Manufacturing Investment and Employee Earnings: Evidence from Accelerated Depreciation*

Yige Duan† Terry S. Moon‡

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Abstract

This paper assesses the effects of a tax policy designed to induce manufacturing investment on employee earnings. We examine the 2007 accelerated depreciation policy in Canada, which targets firms acquiring machinery or equipment and lowers their cost of capital by accelerating the schedule for deducting capital costs from corporate taxes. Following the reform, treated firms increase manufacturing investment and experience increases in employment, average payrolls, sales, and profitability. However, we find that incumbent workers at treated firms experience a decrease in their earnings, mostly driven by those displaced from treated firms and move to smaller firms with lower wage premiums after the reform. Notably, this decline in earnings is concentrated among job movers in the lower quantile of the within-firm earnings distribution or worker fixed effects. Furthermore, the policy led to an increase in the within-firm wage inequality, which provides evidence of skill-biased technological change. Taken together, our findings suggest that a tax policy 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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†Vancouver School of Economics, University of British Columbia (email: yige.duan@gmail.com)
‡Vancouver School of Economics, University of British Columbia (email: tsmoon@mail.ubc.ca)

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, if the increased capital replaces labor, both employment and wages would fall after the policy change. On the other hand, if the scale effects from firm expansions dominate the substitution effects, then employment would go up, with ambiguous impacts on average wages depending on changes in worker composition. Therefore, it is important to accurately measure how such tax policies 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 composition.

This paper studies the effects of an accelerated depreciation policy on worker-level outcomes using administrative employer-employee matched data from tax records in Canada. We exploit a 2007 federal reform, which accelerated the depreciation schedule for 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 assets before the reform would be least likely affected by the policy because they either rent, outsource, or do not use such assets in their production. While the majority of treated firms is in the manufacturing sector, there are firms in other sectors that invest in machinery or equipment. Furthermore, we have firms within the manufacturing sector that do not buy machinery, but just rent it or outsource. This allows us to additionally exploit within-sector variation.

[1] While there are a few studies on how corporate taxes affect wages, including Arulampalam et al. (2012) Suárez Serrato and Zidar (2016), Fuest et al. (2018), Dobridge et al. (2021), Kennedy et al. (2023), and Duan and Moon (2023), there is currently no paper that examines how an accelerated depreciation policy affects wages using employee-level data.

First, after the accelerated depreciation policy, firms with any positive investment in machinery or equipment prior to the reform acquire additional manufacturing assets on average, while firms without any manufacturing investment do not respond to those tax incentives. On average, treated firms' investment rate on eligible assets increased by roughly 19 percent. Since not all firms with positive manufacturing investment respond to the policy, our estimates capture the intent-to-treat effects. Furthermore, we find that treated firms' investment rate on overall tangible assets (including machinery or equipment) increased by 13 percent on average, relative to control firms.

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 who 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 leave their firm compared to those at control firms, job movers from treated firms experience a larger decline in their earnings relative to job movers from control firms 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 2.2 percent, respectively, on average the reform. This suggests that the decline in existing workers' earnings is not driven by treated firms replacing workers with capital. The fact that treated firms raised average salaries, without increasing existing workers' average wages, suggests that new hires are paid higher wages.

Importantly, we find that treated firms experience increases in sales, after-tax profits, and return on assets, implying that these additional investments in capital and new workers result in higher profitability. In contrast to the findings in Kline et al. (2019), 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 new hires to recruit more skilled workers who 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 5 percent of job movers move to firms in different

industries or sectors on average after the reform. While those who move to different industries may also switch occupations, the share of movers that switches 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 who 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 of job movers have about 53 log points lower employment and 2.3 log points lower firm fixed effects compared to treated firms. Therefore, the decline in earnings of job movers can be explained by treated workers moving to smaller firms with lower wage premiums relative to control workers after the reform. Furthermore, the reason behind job separations may be important in explaining why workers moving from treated firms end up at smaller firms relative to workers who leave control firms. The increased average payrolls for new hires implies that workers displaced from treated firms are more likely to be replaced by workers with higher skills (who can operate new machines or equipment) relative to workers who leave control firms (where the relative demand for high-skilled workers does not change) 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, even if accelerated depreciation does not result in treated firms substituting labor with capital in our setting, it may lead treated firms to increase the relative demand for high-skilled workers to complement capital investment. This shift in labor demand for specific 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 displaced relative to those who move voluntarily. This is because workers are less likely to voluntarily move to other firms during the time period where there is less demand for their skills unless their new firm would pay them a comparable salary. Indeed, we find that workers who are displaced 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 or worker fixed effects experience larger decreases in their earnings and wage premiums, compared to workers at the top quartile of the distribution and worker fixed effects. These results are consistent with the idea that workers who are displaced or with lower skills (proxied by their pre-reform earnings or fixed effects relative to those of other workers) may face larger search frictions to find a new firm that would give them a comparable wage when the relative demand for high-skilled workers increases to complement the rise in capital investment (Krusell et al. 2000). Moreover, this rise in

relative demand for high-skilled workers resulted in the increase in within-firm earnings inequality. We find that the earnings gap between workers in the top 10 percentile and those in the bottom 10 percentile increased significantly after the reform. This is evidence of skill-biased technological change induced by a tax policy.

We conduct several robustness tests to strengthen the internal validity of our results. While our baseline specification includes industry by year fixed effects to absorb any time-varying industry-specific shock, our results are robust to alternative specifications with a different set of controls and fixed effects. Furthermore, our results are qualitatively similar to redefining treated firms and control firms based on the pre-reform share of manufacturing assets, which excludes firms with zero investment in machinery or equipment, thereby minimizing the concern that firms without any manufacturing investment might be exposed to differential shocks.

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 workers. 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 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, which allow us to control for changes in worker composition by tracking the same workers over time. Being able to follow workers over time is important since it allows us to examine how the policy affects not only workers who continue to remain at treated firms, but also those who get displaced or move to other firms after the reform. Furthermore, the worker-level data allows us to assess how the policy affects reallocation of workers across different firms and sectors, allowing us to conduct a comprehensive analysis on how accelerated depreciation impacts overall workers in the economy.

Moreover, 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. We find that firms responsive to the accelerated depreciation policy significantly increased investment in both labor and capital, consistent with the findings from prior studies. In particular, this increased investment subsequently 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) that shows 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 show 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 hires. 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.

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 profitability 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 investment 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 employer-employee matched data, we find that treated firms likely hired more high-skilled workers. However, this change in demand for high-skilled workers resulted in earnings losses for workers who 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 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 the accelerated depreciation policy does not lead to differential job displacements for workers at treated firms relative to workers at control firms. However, those that leave treated firms experience a larger decline in earnings relative to those that leave control firms. Furthermore, workers who 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, 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, 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 behind 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. Under the CCA system, firms can claim deductions 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. For example, the CCA rate for an asset class 1 (buildings) is 4 percent, and the CCA rate for an asset class 43 (machinery and equipment for manufacturing and processing goods) is 30 percent.[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 equipment. 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

[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.

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 asset classes. 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 asset 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 percent 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 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 percentage points (13 percent) increase from z_{43}. Since 2007, the federal government continued to extend this accelerated depreciation schedule until 2015.

In 2016, the federal government maintained the 50 percent CCA rate for Class 29, but reverted to the declining balance method. Machinery and equipment that would otherwise be included in Class 29 will be now 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 7 percentage points (8 percent) higher than the original z_{43} prior to the reform in 2007 (Panel (a), Figure 1).

[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 with longer depreciation schedules (i.e., buildings) due to lower CCA rates, compared to firms in the information technology sector that have a larger share of assets that depreciate much more quickly (i.e., computers). 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 2008; Zwick and Mahon 2017; Curtis et al. 2022).

Based on the value of z, we can compute the cost of capital using 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. For firms investing in machinery and equipment, the cost of capital decreases from 0.291 to 0.277 (5 percent) on average after the reform (Panel (b), Figure 1).[5]

While the 2007 reform (and the subsequent extension in 2016) aimed to induce investment in machinery and equipment by targeting a particular asset class (initially labeled as 43, but re-labeled as 29 and then 53), the depreciation schedules for other asset classes were generally not affected by the reform.[6] 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 (largely 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. For example, Panels (c) and (d) of Figure 1 show that the estimated changes in z (0.101 percentage points) and cost of capital for machinery and equipment (0.011 percentage points) for the treated firms based on our data are a bit smaller because not all eligible firms claim CCA in each period.

[5]Note that this estimate is computed holding r, δ, and τ constant. 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 (defined in Section 3) in our setting, so their impact on the cost of capital would be absorbed in a difference-in-differences framework.

[6]Three exceptions are (1) Class 1 (buildings used for manufacturing and processing goods) in which the CCA rate increased from 4 percent to 10 percent in 2007, (2) Class 10 (computers) in which the CCA rate increased from 45 percent to 55 percent in 2007 and to 100 percent in 2009, and (3) Class 43.1 (green technology adoptions) in which the CCA rate increased from 30 percent to 50 percent in 2007. However, the actual changes in the net-present value and cost of capital for these other assets are small for treated firms relative to control firms after the reform (in difference-in-differences) because control firms also invest in these asset classes and would benefit from these (Appendix A). This suggests 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 percent to 50 percent based on a straight-line rule, any firm that invests in machinery and equipment for manufacturing and processing goods after 2007 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 either (1) rent, (2) outsource, or (3) do not use machinery or equipment in their production, and thus would be unlikely to buy those assets in response to the policy.[7] 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 accelerated depreciation.

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 from 2002 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.[8]

[7] For example, manufacturing firms that do not buy machinery or equipment must rent it or outsource to produce goods and services; otherwise, they would not be classified as part of the manufacturing sector.

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

One potential concern is that firms that invest in machinery or equipment are concentrated in a particular sector compared to firms that never buy 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 that rent or outsource 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. It is also possible that our results are driven by other shocks particular to a certain industry within our treated group. To account for industry-specific shocks potentially 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 policy 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. Second, we exclude firms with less than five employees in 2006, thereby dropping very small businesses that rarely invest in machinery or equipment, but also cannot be used as part of the control group because they are much smaller than treated firms. Third, we focus on the four major provinces (Alberta, British Columbia, Ontario, and Quebec), which make up for almost 90 percent of all firms in Canada, in terms of shares based on the number of firms, total revenue, assets, and employment (Duan and Moon 2023). Finally, we exclude firms in agriculture, finance and real estate sectors (accounting for 4 percent of all firms in our sample) mainly because these sectors appear least comparable to the manufacturing sector. In Appendix B, we show that our main results are robust to including firms in these previously 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 several 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, our results are robust to various specifications with different types of controls and fixed effects. Third, within the manufacturing sector, control firms that rent machinery or equipment (instead of buying it) might be also affected by the reform since the accel-

erated depreciation potentially made it cheaper for treated firms to build machinery and equipment and lend them to control firms. In that case, control firms would be positively affected by the reform because the rental price for machinery and equipment could also decrease in response to the accelerated depreciation. While this could lead to a downward bias in our firm-level estimates, we find that the industrial producer price index for the machinery manufacturing industry (NAICS 333) did not change much relative to producer price indices of other manufacturing industries after the reform. Furthermore, the purchase price of machinery and equipment for the manufacturing sector stayed roughly flat compared to that of non-manufacturing sectors. These results (in Appendix B) provide suggestive evidence that the rental price likely stayed flat given that the price of machinery or equipment did not change much compared to prices of other assets after the reform, consistent with findings based on the U.S. bonus depreciation policy (Curtis et al. 2022).

Finally, a remaining concern could be that even if treated and control firms share parallel pre-trends on key outcomes prior to the reform, firms that either rent, outsource, or never buy machinery or equipment may be exposed to differential shocks relative to firms that buy these assets for their production. To address this concern, we drop firms that never buy machinery or equipment from our analysis sample, and redefine the treated and control groups based on the share of their manufacturing assets prior to the reform. For example, our results are qualitatively similar to the specification where we define treated firms as those whose manufacturing assets account for at least 30 percent of their total assets prior to the reform on average, while we define control firms as those whose manufacturing assets account for less than 30 percent of their total assets (but still have a positive share).[9] In other words, our control firms still invested in machinery or equipment prior to the reform, but not as intensively as treated firms did, thereby minimizing the aforementioned concern (see Appendix B).

3.2 Estimating Policy Impacts on Worker Outcomes

To assess the effects of the accelerated depreciation policy on worker-level 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

[9] Our results are qualitatively similar when we use a different cut, although the 30-percent cut gives us the cleanest results in terms of the parallel pre-trends. Results based on other cuts can be provided upon request.

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 policy 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 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 full-time workers who 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 B).

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 employee tax records (T4) 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, investment rate, total revenue, after-tax profits, and return on assets. 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. We define the 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 year t and δ_t is the depreciation rate from our data.[10] After-tax profits are defined as net income after taxes and extraordinary items. Return on assets is defined as net income divided by total assets.

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, employment, average payrolls, and revenue.[11] Another key difference is that control firms have a higher leverage ratio and lower retained earnings (scaled by assets), implying that they are more cash-constrained relative to treated firms. This can help explain why control firms in the manufacturing sector rent machinery or equipment, instead of buying it.[12] However, treated firms and control firms are similar in terms of their investment rate and profitability in the pre-reform period, suggesting that any pre-existing differences in investment rates or profitability are too small to confound the responses in investment and profit margins after the reform. Furthermore, if differences in cash constraints or sizes between treated firms and control firms are

[10] 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 lagged tangible assets.

[11] Note that our estimates on firm outcomes may be understated due to potential mean-reversion, given that treated firms are larger than control firms on average. We conduct a robustness check by matching firms based on pre-reform firm sizes and find quantitatively similar results, thereby minimizing this potential concern.

[12] In Table A.2 in Appendix A, we show descriptive statistics separately for the manufacturing sector and for non-manufacturing sectors. Note that the differences in leverage ratio and retained earnings (scaled by assets) are most pronounced in the manufacturing sector, where control firms by default must be outsourcing or renting machinery or equipment to manufacture goods or services. This further supports that the likely reason why control firms in the manufacturing sector choose to rent machinery or equipment (instead of buying it) is cash constraint.

main confounders, with the global financial crisis coinciding with the reform, we should expect pre-trends on key outcomes (i.e., employment, investment, etc) to be *not* parallel between 2002 and 2006, when the economy was still recovering from the 2000 – 2002 financial crisis.

