column (1) includes worker fixed effects and commuting zone by year fixed effects. Columns (2) and (3) include firm fixed effects and commuting zone by year fixed effects. Standard errors are two-way clustered at the firm level and worker level in column (1), clustered at the firm level in columns (2) and (3), and reported in parentheses.

Table A.3: Including All Industries

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
Post × Treated	-0.0130**	0.0448***	0.0135***
	(0.0059)	(0.0061)	(0.0020)
Observations	22,752,670	2,095,165	1,966,225
Mean Dep. Var.	53.3	40.2	0.112
Treated Workers/Firms	402,760	15,720	15,155
Control Workers/Firms	1,129,595	154,530	136,780
Adjusted R^2	0.701	0.822	0.143

Notes: Column (1) reports coefficient estimates on *Post* × *Treated* in equation (4) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. Columns (2) and (3) report coefficient estimates on *Post* × *Treated* in equation (2) for firms' log(employment) and investment rate, respectively. The samples consist of workers and firms in all provinces. The mean for each dependent variable is based on years 2002 – 2006, measured in thousand CAD for columns (1) and (4), and in level for columns (2) and (3). Column (1) includes worker fixed effects and industry by year fixed effects. Columns (2) and (3) include firm fixed effects and industry by year fixed effects. Standard errors are two-way clustered at the firm level and worker level in column (1), clustered at the firm level in columns (2) and (3), and reported in parentheses.

Table A.4: Including All Provinces

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
Post × Treated	-0.0166***	0.0452***	0.0133***
	(0.0046)	(0.0058)	(0.0019)
Observations	20,531,850	2,287,450	2,148,615
Mean Dep. Var.	51.6	40.2	0.111
Treated Workers/Firms	420,495	17,445	16,825
Control Workers/Firms	959,825	168,060	148,610
Adjusted R^2	0.703	0.822	0.142

Notes: Column (1) reports coefficient estimates on *Post* × *Treated* in equation (4) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. Columns (2) and (3) report coefficient estimates on *Post* × *Treated* in equation (2) for firms' log(employment) and investment rate, respectively. The samples consist of workers and firms in all industries. The mean for each dependent variable is based on years 2002 – 2006, measured in thousand CAD for columns (1) and (4), and in level for columns (2) and (3). Column (1) includes worker fixed effects and industry by year fixed effects. Columns (2) and (3) include firm fixed effects and industry by year fixed effects. Standard errors are two-way clustered at the firm level and worker level in column (1), clustered at the firm level in columns (2) and (3), and reported in parentheses.

Table A.5: Including Part-timers, Moonlighters, and Removing Tenure Restrictions

	(1)
	log(Annual Earnings)
Post × Treated	-0.0389***
	(0.0092)
Observations	74,061,625
Mean Dep. Var.	39.1
Treated Workers	1,129,540
Control Workers	39,101,415
Adjusted R^2	0.712

Notes: The table reports coefficient estimates on *Post* × *Treated* in equation (4) for workers' log(annual earnings). The sample includes part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006. The mean for the dependent variable is based on years 2002 – 2006 and measured in thousand CAD. The specification includes worker fixed effects and year fixed effects. Standard errors are two-way clustered at the firm level and worker level and reported in parentheses.