The majority of treated firms is in the manufacturing sector (70 percent), while the rest of treated firms is in the construction (4 percent), wholesale (9 percent), retail (5 percent), and other services (8 percent), which include administrative support, education, accommodation, and food services. By contrast, the majority of control firms is in the construction (15 percent), retail (20 percent), and other services (25 percent) sectors. Given the difference in the sectoral composition between treated firms and control firms, we control for industry by year fixed effects to absorb any shock 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. In Appendix B, we show qualitatively similar results on earnings, where we match treated workers with control workers based on their industry, age bins, and gender.

4 Results

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

4.1 Capital Investment, Employment, and Average Payrolls

Panels (a) and (b) of Figure 2 plot estimates of θ_τ from equation (1) on investment rates on eligible assets and total tangible assets (including eligible assets) using our analysis sample. As explained in Section 2, the main assets eligible for the accelerated depreciation are machinery and equipment used for manufacturing goods and services, so we define the eligible asset as classes 43, 29, and 53 combined. Then we construct the investment rate as capital expenditures scaled by lagged capital, which captures the annual growth rate as well as the depreciation rate. Panel (a) shows that investment rate in machinery or equipment was relatively flat prior to the reform, dipped slightly in 2007, but began increasing the year after. The initial dip in 2007 is likely driven by the beginning of the global financial crisis, but the quick recovery and subsequent increases are consistent with these firms increasing investment in response to the policy. Since the investment rate on the eligible asset is mechanically zero (by construction) for control firms throughout the sample period, these

estimates simply capture changes in investment rate among treated firms over time; in other words, there is no control group to absorb the effects of the recession (2007 – 2009). To account for this concern, we additionally look at the investment rate on total tangible assets in which control firms also have non-zero investment rates as they invest in other tangible assets (i.e., buildings or computers). Panel (b) shows that treated firms and control firms share a parallel pre-trend in the investment rate prior to the reform. After 2007, the investment rate increased for treated firms relative to control firms, consistent with the increases in the eligible assets.

Columns (1) and (2) of Table 2 present the difference-in-differences estimates on investment rates on the eligible assets and total tangible assets. Column (1) shows that treated firms' investment rate on machinery or equipment increased by 5.4 percentage points (about a 19-percent increase) on average after the reform. Column (2) shows that firms' investment rate on total tangible assets increased by 1.4 percentage points (about a 13-percent increase) on average after the reform. Taken together, these results suggest that firms with any positive investment in machinery or equipment prior to the reform responded significantly to the policy by investing in the eligible assets and in overall tangible assets relative to control firms.

Panels (c) and (d) of Figure 2 plot estimates of θ_τ from equation (1) on employment and average payrolls. Both panels show that treated firms followed a similar pattern as control firms before 2007 for both 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 and pay higher salaries on average. Columns (3) and (4) of Table 2 show that treated firms' employment and average payrolls increased by 4.5 percent and 2.2 percent on average, respectively, implying a significant response in labor demand. Note that average payrolls stayed relatively flat for the first three years after the reform and began increasing afterwards. In the following section, we assess the responses of earnings at the worker-level to estimate the reform's impacts on workers as well as to check whether these dynamic responses in firm-level average payrolls are driven by changes in worker composition.

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 changes in composition 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 worker earnings in treated firms decreased by 1.4 percent on average after 2007.

Next, we examine whether workers from treated firms move to other firms after 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 who continued to stay at treated firms, relative to workers who stayed at control firms, their annual earnings eventually return 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 earnings for workers who leave treated firms after the reform experience a significant and persistent decline in their 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 largely driven by the job movers. In the rest of the paper, we explore potential mechanisms behind the decline in job movers' earnings.

4.3 Sales and Profitability

The increases in employment and average payrolls suggest the decline in worker earnings is not driven by a decrease in overall labor demand among treated firms relative to control firms. 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 4 plots estimates of θ_τ from equation (1) on total revenue, after-tax profits, and return on assets (ROA). 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 return on assets. While the increase in after-tax profits partly reflects a mechanical effect from the tax deductions, the increased ROA suggests that treated firms became more profitable after increasing investment in response to accelerated depreciation. Note that the increases in revenue and after-tax profits happen gradually, similar to investment responses (Panel (a) of Figure 2), which also occur gradually after the reform. For example, it took three years for investment rate on the eligible assets to reach a higher level relative to the pre-reform level, as it did for both revenue and after-tax profits. The timing of these responses suggests that the increased investment may be connected with the increased sales and profitability.

Table 4 presents the difference-in-differences estimates on these firm-level outcomes. Columns

(1) – (3) show that treated firms' total revenue, after-tax profits, and ROA increased by 3.3 percent, 30,201 CAD, and 0.6 percentage points respectively, on average, implying large and significant responses in firm growth and profitability. Overall, these results suggest that the decline in incumbent workers' earnings is not driven by treated firms becoming less profitable after the reform.

4.4 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 shocks 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 provinces and previously excluded sectors (agriculture, finance, and public administration) as part of our main analysis sample, and find qualitatively similar 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. We relax these restrictions and include the previously excluded workers, finding qualitatively similar results. Furthermore, we use an alternative definition of treated and control groups based on the share of its pre-reform investment in manufacturing assets, and find qualitatively similar results. Finally, we also match workers based on their industry, gender, and age, and find similar effects of the reform on the earnings of the matched workers. Results from these robustness tests are included in Appendix B.

5 Potential Mechanisms and Economic Interpretations

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

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. In this section, we examine whether changes

in firm characteristics, such as their industries, sizes, or firm-specific wage premiums, can explain the decline in earnings of the job movers.

5.1.1 Switching Industries or Becoming Unemployed

We first test whether the decline in earnings of job movers can be explained by these workers either switching industries or becoming unemployed. Panels (a) and (b) of Figure 5 show that a non-trivial share of workers moves to firms in different industries or sectors. Columns (1) and (2) of Table 5 show that the share of workers who switches industries or sectors increases by 5 percentage points on average after the reform among the job movers. While this is a non-trivial share, it is unlikely the main explanation behind the decline in their earnings. Furthermore, Panel (c) of Figure 5 and Column (3) of Table 5 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 becoming unemployed after leaving their firm.

5.1.2 Changes in Firm Size

Given that the vast majority of job movers from treated firms transition to different firms in the same industry after the reform, we next examine the characteristics of these new employers. Panel (a) of Figure 6 shows that workers who 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 the original employer's 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 6 shows that these workers move to firms with 52.3 log points lower employment on average. Next, we estimate firm-specific wage premiums to see whether these firms have lower wage premiums on average.

5.1.3 Changes in Employer Fixed Effects

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 help 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 (3), 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 6 shows that workers who leave treated firms after the reform move to firms with lower wage premiums on average. Column (2) of Table 6 shows that these workers move to firms with 2.3 log points lower firm-specific wage premiums on average. To summarize, we find that workers who transition jobs after the accelerated depreciation policy move to smaller firms with lower wage premiums relative to job movers from control firms.

5.1.4 Changes in Firms' Investment

Next, we assess whether workers who leave treated firms move to other firms that are not only smaller (in terms of employment and AKM), but also are less likely to invest in new machinery or equipment. Panel (c) of Figure 6 shows that workers who leave treated firms after the reform move to firms with a much lower likelihood of purchasing any machinery or equipment in a given year on average. Column (3) of Table 6 shows that these workers move to firms with a 42 percentage points lower chance of investing in any manufacturing assets on average. This result implies that workers who leave treated firms move to other firms with relatively lower demand for machinery and equipment on average. Next, we explore potential mechanisms behind the decline in worker earnings and wage premiums of job movers from treated firms after the reform by looking at the type of job separations and worker characteristics.

5.2 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 reform.[13] 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 who 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 for many reasons (i.e., search frictions). However, since control workers are also displaced from their firm after the reform, it suggests that job movers from treated firms face even more difficulties in finding new employers with comparable wages relative to displaced workers from control firms. We hypothesize that this is due to changes in relative demand for high-skilled workers among treated firms after the policy change.

Panel (a) of Figure 7 shows that earnings of workers at treated firms that move to other firms, separately for those that move voluntarily and for those who move involuntarily (i.e., displaced). Relative to their control workers, those who move involuntarily show a larger decline in earnings compared to those who move voluntarily after the event. Panel (b) shows that these workers move to firms with lower wage premiums on average. Table 7 confirms that the decline in worker earnings and firm-specific wage premiums is much larger for workers who 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, as the relative demand for high-skilled workers increases among treated firms after the accelerated depreciation policy. By contrast, since the relative demand remains unchanged among control firms, displaced workers from control firms suffer a smaller decline in their earnings relative to displaced workers from treated firms.

5.3 Heterogeneity by Worker Characteristics

Relatedly, we consider heterogeneity by the type of workers. Intuitively, low-skilled workers who get displaced 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. To proxy the skill type, we exploit two sources of variation: (1) within-firm earnings distribution and (2) worker fixed effects. 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 or equipment after the

[13] 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.

reform. Similarly, workers with high fixed effects estimated from equation (5) may be deemed as highly skilled by their original employer that buy new machinery or equipment. 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 at the bottom 75 percentile, and we find even an increase in earnings for workers at the top 25 percentile. In Figure 10, we plot the impacts for job movers across different quartiles of the worker fixed effects. Similar to the previous result, we find that the decline in earnings is concentrated among workers at the bottom 50 percentile, and we find even an increase in earnings for workers at the top 25 percentile.

Additionally, we find that bottom-quartile workers move to worse-paying firms on average. As shown in Panel (b) of Figure 8 and Figure 9, job movers at the bottom 50 percentile are moving to firms with lower levels of AKM effects. Table 8 shows that job movers at the bottom quartile of the within-firm earnings distribution move to firms with 4.4 log points lower wage premiums, and experience a 8.1 log points decrease in their annual earnings after the reform. Similarly, Table 9 shows that job movers at the bottom quartile of the worker fixed effects move to firms with 4.2 log points lower wage premiums, and experience 11.5 log points decline in their annual earnings after the reform. Our worker heterogeneity results are consistent with the idea that those in the lower within-firm wage distribution or with lower worker fixed effects suffer the larger earnings losses after the reform, likely due to their skills incompatible with the increased manufacturing investment among treated firms.

5.4 Changes in Within-firm Earnings Inequality

Previous results show that job movers from treated firms transition to smaller firms with lower wage premiums on average after the reform. Furthermore, we find the decline in earnings and wage premiums is concentrated among job movers who are displaced, in the lower quantile of the within-firm earnings distribution, or with lower worker fixed effects, suggesting that the decline is likely driven by changes in relative demand for high-skilled workers among treated firms to complement their investment in new machinery or equipment after the reform. Next, we examine whether these changes in relative demand for skilled workers lead to changes in within-firm wage inequality among treated firms, thereby providing evidence of skill-biased technological change.

Panel (a) of Figure 10 shows the estimates on the log of the average earnings gap between workers at the top 10 percentile and those at the bottom 10 percentile in the within-firm earnings distribution, defined as $log(\frac{\text{Earnings}_{jt}^{90}}{\text{Earnings}_{jt}^{10}})$. Column (1) of Table 10 shows that the earnings gap increases by 2.1 log points, which are large and statistically significant. Panel (c) and (e) of Figure 10 show the estimates on log(earnings) separately for workers at the top 10 percentile and for workers at

the bottom 10 percentile, respectively. Columns (2) and (3) of Table 10 confirm that the increase in the earnings gap is largely driven by the increase in earnings of workers at the top 10 percentile.

Since this increase in the wage gap can be driven by changes in worker composition, Panel (b) of Figure 10 shows these estimates only for workers who continue to stay at their original firm after the reform, thereby holding the worker composition fixed. Column (4) of Table 10 shows that the earnings gap increases by 7.5 log points, which are even larger than the previous estimate. Panel (d) and (f) of Figure 10 show the estimates on log(earnings) separately for workers at the top 10 percentile and for workers at the bottom 10 percentile, respectively, among stayers. Columns (5) and (6) show that the increase in the earnings gap among stayers is driven by both an increase in earnings of stayers at the top 10 percentile and a decrease in earnings of stayers at the bottom 10 percentile. In Appendix B, we repeat the same exercise looking at the earnings gap between workers at the top 25 percentile and those at the bottom 25 percentile, and find qualitatively similar results. Taken together, these results show direct evidence of skill-biased technological change induced by a tax policy designed to induce manufacturing investment.

5.5 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 historical statutory 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 the additional revenue in the future. These estimates yield an estimate of the cost equal to 3,255 CAD per job.

Our estimated cost-per-job is much smaller than the estimate of 20,000 USD per job from Garrett et al. (2020), which assesses the bonus depreciation policy in the United States. While this may seem like the Canadian acceleration depreciation policy was more efficient at creating jobs relative to the bonus depreciation policy in the U.S., several factors may explain the discrepancy in estimates across different settings. First and foremost, Garrett et al. (2020) does not directly observe $CCA_t^{accelerated} - CCA_t^{normal}$, but instead relies on past (conservative) estimates to approximate its value, potentially leading to an overestimate on the cost-per-job in their setting. By contrast, we directly estimate it using our tax records, allowing us to measure the fiscal cost more accurately. Second, differences in firm characteristics or institutional differences may result in different responses to the tax incentive, potentially leading treated firms in our setting to hire more workers for the same change in the cost of capital. While understanding the precise source of differences between these estimates is difficult and beyond the scope of our study, our cost-per-job estimate shows that the accelerated depreciation policy was effective in inducing job creation and investment in the Canadian economy, despite having negative impacts on low-skilled workers.

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. In addition, we find the decrease in earnings is concentrated among job movers from the lower quantile of the within-firm earnings distribution or worker fixed effects. Furthermore, the within-firm wage inequality increased significantly among treated firms relative to control firms after the reform, thereby providing evidence of skill-biased technological change induced by a tax policy. 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 who are replaced by high-skilled workers.

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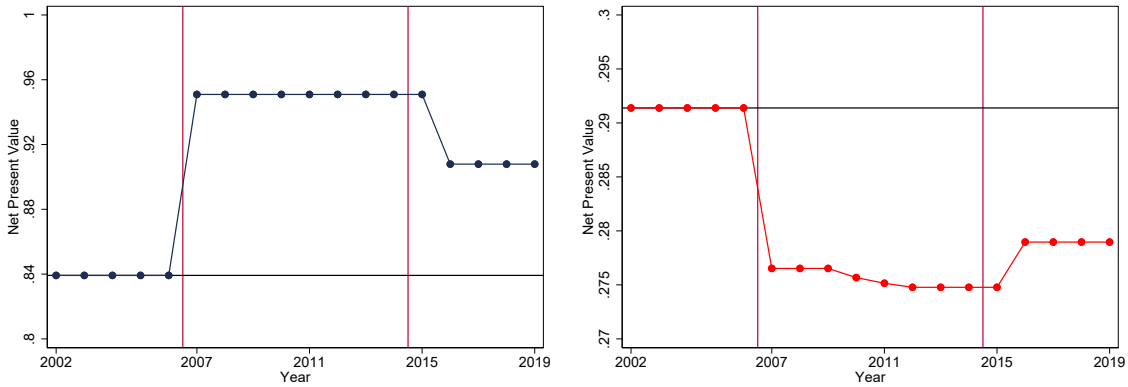
Figure 1: Changes in NPV of CCA and Cost of Capital

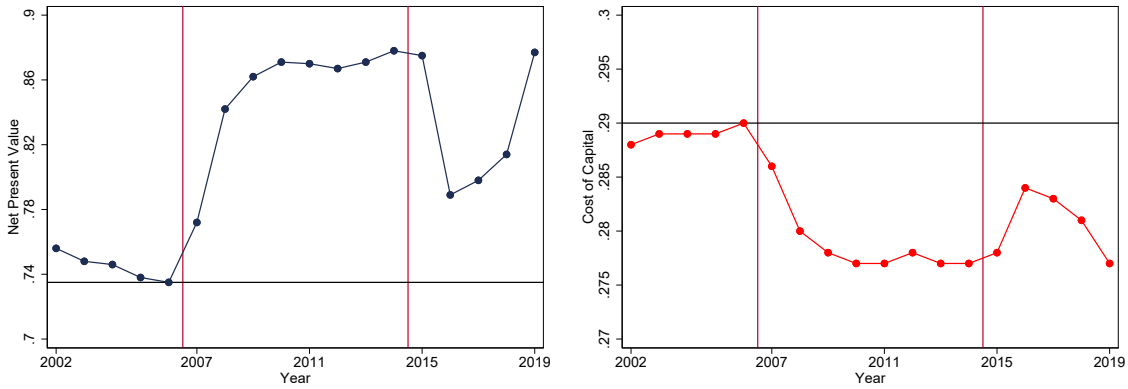
(a) Changes in NPV of CCA

(b) Changes in Cost of Capital



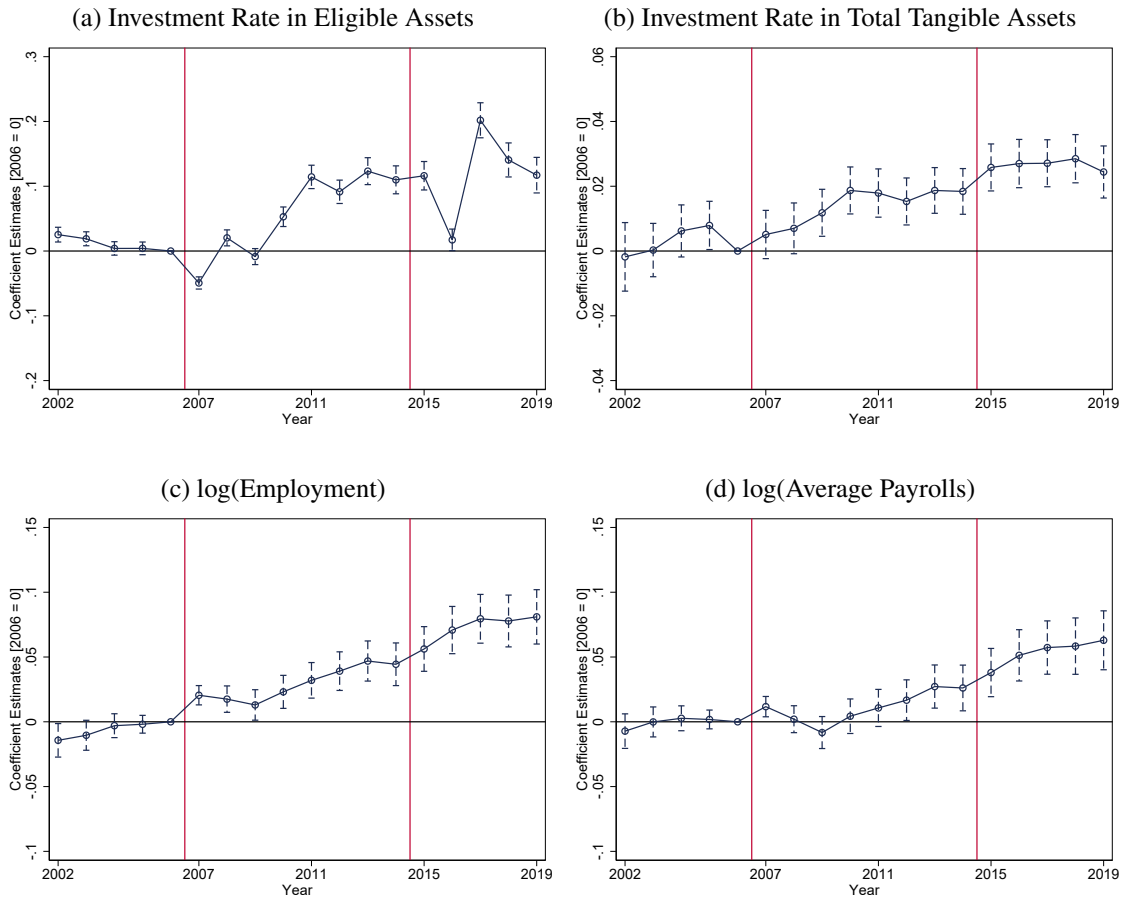
(c) Changes in NPV of CCA

(d) Changes in Cost of Capital



Notes: Panel (a) shows changes in the net present value (NPV) of one dollar of capital cost allowance (CCA) on assets related to machinery and equipment for manufacturing goods and services in 2002 – 2019. In 2007, the federal government increased the CCA rate for machinery and equipment from 30 percent to 50 percent and switched from the declining balance method to a straight-line rule. In 2016, the CCA rate remained at 50 percent, but switched back to the declining-balance method. Panel (b) 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 are minimal in our setting. Panels (c) and (d) show changes in NPV and cost of capital similar to ones in Panels (a) and (b). The key difference is that in Panels (c) and (d), we calculate the NPV and the cost of capital using actual depreciation rates from our data instead of statutory CCA rates. Actual depreciation rates are computed based on firms' claims for capital cost allowance scaled by their capital stock at the end of the year, both reported in firms' T2 Schedule 8 by each asset class, and averaged across asset classes by firms' expenditure share. The changes in average NPV and average cost of capital after the reform are 0.101 percentage points and -0.011 percentage points, respectively.

Figure 2: Capital Investment, Employment, and Average Payrolls



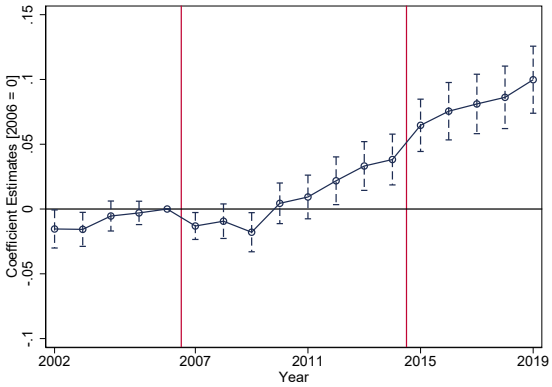
Notes: Panels (a) – (d) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times \textit{Treated}$ in equation (1) for firms' investment rate on ACCA-eligible assets (machinery and equipment), investment rate on total tangible assets, log(employment), and log(average payrolls), respectively. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 3: Worker-level Outcomes

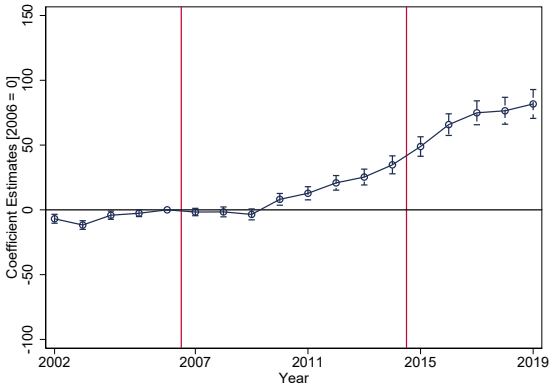
Notes: Panels (a) and (b) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times \textit{Treated}$ in equation (3) for workers' log(annual earnings) and job transition probabilities, respectively. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are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

Figure 4: Sales and Profitability

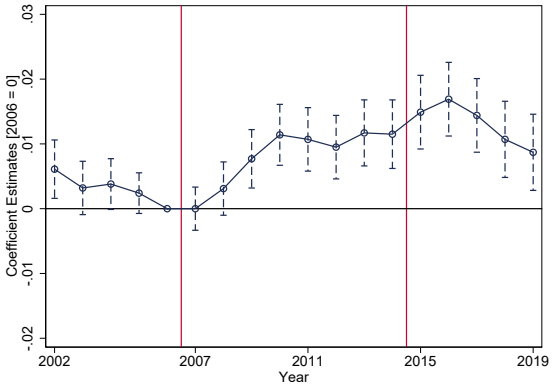
(a) log(Revenue)



(b) After-tax Profits



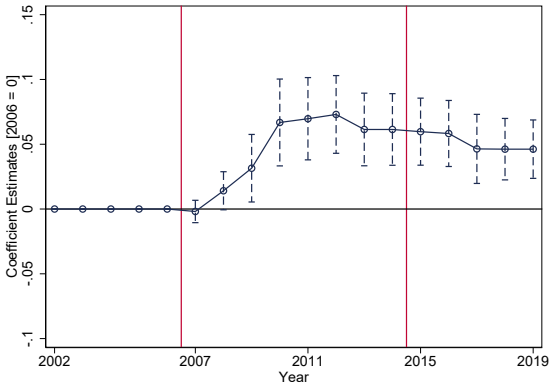
(c) Return on Assets



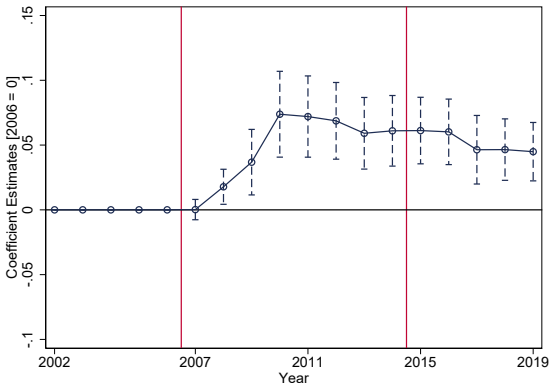
Notes: Panels (a) – (c) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (1) for firms' log(revenue), after-tax profits, and return on assets (ROA), respectively. After-tax profits are defined as firms' net income after taxes and extraordinary items and are measured in thousand CAD. Return on assets are defined as net income scaled by total assets. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 5: Job Movers Switching Industries or Becoming Unemployed

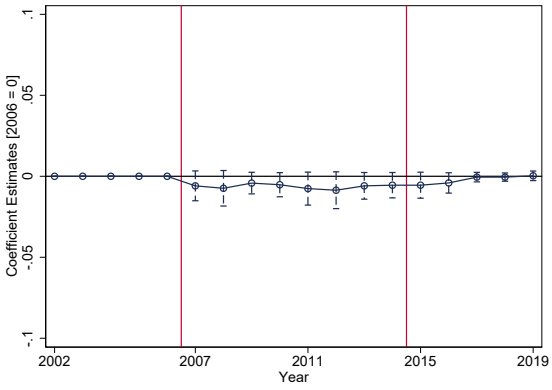
(a) Switching Industries



(b) Switching Sectors



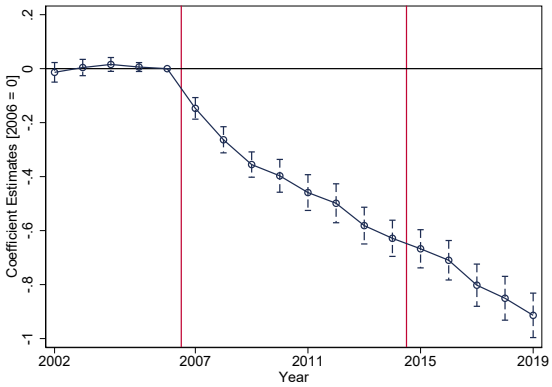
(c) Unemployed



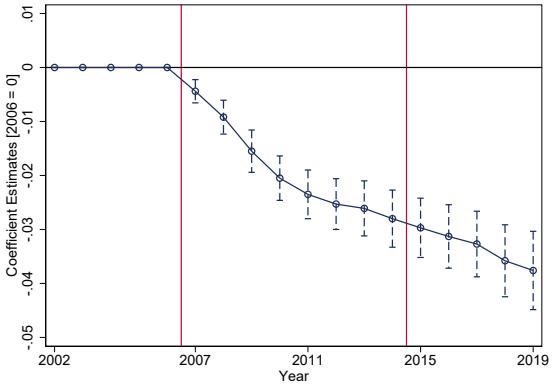
Notes: Panels (a) – (c) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times \textit{Treated}$ in equation (3) for the share of workers who move to different 4-digit NAICS industries (from the ones in 2006), to different 2-digit NAICS sectors, and becoming unemployed, respectively. The sample consists of workers who leave their firms after 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. Panels (a) and (b) focus on years when the workers are employed, whereas panel (c) includes unemployed years. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 6: Changes in Firm Characteristics of Job Movers

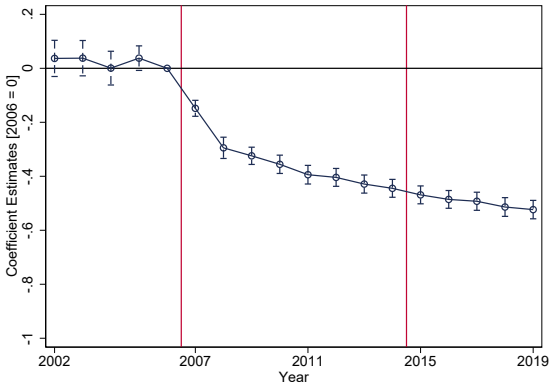
(a) log(Employment)



(b) AKM Firm Effects



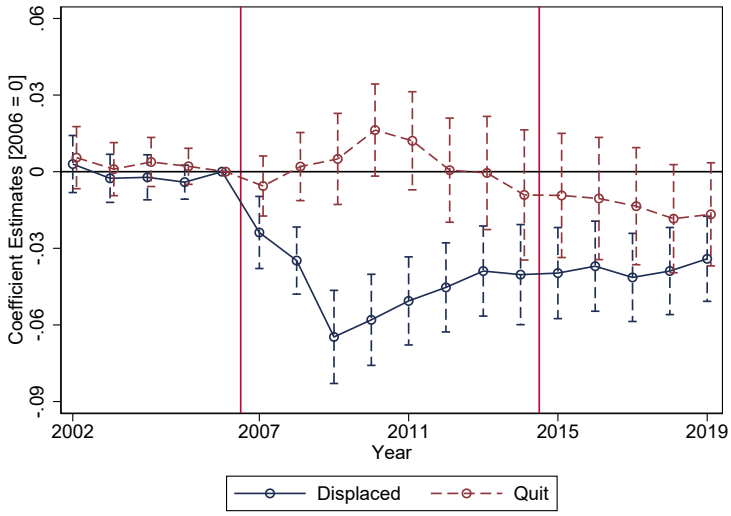
(c) Invest in Manufacturing Assets



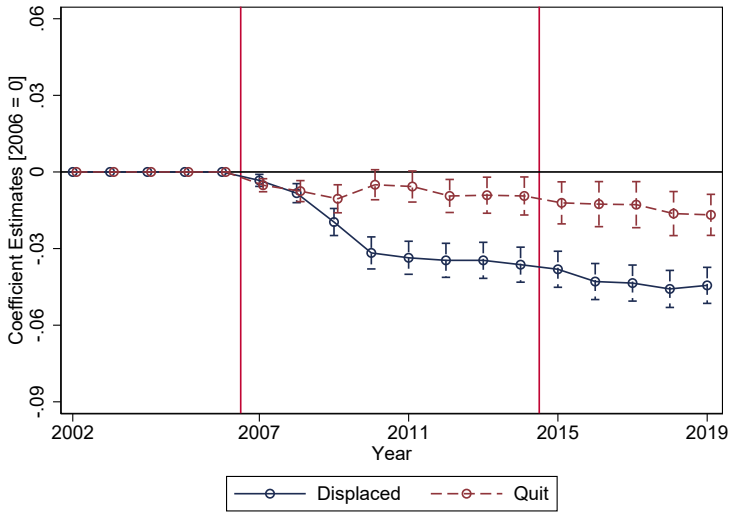
Notes: Panels (a) – (c) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for log(employment), AKM firm effects, and whether the firm invests in machinery or equipment, respectively, for job movers' employers. The sample consists of workers who leave their firms after 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. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 7: Heterogeneity by Type of Job Separations

(a) log(Annual Earnings)



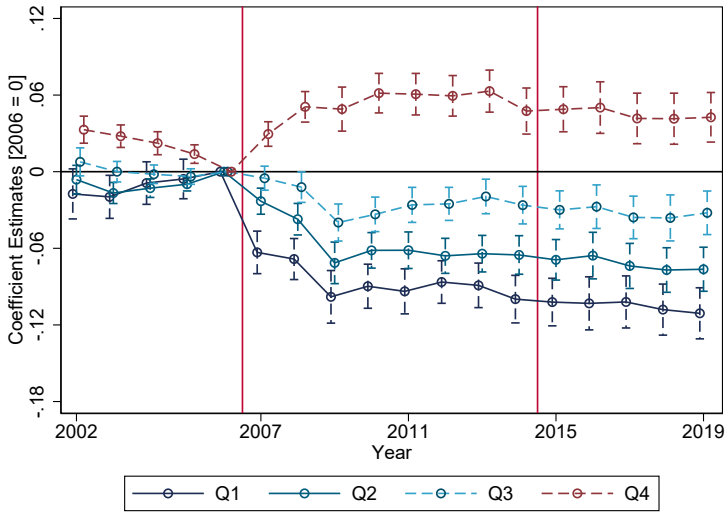
(b) AKM Firm Effects



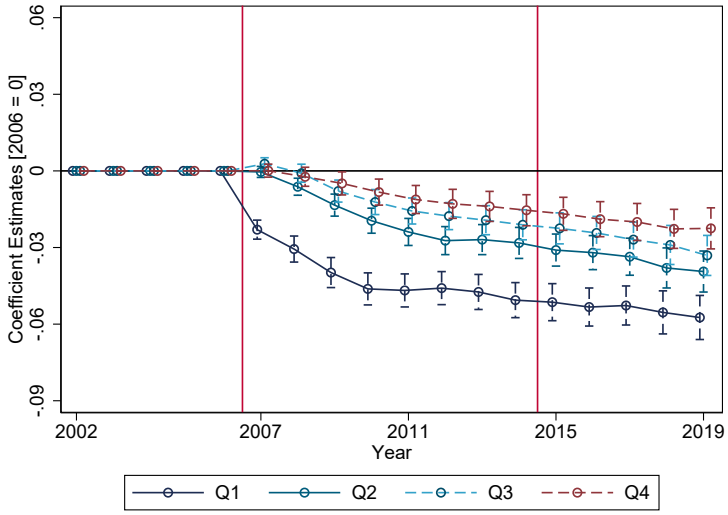
Notes: Panels (a) and (b) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for workers' log(annual earnings) and AKM firm effects, separately for workers who move involuntarily to other firms ("Displaced") and for workers who move voluntarily ("Quit"). The sample consists of workers who leave their firms after 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. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 8: Heterogeneity by Workers' Pre-Reform Earnings

(a) log(Annual Earnings)



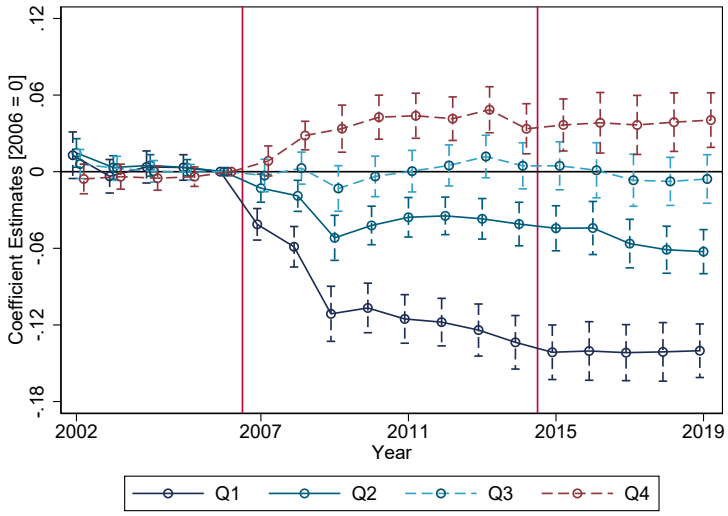
(b) AKM Firm Effects



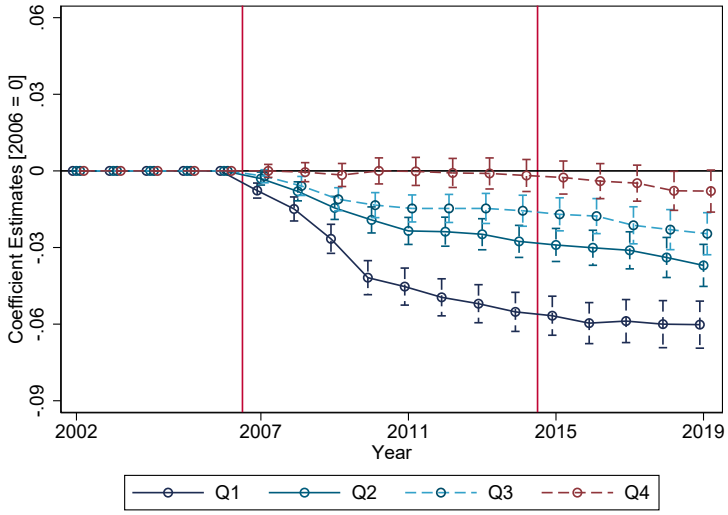
Notes: Panels (a) and (b) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for workers' log(annual earnings) and AKM firm effects, separately for job movers in each quartile of the within-firm distribution of annual earnings (measured in 2006). The sample consists of workers who leave their firms after 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. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 9: Heterogeneity by Worker Fixed Effects

(a) log(Annual Earnings)

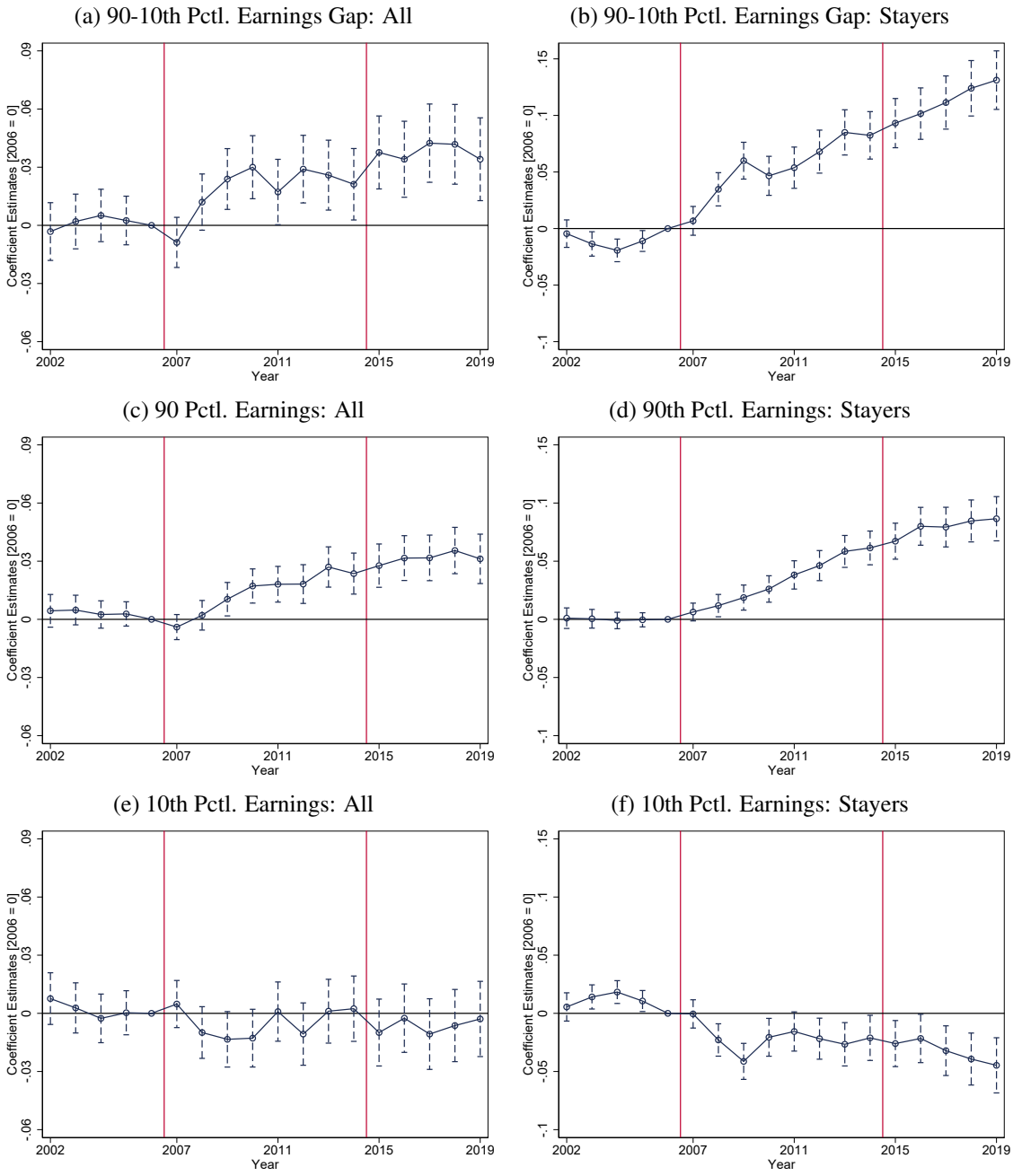


(b) AKM Firm Effects



Notes: Panels (a) and (b) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times \textit{Treated}$ in equation (3) for workers' log(annual earnings) and AKM firm effects, separately for job movers in each quartile of the distribution of AKM worker effects. The sample consists of workers who leave their firms after 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. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 10: Within-firm Earnings Inequality



Notes: Panels (a), (c), and (e) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (1) for the 90th-10th percentile gap, the 90th percentile, and the 10th percentile, respectively, of workers' log(annual earnings) within each firm. The 90th-10th percentile gap is defined as $\log(\text{earnings})_{jt}^{p90}$ - $\log(\text{earnings})_{jt}^{p10}$. Panels (b), (d), and (f) show the estimates using workers who stay at their pre-reform employers after 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. The solid vertical lines indicate 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)	2543.0	766.2
Investment Rate	0.112	0.123
Employment	39.7	17.9
Average Payrolls ('000)	124.2	50.1
Revenue ('000)	7799.1	3188.7
After-tax Profits ('000)	110.8	62.0
Profit Margins	0.037	0.039
Leverage Ratio	0.659	0.730
Retained Earnings / Assets	0.270	0.216
Firm Age	11.5	9.9
Sectors:		
Utility/Mining	0.006	0.008
Construction	0.041	0.149
Manufacturing	0.700	0.058
Wholesale Trade	0.088	0.087
Retail Trade	0.050	0.201
Transportation	0.004	0.045
Professional Services	0.026	0.122
Education/Healthcare	0.006	0.081
Other Services	0.079	0.249
Number of Firms	15,480	141,330
B. Worker Characteristics		
Annual Earnings ('000)	52.0	47.0
Male	0.717	0.596
Age	43.6	44.3
Sectors:		
Utility/Mining	0.062	0.022
Construction	0.016	0.131
Manufacturing	0.757	0.055
Wholesale Trade	0.070	0.106
Retail Trade	0.045	0.185
Transportation	0.003	0.122
Professional Services	0.017	0.117
Education/Healthcare	0.007	0.082
Other Services	0.023	0.181
Number of Workers	400,675	791,735

Notes: Panels A and B report summary statistics for firms and workers in our analysis sample, respectively. The firm sample consists of CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec with at least five employees in 2006. Treated and control firms are defined, respectively, as those with and without any investment in machinery or equipment for manufacturing goods in 2002 – 2006. The worker sample consists of workers at the treated and control firms in 2002 – 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 in 2002 – 2006. Other services sector includes real estate, rental and leasing, management, administrative support, arts and entertainment, accommodation and food services.

Table 2: Capital Investment, Employment, and Average Payrolls

	(1) Investment Rate in Eligible Assets	(2) Investment Rate in Tangible Assets	(3) log(Employment)	(4) log(Average Payrolls)
Post × Treated	0.0543***	0.0141***	0.0451***	0.0219***
	(0.0040)	(0.0020)	(0.0062)	(0.0065)
Mean Dep. Var.	0.289	0.112	39.7	124.2
Observations	1,935,975	1,840,290	1,956,395	1,956,395
Treated Firms	15,285	14,930	15,480	15,480
Control Firms	141,330	126,120	141,330	141,330
Adjusted R^2	0.307	0.144	0.821	0.852

Notes: Columns (1) to (4) report coefficient estimates on *Post* × *Treated* in equation (2) for firms' investment rate on ACCA-eligible assets (machinery and equipment), investment rate on total tangible assets, log(employment), and log(average payrolls), respectively. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD for column (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): Stayers	(4) log(Annual Earnings): Movers
Post × Treated	-0.0139***	0.0001	-0.0055	-0.0297***
	(0.0040)	(0.0041)	(0.0034)	(0.0051)
Mean Dep. Var.	52.0	0.000	52.0	50.1
Observations	17,809,335	17,809,335	13,404,625	9,773,525
Treated Workers	400,675	400,675	400,675	205,895
Control Workers	791,735	791,735	791,735	409,745
Adjusted R^2	0.701	0.121	0.792	0.641

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. Column (3) shows the estimates on log(annual earnings) for workers who stayed with their pre-reform employers after 2006 and tracks them until they leave their firms. Column (4) shows the estimates on log(annual earnings) for workers who leave their firms after 2006. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD for columns (1), (3) and (4). 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: Sales and Profitability

	(1) log(Revenue)	(2) After-tax Profits	(3) Returns on Assets
Post × Treated	0.0325***	30.2014***	0.0060***
	(0.0070)	(1.9185)	(0.0014)
Mean Dep. Var.	7799.1	110.8	0.090
Observations	1,956,395	1,956,395	1,898,320
Treated Firms	15,480	15,480	14,985
Control Firms	141,330	141,330	131,305
Adjusted R^2	0.879	0.528	0.393

Notes: Columns (1) – (3) report coefficient estimates on *Post* × *Treated* in equation (2) for firms' log(revenue), after-tax profits, and return on assets, respectively. After-tax profits are defined as firms' net income after taxes and extraordinary items. Return on assets are defined as net income scaled by total assets. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD for columns (1) – (2). 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: Destination of Job Movers

	(1) Switching Sectors	(2) Switching Industries	(3) Unemployed
Post × Treated	0.0486***	0.0472***	-0.0050
	(0.0098)	(0.0100)	(0.0036)
Mean Dep. Var.	0.000	0.000	0.000
Observations	9,773,525	9,773,525	10,243,040
Treated Workers	205,895	205,895	205,895
Control Workers	409,745	409,745	409,745
Adjusted R^2	0.486	0.521	0.205

Notes: Columns (1) – (3) report coefficient estimates on *Post* × *Treated* in equation (4) for workers' probabilities of moving to different 4-digit NAICS industries (from the one in 2006), to different 2-digit NAICS sectors, and becoming unemployed, respectively. The sample consists of workers who leave their firms after 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. Columns (1) – (2) focus on years when the workers are employed, whereas column (3) includes unemployed years. The mean for each dependent variable is based on years 2002 – 2006. 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 6: Firm Characteristics of Job Movers

	(1) log(Employment)	(2) AKM Firm Effects	(3) Invest in Manufacturing
Post × Treated	-0.5231***	-0.0227***	-0.4163***
	(0.0275)	(0.0021)	(0.0148)
Mean Dep. Var.	64.4	-0.110	0.800
Observations	8,627,050	8,968,675	9,773,525
Treated Workers	205,895	205,895	205,895
Control Workers	409,745	408,835	409,745
Adjusted R^2	0.735	0.815	0.644

Notes: Columns (1) – (3) report coefficient estimates on *Post* × *Treated* in equation (4) for log(employment), AKM firm effects, and whether the firm invests in manufacturing equipment, respectively, for job movers' employers. The sample consists of workers who leave their firms after 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. The mean for each dependent variable is based on years 2002 – 2006. 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: Heterogeneity by Type of Job Separations

	(1) log(Annual Earnings)	(2) AKM Firm Effects
Post × Treated (Quit)	-0.0050	-0.0091***
	(0.0086)	(0.0028)
Post × Treated (Displaced)	-0.0414***	-0.0297***
	(0.0073)	(0.0026)
Difference	-0.0364***	-0.0205***
	(0.0087)	(0.0028)
Mean Dep. Var. (Quit)	43.7	0.037
Mean Dep. Var. (Displaced)	47.6	0.137
Observations	5,055,490	4,630,980
Treated Workers	117,925	117,925
Control Workers	202,955	202,880
Adjusted R^2	0.606	0.780

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 who leave involuntarily ("Displaced") and for workers who leave voluntarily ("Quit") after the reform. The sample consists of workers who leave their firms after 2006 with non-missing information on the reasons for separation. 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 and measured in thousand CAD for column (1). 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 Within-firm Earnings Distribution

	(1) log(Annual Earnings)	(2) AKM Firm Effects
Post × Treated (1st Quantile)	-0.0814***	-0.0444***
	(0.0077)	(0.0028)
Post × Treated (2nd Quantile)	-0.0518***	-0.0225***
	(0.0056)	(0.0023)
Post × Treated (3rd Quantile)	-0.0266***	-0.0155***
	(0.0055)	(0.0023)
Post × Treated (4th Quantile)	0.0300***	-0.0116***
	(0.0077)	(0.0023)
Mean Dep. Var. (1st Quantile)	32.5	0.133
Mean Dep. Var. (2nd Quantile)	40.6	0.134
Mean Dep. Var. (3rd Quantile)	50.2	0.138
Mean Dep. Var. (4th Quantile)	77.0	0.138
Observations	9,746,905	8,946,195
Treated Workers	205,830	205,830
Control Workers	408,015	407,190
Adjusted R^2	0.643	0.805

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 in each quartile of the within-firm distribution of annual earnings (measured in 2006). The sample consists of workers who leave their firms after 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. Firms with fewer than four employees in 2006 are excluded. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD for column (1). 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 Worker Fixed Effects

	(1) log(Annual Earnings)	(2) AKM Firm Effects
Post × Treated (1st Quantile)	-0.1149***	-0.0417***
	(0.0088)	(0.0028)
Post × Treated (2nd Quantile)	-0.0454***	-0.0218***
	(0.0059)	(0.0024)
Post × Treated (3rd Quantile)	-0.0024	-0.0138***
	(0.0065)	(0.0024)
Post × Treated (4th Quantile)	0.0393***	-0.0019
	(0.0083)	(0.0024)
Mean Dep. Var. (1st Quantile)	34.2	0.160
Mean Dep. Var. (2nd Quantile)	41.8	0.138
Mean Dep. Var. (3rd Quantile)	50.0	0.139
Mean Dep. Var. (4th Quantile)	74.1	0.128
Observations	8,705,060	8,001,105
Treated Workers	199,555	199,275
Control Workers	347,805	347,015
Adjusted R^2	0.646	0.805

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 in each quartile of the distribution of AKM worker fixed effects. The sample consists of workers who leave their firms after 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. Firms with fewer than four employees in 2006 are excluded. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD for column (1). 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 10: Within-firm Earnings Inequality

	(1) 90-10th Percentile Gap log(Annual Earnings)	(2) 90th Percentile log(Annual Earnings)	(3) 10th Percentile log(Annual Earnings)
All Workers			
Post × Treated	0.0211***	0.0143***	-0.0067
	(0.0049)	(0.0033)	(0.0042)
Mean Dep. Var.	1.533	59.2	13.5
Observations	1,924,090	1,924,090	1,924,090
Treated Workers	15,470	15,470	15,470
Control Workers	140,425	140,425	140,425
Adjusted R^2	0.405	0.765	0.559
	(4) 90-10th Percentile Gap log(Annual Earnings)	(5) 90th Percentile log(Annual Earnings)	(6) 10th Percentile log(Annual Earnings)
Stayers			
Post × Treated	0.0754***	0.0423***	-0.0332***
	(0.0059)	(0.0047)	(0.0052)
Mean Dep. Var.	1.002	70.5	25.4
Observations	1,431,635	1,431,635	1,431,635
Treated Workers	12,795	12,795	12,795
Control Workers	98,770	98,770	98,770
Adjusted R^2	0.605	0.779	0.628

Notes: Columns (1) – (3) show coefficient estimates on *Post* × *Treated* in equation (2) for the 90th-10th percentile gap, the 90th percentile, and the 10th percentile, respectively, of workers' log(annual earnings) within each firm. The 90th-10th percentile gap is defined as $\log(\text{earnings})_{jt}^{p90}$ - $\log(\text{earnings})_{jt}^{p10}$. Columns (4) – (6) show the estimates using workers who stay at their pre-reform employers after 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. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD for columns (2), (3), (5), and (6). All specifications include firm fixed effects and industry by year fixed effects. Standard errors are clustered at the firm 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 Additional Descriptive Statistics and Institutional Background

In Appendix A, we provide further descriptive statistics from our data and institutional details regarding the corporate income tax rates in Canada.

Panel (a) and (b) of Figure A1 describe the distribution of top ten industries within treated firms and control firms, respectively, in our analysis sample. For example, "printing" industry accounts for the largest share within the treated group, while "building equipment contractor" industry accounts for the largest share within the control group. Figure A2 shows these separately for the manufacturing sector and for non-manufacturing sectors. For example, within the manufacturing sector, "printing" industry accounts for the largest share of firms both among treated firms and control firms. Within non-manufacturing sectors, "grocery store" industry accounts for the largest share of treated firms, while "building equipment contractor" industry accounts for the largest share of control firm. Among the top five industries within the manufacturing sector, three industries overlap between the treated group and the control group: (1) "printing" industry, (2) "furniture and kitchen cabinet" industry, and (3) "architectural and structural metal" industry. Among the top five industries within non-manufacturing sector, two industries overlap between the treated group and the control group: (1) "grocery store" industry and (2) "restaurant" industry.

Figure A3 shows the share of new entrants and those that exit the sample across years, separately for the treated group and for the control group. From 2002 to 2006, treated and control firms show parallel trends in entry rates, but a large and persistent decrease in entry rates after 2006. Similarly, the treated and control groups show parallel trends in exit rates before 2006, but a sharp and persistent increase in exit rates after 2006.

Table A1 describes federal and provincial corporate income tax rates from 2002 to 2008. Alberta, British Columbia, Ontario, and Quebec experience some non-trivial changes in the general or small business tax rates between 2002 and 2008. There are also some changes in the general and small business tax rates both at the federal and provincial level after 2008, and the post-2008 table can be found in Appendix A of Duan and Moon (2023). In Appendix B, we show results including previously excluded provinces (besides Alberta, British Columbia, Ontario, and Quebec).

Table A2 shows descriptive statistics for firms and workers in our analysis sample, separately for those in the manufacturing sector and for those in non-manufacturing sectors. Consistent with Table 1, the differences in firm sizes (i.e., tangible assets, employment, average payrolls, and revenue) between treated firms and control firms persist, whether we look at the manufacturing sector and non-manufacturing sectors separately, or look at them together. However, the differences in leverage ratio and retained earnings (scaled by total assets) between treated firms and control firms are more pronounced in the manufacturing sector, consistent with the idea that manufacturing firms

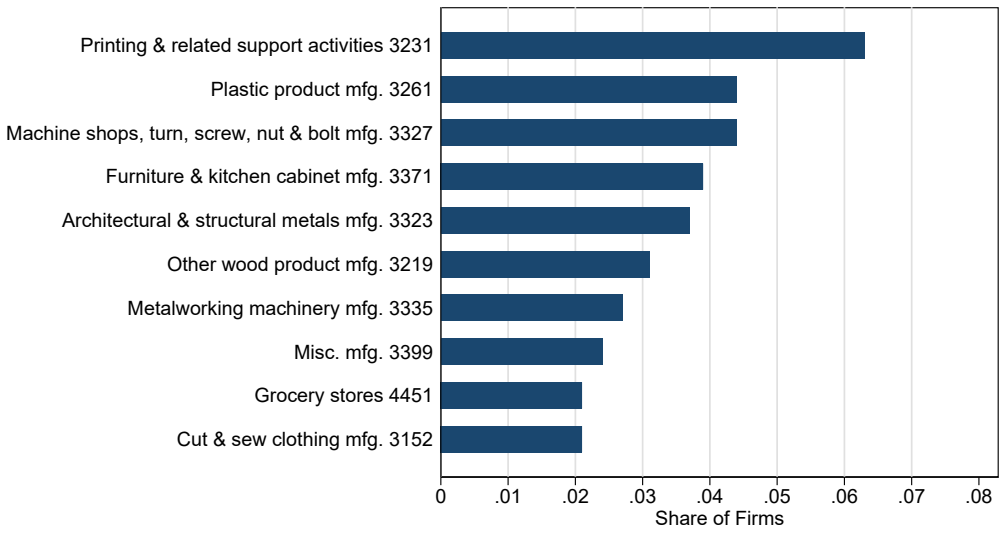
in the control group choose to rent machinery or equipment likely due to cash-constraints. Besides this, treated firms and control firms are still quite comparable in terms of investment rate, profit margins, and firm age.

Table A3 describes the type of assets that treated and control firms invested prior to the reform. On average, treated firms and control firms have a similar share of buildings as part of total assets on average, although the control firms have a larger share of buildings in the manufacturing sector, while the treated firms have a larger share within non-manufacturing sectors. Furthermore, control firms have a much larger share of assets invested in computers (while having a zero share in machinery or equipment by construction). Besides investing in computers, control firms also have a large share invested in miscellaneous assets (i.e., data network infrastructure equipment and systems software for that equipment)

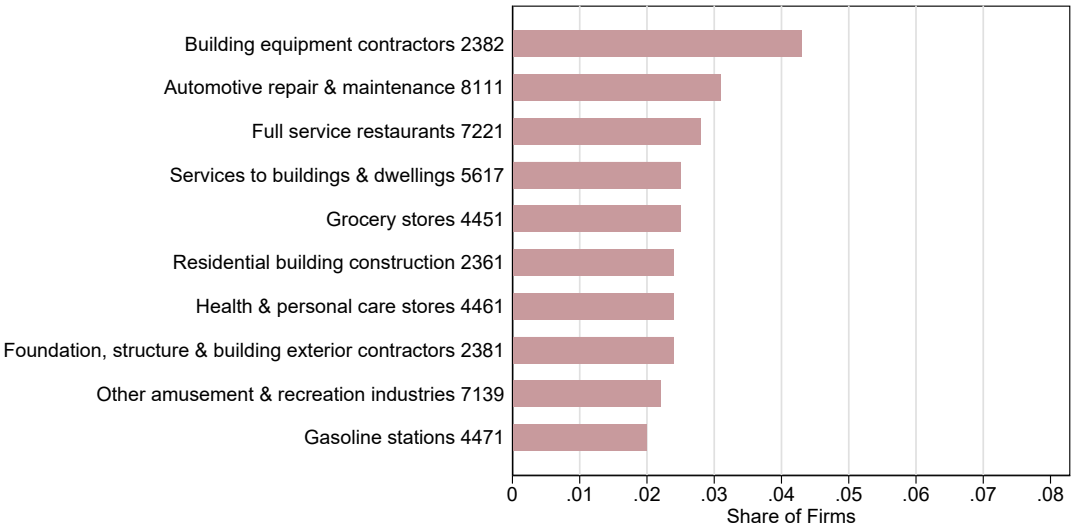
Columns (1) and (2) of Table A4 show changes in firms' net-present value of one dollar of capital cost allowance and the cost of capital for investment in buildings. Columns (3) and (4) show the estimates for investment in computers and software. We compute the net present value and the cost of capital using actual depreciation rates based on firms' claims for capital cost allowance scaled by their capital stock at the end of the year. As shown in the table, the estimated changes in the net-present value and the cost of capital of one dollar of investment in buildings and computers are much smaller than those of one dollar of investment in machinery and equipment for treated firms relative to control firms because control firms also invest in buildings and computers and would therefore benefit from the accelerated depreciation policy on these other asset classes. These results suggest that the impacts of the ACCA on other asset classes (besides machinery and equipment) are muted in a difference-in-differences framework in our setting.

Figure A1: Distribution of Industries

(a) Treated



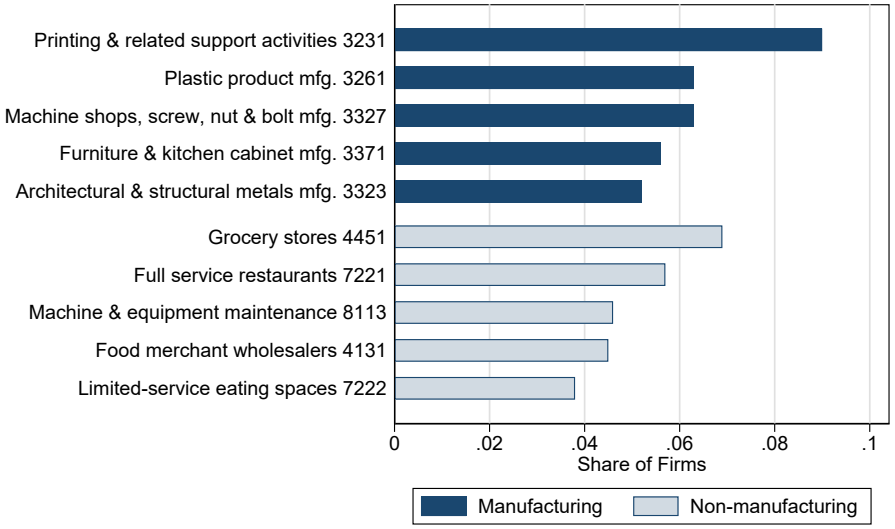
(b) Control



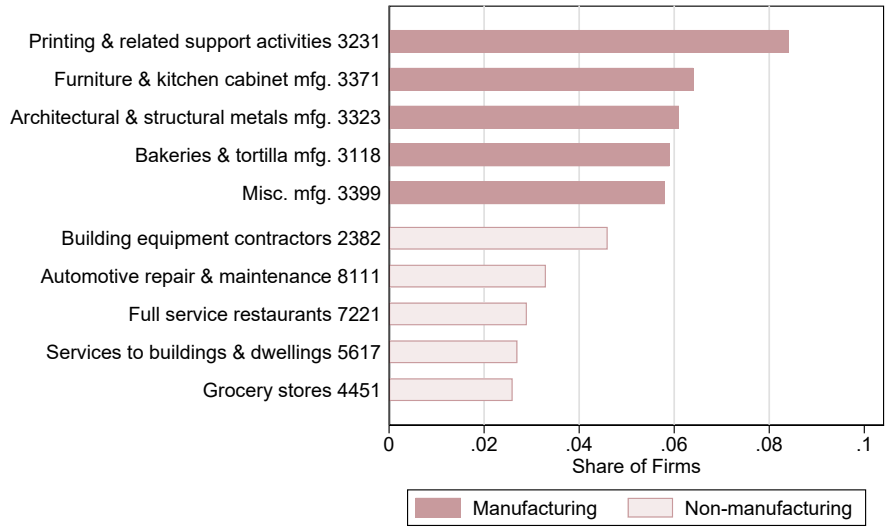
Notes: Panels (a) and (b) show the shares of treated and control firms, respectively, in each of the top 10 industries (4-digit NAICS). The firm sample consists of CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec with at least five employees in 2006. Treated and control firms are defined, respectively, as those with and without any investment in machinery or equipment for manufacturing goods in 2002 – 2006.

Figure A2: Distribution of Industries: Manufacturing Sector vs. Non-manufacturing Sectors

(a) Treated



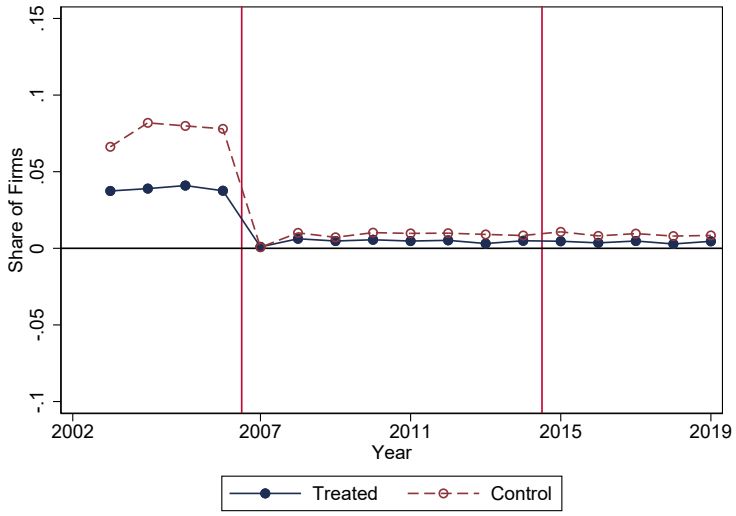
(b) Control



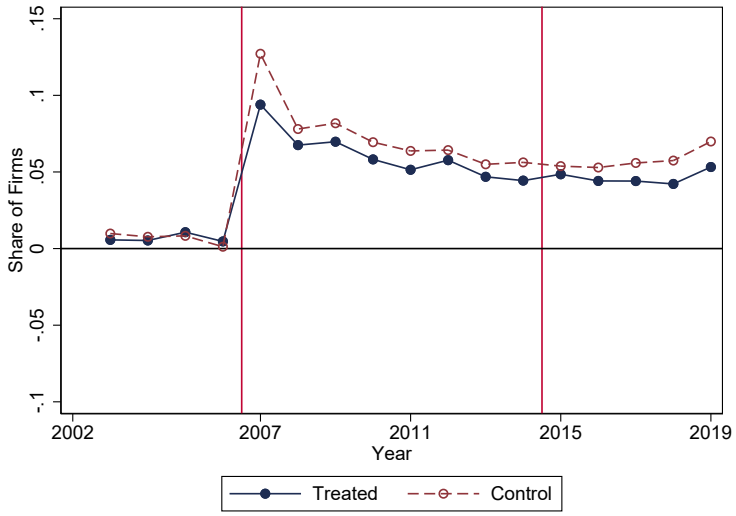
Notes: Panels (a) and (b) show the shares of treated and control firms, respectively, in each of the top five industries (4-digit NAICS) in manufacturing and non-manufacturing sectors. The firm sample consists of CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec with at least five employees in 2006. Treated and control firms are defined, respectively, as those with and without any investment in machinery or equipment for manufacturing goods in 2002 – 2006.

Figure A3: Firm Entry and Exit

(a) Share of Firms that Enter



(b) Share of Firms that Exit



Notes: Panels (a) and (b) show the number of firms that enter and exit our analysis sample each year, respectively, scaled by previous years' number of firms. The dark blue lines represent treated firms and the dashed red lines represent control firms. The firm sample consists of CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec with at least five employees in 2006. Treated and control firms are defined, respectively, as those with and without any investment in machinery or equipment for manufacturing goods in 2002 – 2006.

Table A1: Corporate Income Tax Rates (2002 – 2008)

Province	Types	2002	2003	2004	2005	2006	2007	2008
Federal	General (mfg.)	21	21	21	21	21	21	19.5
	General (non-mfg.)	25	23	21	21	21	21	19.5
	Small (mfg.)	12	12	12	12	12	12	11
	Small (non-mfg.)	12	12	12	12	12	12	11
Alberta	General (mfg.)	13	12.5	11.5	11.5	11.5	10	10
	General (non-mfg.)	13	12.5	11.5	11.5	11.5	10	10
	Small (mfg.)	4.5	4	3	3	3	3	3
	Small (non-mfg.)	4.5	4	3	3	3	3	3
British Columbia	General (mfg.)	13.5	13.5	13.5	13.5	12	12	11
	General (non-mfg.)	13.5	13.5	13.5	13.5	12	12	11
	Small (mfg.)	4.5	4.5	4.5	4.5	4.5	4.5	3.5
	Small (non-mfg.)	4.5	4.5	4.5	4.5	4.5	4.5	3.5
Ontario	General (mfg.)	11	11	12	12	12	12	12
	General (non-mfg.)	12.5	12.5	14	14	14	14	14
	Small (mfg.)	6	5.5	5.5	5.5	5.5	5.5	5.5
	Small (non-mfg.)	6	5.5	5.5	5.5	5.5	5.5	5.5
Quebec	General (mfg.)	9	9	8.9	8.9	8.9	9.9	11.4
	General (non-mfg.)	9	9	8.9	8.9	8.9	9.9	11.4
	Small (mfg.)	9	9	8.9	8.9	8.9	8	8
	Small (non-mfg.)	9	9	8.9	8.9	8.9	8	8

Table A2: Descriptive Statistics on Firms and Workers

	(1)	(2)	(3)	(4)
	Manufacturing		Non-manufacturing	
	Treated	Control	Treated	Control
A. Firms				
Tangible Assets ('000)	2844.2	580.4	1833.8	778.0
Investment Rate	0.109	0.111	0.120	0.123
Employment	41.4	15.4	35.7	18.1
Average Payrolls ('000)	135.7	45.2	97.4	50.4
Revenue ('000)	7964.2	2188.6	7414.3	3249.9
After-tax Profits ('000)	115.9	50.6	99.0	62.7
Profit Margins	0.038	0.029	0.034	0.039
Leverage Ratio	0.639	0.718	0.706	0.731
Retained Earnings / Assets	0.291	0.233	0.223	0.215
Firm Age	11.7	10.6	10.8	9.9
Number of Firms	10,760	7,835	4,720	133,495
B. Workers				
Annual Earnings ('000)	50.5	42.5	56.5	47.2
Male	0.747	0.696	0.623	0.590
Age	43.7	44.7	43.4	44.2
Number of Workers	303,480	43,295	97,195	748,440

Notes: Panels A and B report summary statistics for firms and workers in our analysis sample, respectively. Columns (1) – (2) report statistics for the manufacturing sector. Columns (3) – (4) report statistics for non-manufacturing sectors. The firm sample consists of CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec with at least five employees in 2006. Treated and control firms are defined, respectively, as those with and without any investment in machinery or equipment for manufacturing goods in 2002 – 2006. The worker sample consists of workers at the treated and control firms in 2002 – 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 in 2002 – 2006. Other services sector includes real estate, rental and leasing, management, administrative support, arts and entertainment, accommodation and food services.

Table A3: Pre-reform Investment Share by Asset Classes

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Manufacturing		Non-manufacturing	
	Treated	Control	Treated	Control	Treated	Control
Machinery and equipment	0.511	0.000	0.652	0.000	0.208	0.000
Computers and software	0.102	0.422	0.085	0.256	0.140	0.426
Clean technology	0.007	0.009	0.001	0.003	0.019	0.009
Buildings	0.141	0.150	0.104	0.132	0.220	0.151
Small tools	0.069	0.036	0.070	0.041	0.067	0.036
Property w/ leasehold interests	0.042	0.068	0.028	0.071	0.072	0.068
Patents and licences	0.007	0.007	0.004	0.022	0.015	0.007
Vehicles	0.004	0.097	0.002	0.005	0.009	0.099
Surface construction	0.016	0.015	0.007	0.004	0.034	0.015
Movable equipment	0.024	0.043	0.002	0.024	0.073	0.044
Miscellaneous	0.074	0.145	0.042	0.424	0.142	0.139

Notes: This table reports the average expenditure share by asset classes during 2002 – 2006. The firm sample consists of CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec with at least five employees in 2006. Columns (1) and (2) include all firms in our analysis sample. Columns (3) and (4) focus on firms in the manufacturing sector, and columns (5) and (6) focus on firms in non-manufacturing sectors. Treated and control firms are defined, respectively, as those with and without any investment in machinery or equipment for manufacturing goods from 2002 and 2006.

Table A4: Net Present Value of CCA and Cost of Capital for Buildings and Computers

	(1) Net Present Value: Buildings	(2) Cost of Capital: Buildings	(3) Net Present Value: Computers	(4) Cost of Capital: Computers
Post × Treated	0.0115***	-0.0037***	0.0067***	-0.0009***
	(0.0034)	(0.0005)	(0.0019)	(0.0001)
Mean Dep. Var.	0.334	0.331	0.747	0.291
Observations	152,320	152,320	993,355	993,355
Treated Firms	4,715	4,715	14,240	14,240
Control Firms	23,045	23,045	103,980	103,980
Adjusted R^2	0.400	0.570	0.348	0.406

Notes: Columns (1) and (2) report coefficient estimates on *Post* × *Treated* in equation (2) for firms' net present value of one dollar of capital cost allowance and the cost of capital, respectively, for investment in buildings. Columns (3) and (4) show the estimates for investment in computers and software. We calculate the net present value and the cost of capital using actual depreciation rates, which are firms' claims for capital cost allowance scaled by their capital stock at the end of the year, both reported in firms' T2 Schedule 8 by each asset class. The mean for each dependent variable is based on years 2002 – 2006. All specifications include firm fixed effects and industry by year fixed effects. Standard errors are clustered at the firm level and reported in parentheses.

B Robustness Checks and Internal Validity

In Appendix B, we provide results from robustness tests discussed in Sections 3 – 5.

B.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 B1 and Table B1 show that our key results are robust to controlling for sector by year fixed effects.

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 B2 and Table B2). Here, we cluster standard errors at the industry by commuting zone level. The results are qualitatively similar to our main findings.

In our main analysis sample, we include firms and workers in four major provinces in Canada: Alberta, British Columbia, Ontario, and Quebec, which account for 90 percent of all firms in Canada. Figure B3 and Table 3 show results including the rest of provinces and territories in Canada, which are qualitatively similar to our main results.

In our main analysis sample, we exclude firms in agriculture, finance, and real estate sectors. Figure B4 and Table B4 show qualitatively similar results when we include these excluded sectors.

Furthermore, Figure B5 and Table B5 show results separately for the manufacturing sector and for non-manufacturing sectors to check whether our results are primarily driven by firms and workers in the manufacturing sector alone. We find qualitatively similar results for both the manufacturing sector and non-manufacturing sectors, implying that our results are unlikely to be driven by a manufacturing-specific shock that may have coincided with the reform in 2007.

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. In Figure B6 and Table B6, 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.

B.2 Redefining Treated and Control Groups

In our main specification, we define treated firms as those with any positive investment in machinery or equipment prior to the reform, while control firms are defined as those that either rented, outsourced, or never bought any machinery or equipment. To address a potential concern that these treated firms and control firms may not be comparable, we redefine treated firms as those whose share of investment in machinery or equipment is at least 30 percent, while control firms are defined as those whose share of investment in machinery or equipment is below 30 percent, excluding firms with zero investment in machinery or equipment. In other words, these newly defined control firms also had positive investment in manufacturing assets, but just with a smaller share relative to the newly defined treated firms. Figure B7 and Table B7 show that the results based on this new definition are qualitatively similar to our main findings.

B.3 Matching Workers

In our main specification, we use all workers from treated and control firms who satisfy the restrictions mentioned in Section 3. Since treated firms are larger than control firms on average, workers from treated firms have slightly higher earnings relative to workers from control firms on average prior to the reform. We match workers from treated firms with workers from control firms based on their industry, gender, and age bins. After matching, workers from treated firms have slightly lower earnings relative to workers from control firms on average. Even if we match control workers to be in the same 4-digit industry, of the same gender, and in the same age bin, we still find qualitatively similar results to our main findings (Figure B8 and Table B8).

B.4 Changes in Within-firm Wage Inequality

In Section 5, we examine whether changes in relative demand for skilled workers lead to changes in within-firm wage inequality, comparing workers in the top 10 percentile with workers in the bottom 10 percentile of the within-firm wage distribution. Here, we repeat the same exercise for workers in the top 25 percentile and for workers in the bottom 25 percentile.

Panel (a) of Figure B9 shows the estimates on the log of the average earnings gap between workers at the top 25 percentile and those at the bottom 25 percentile in the within-firm earnings distribution, defined as $log(\frac{\text{Earnings}_{jt}^{p75}}{\text{Earnings}_{jt}^{p25}})$. Column (1) of Table B9 shows that the earnings gap increases by 0.6 log points. Panel (c) and (e) of Figure B10 show the estimates on log(annual earnings) separately for workers at the top 25 percentile and for workers at the bottom 25 percentile, respec-

tively. Columns (2) and (3) of Table B9 confirm that the increase in the earnings gap is largely driven by the increase in earnings of workers at the top 25 percentile.

Since this increase in the wage gap can be affected by changes in worker composition, Panel (b) of Figure B9 shows these estimates only for workers who continue to stay at their original firms after the reform, thereby holding the composition fixed. Column (4) of Table B9 shows that the earnings gap increases by 4.8 log points, which are even larger than the previous estimate. Panel (d) and (f) of Figure B9 show the estimates on log(annual earnings) separately for workers at the top 10 percentile and for workers at the bottom 25 percentile, respectively, among stayers. In contrast to the previous result, Columns (5) and (6) show that the increase in the earnings gap among stayers is driven by both the increase in earnings of stayers at the top 25 percentile and the decrease in earnings of stayers at the bottom 25 percentile. Overall, our results on changes in the within-firm earnings inequality using workers at the top and the bottom 25 percentile are similar to those in Figure B9 and Table B9 using workers at the top 10 percentile and those at the bottom 10 percentile.

B.5 Worker Reallocation

We examine whether there was reallocation of workers across firms after the reform, and how much this allocation can explain our results on employment. This analysis implicitly assumes that treated firms and control firms compete in the input market for workers. If most of the responses in treated firms' employment were driven by workers moving from control firms to treated firms, then not only our estimates on employment are biased upward, but our results would have a different welfare implication on the labor market.

Besides workers already employed at treated firms, every worker that gets hired by a treated firm after the reform must come from either (1) control firms, (2) out-of-sample firms (e.g., in other provinces or in excluded sectors), or (3) non-employment (unemployed, fresh graduates, or new immigrants). In Figure B10, Panels (a) and (b) show the rate of gross outflow and net outflow of workers from treated firms to each of the three groups to treated firms. All estimates are scaled by the total number of workers in the origin group in each previous year.

Panel (a) of Figure B10 shows that the gross outflow of workers from treated firms to different types of firms moved in parallel before the reform, and remained relatively flat after the reform. In other words, there was not much reallocation of workers from treated firms to control firms or other treated firms (or vice versa) after the reform.

In addition, Panel (b) of Figure B10 shows that the net outflow of workers from treated firms to out-of-sample firms followed a flat trend before the reform, increased after the reform for the first

few years, and returned to its original level afterwards, suggesting that there was a small increase in the share of workers at treated firms who moved to out-of-sample firms after the reform. However, we observe a similar pattern for workers at control firms (Panel (d)), suggesting that the reallocation of workers from treated firms to out-of-sample firms remained flat relative to workers at control firms.

Finally, the net inflow of workers from treated firms to unemployment followed a flat trend before the reform, increased after the reform for the first few years, and returned to its original level afterwards, suggesting that there was a small increase in the share of workers at treated firms who became unemployed after the reform. However, we observe a similar pattern for workers at control firms (Panel (d)), suggesting that the share of workers from treated firms who became unemployed remained flat relative to workers at control firms, consistent with the results from Figure 6 and Table 6.

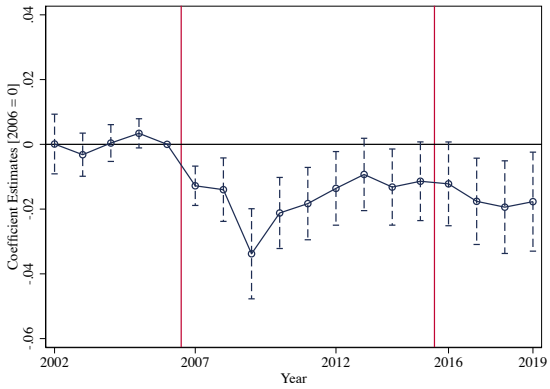
In summary, there was not much reallocation of workers from treated firms to control firms (or vice versa). If anything, there was a small increase in net outflow of workers from treated firms to out-of-sample firms after the reform.

B.6 Changes in Price of Capital

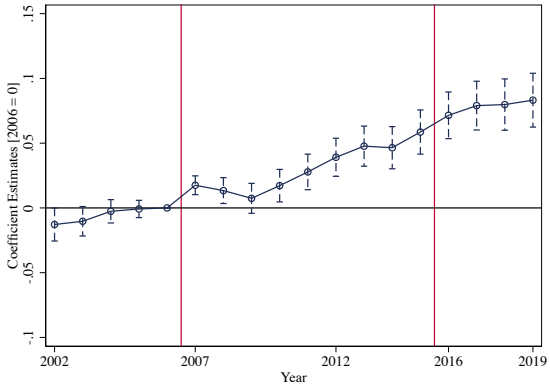
Panel (a) of Figure B11 shows the industrial producer price index (IPPI) for the machinery manufacturing industry (NAICS 333) did not change much after the reform relative to the IPPI for other industries in the manufacturing sector. Panel (b) of Figure B11 shows that the purchase price index of machinery and equipment for the manufacturing sector did not change much after the reform relative to the one for non-manufacturing sectors. These results provide suggestive evidence that the price of capital for machinery and equipment did not change much relative to the price of other types of assets after the reform, implying that the rental price for machinery and equipment likely remained flat after the accelerated depreciation policy.

Figure B1: Controlling for Sector by Year Fixed Effects

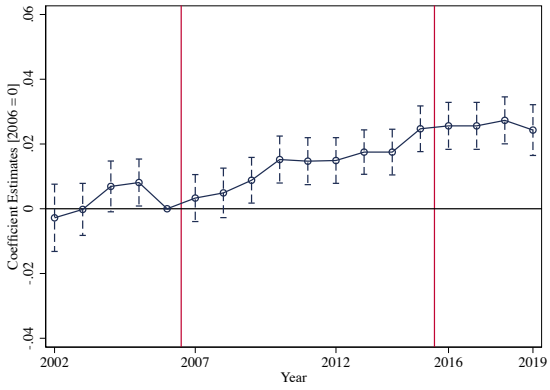
(a) log(Annual Earnings)



(b) log(Employment)



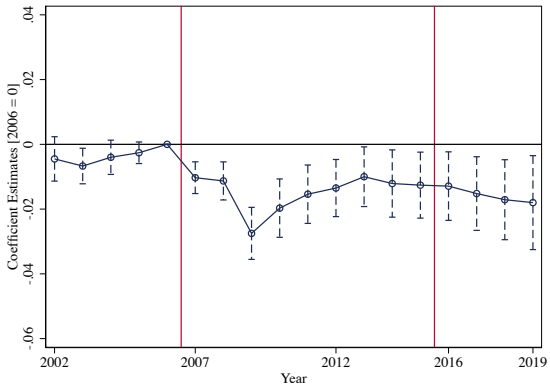
(c) Investment Rate



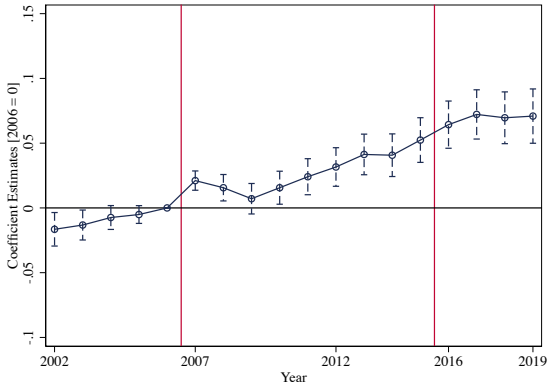
Notes: Panels (a) shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (3) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are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

Figure B2: Controlling for Commuting Zone by Year Fixed Effects

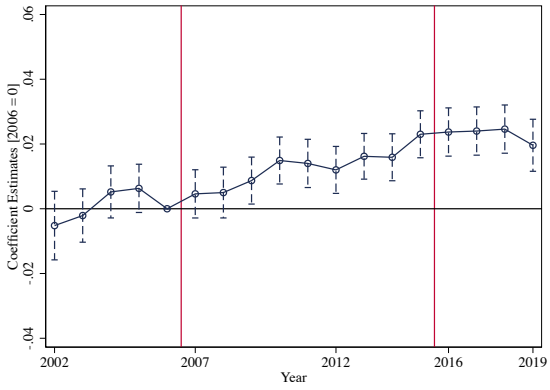
(a) log(Annual Earnings)



(b) log(Employment)



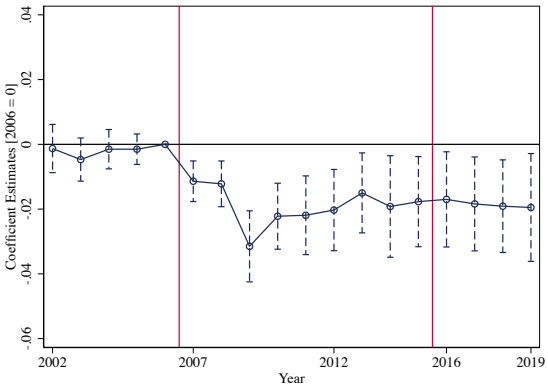
(c) Investment Rate



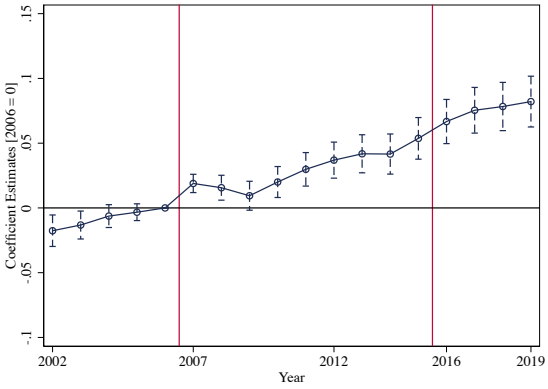
Notes: Panels (a) shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (3) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are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

Figure B3: Including All Provinces

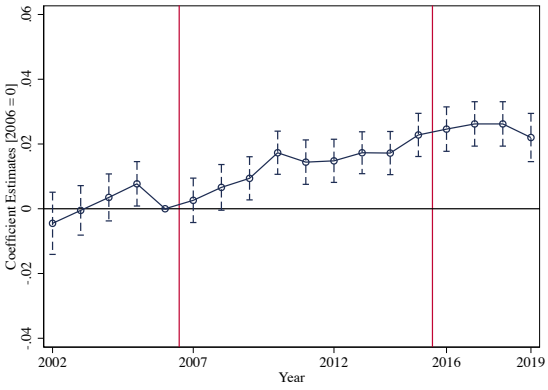
(a) log(Annual Earnings)



(b) log(Employment)



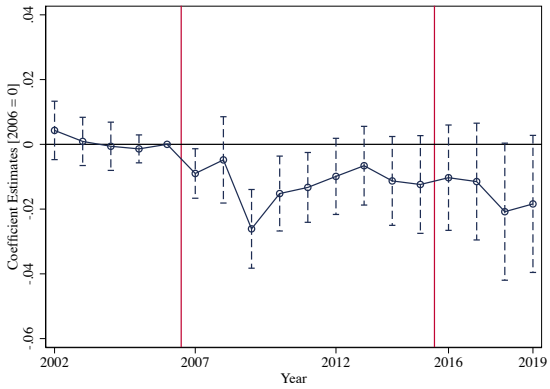
(c) Investment Rate



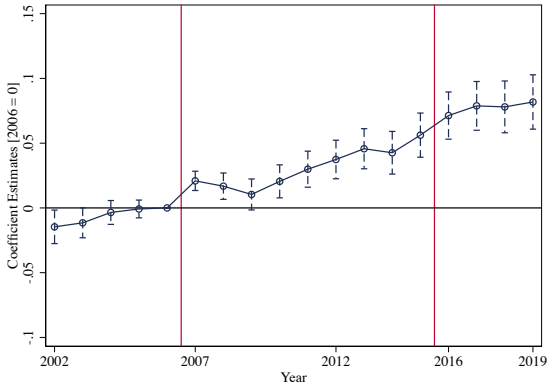
Notes: Panels (a) shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (3) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are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

Figure B4: Including All Sectors

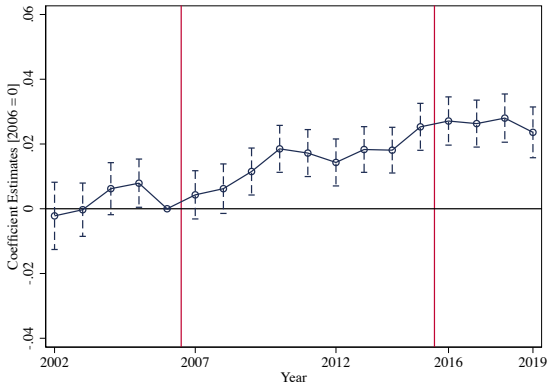
(a) log(Annual Earnings)



(b) log(Employment)



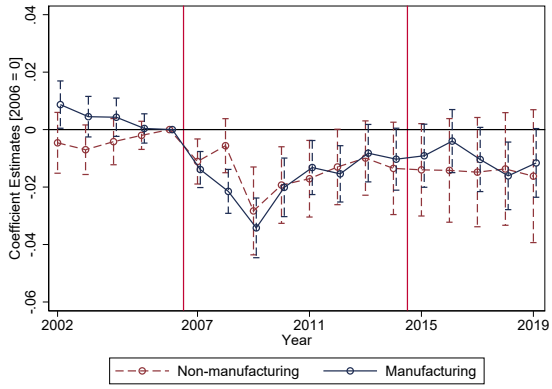
(c) Investment Rate



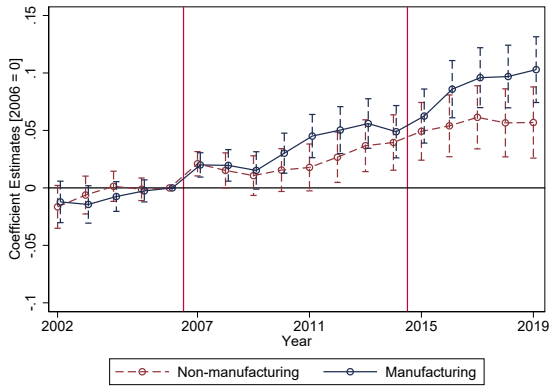
Notes: PPanels (a) shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (3) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are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

Figure B5: Manufacturing versus Non-manufacturing Sectors

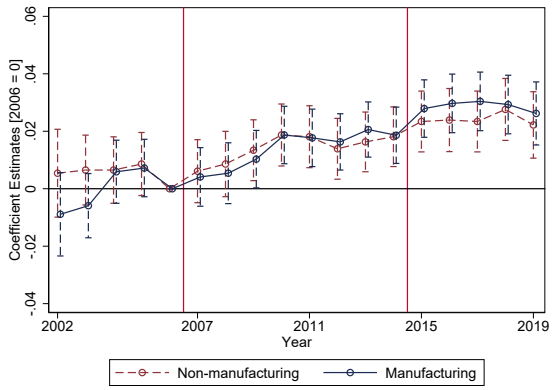
(a) log(Annual Earnings)



(b) log(Employment)



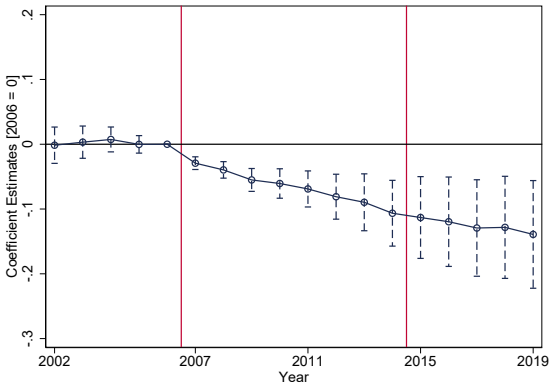
(c) Investment Rate



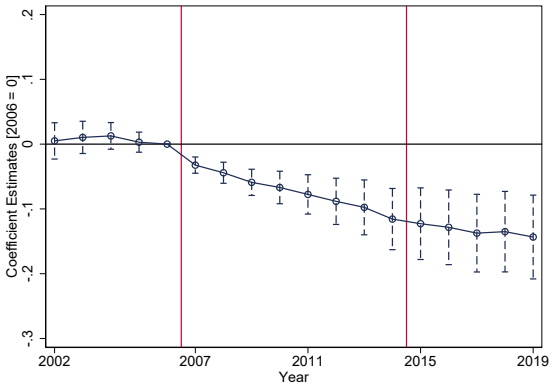
Notes: Panels (a) shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (3) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are 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 lines indicate the reform years. The dashed lines represent 95 percent confidence intervals. The dark navy line indicates the estimates for the manufacturing sector, and the red line indicates the estimates for non-manufacturing sectors.

Figure B6: Include Part-timers, Moonlighters, and Dropping the Tenure Restriction

(a) log(Annual Earnings)



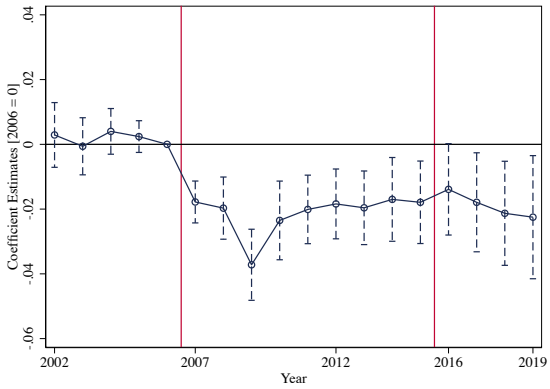
(b) log(Annual Earnings): Movers



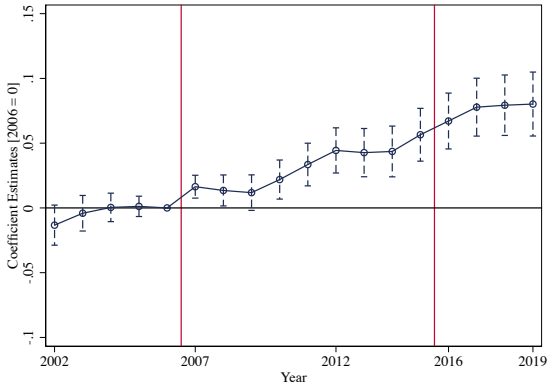
Notes: The figure shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are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

Figure B7: Alternative Definition of Treated Firms/Workers

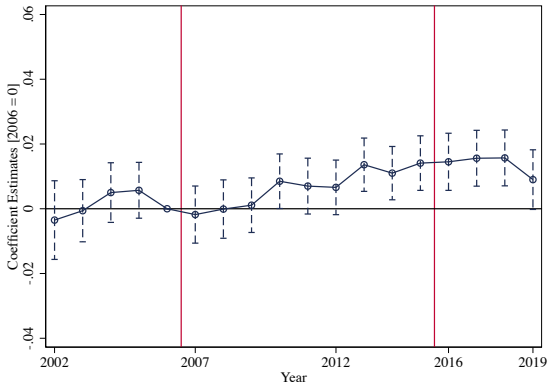
(a) log(Annual Earnings)



(b) log(Employment)



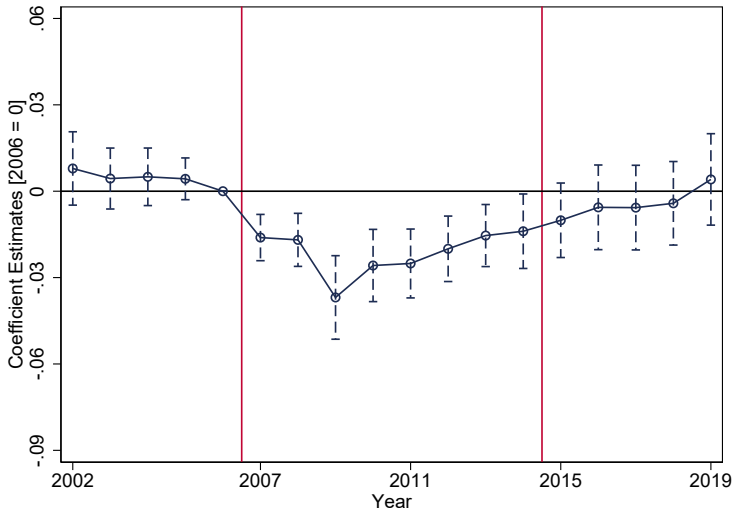
(c) Investment Rate



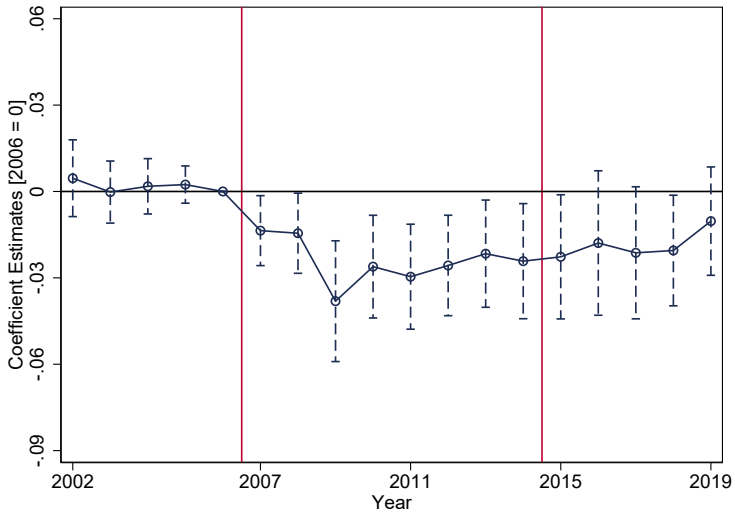
Notes: Panels (a) shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times$ *Treated* in equation (3) for workers' log(annual earnings). Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are 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 lines indicate the reform years. The dashed lines represent 95 percent confidence intervals. A firm (and its workers) is defined as treated if its investment in manufacturing assets exceeds 30 percent of its total investment in any year prior to the reform.

Figure B8: Matched Worker Sample

(a) log(Annual Earnings)

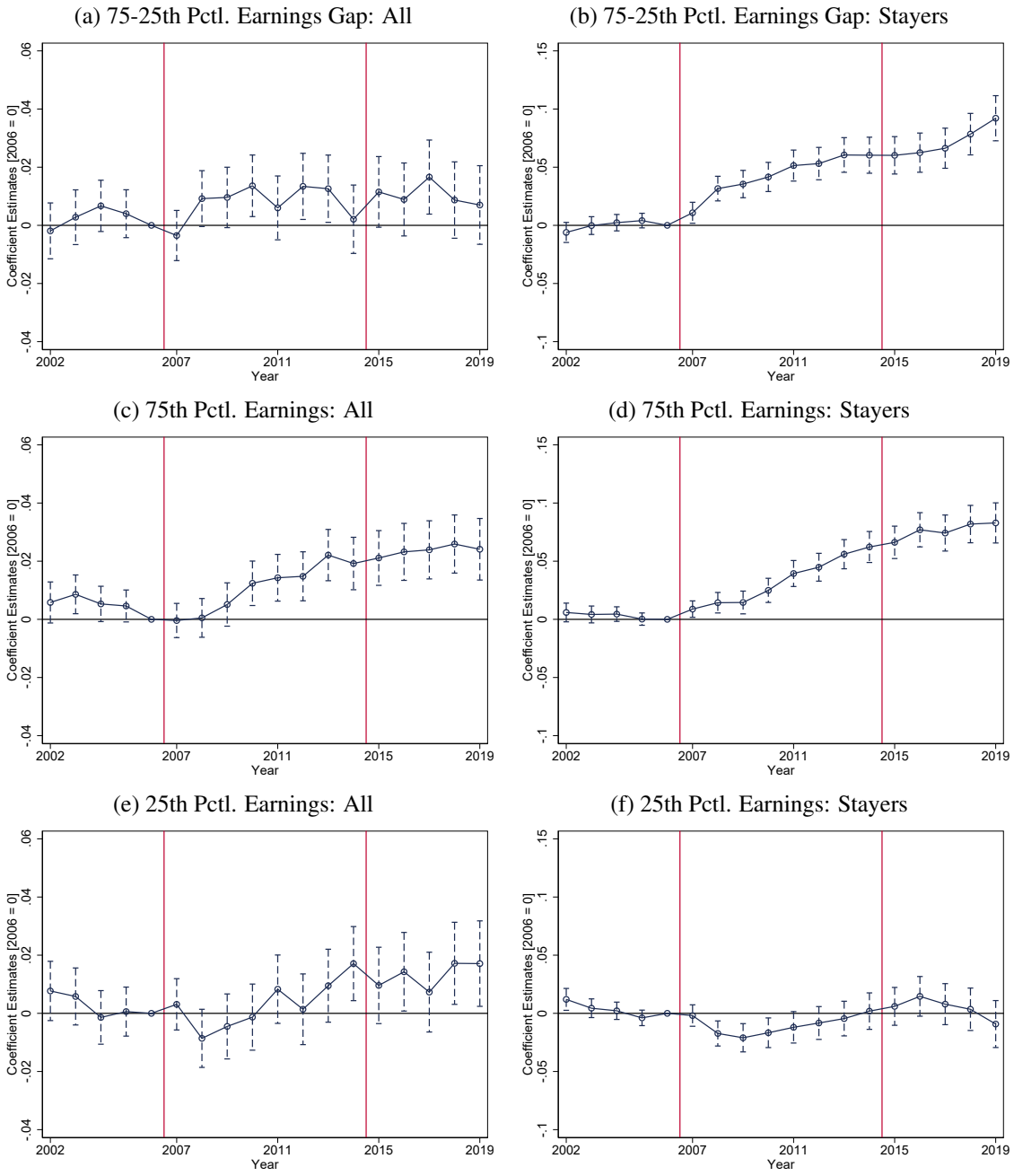


(b) log(Annual Earnings): Movers



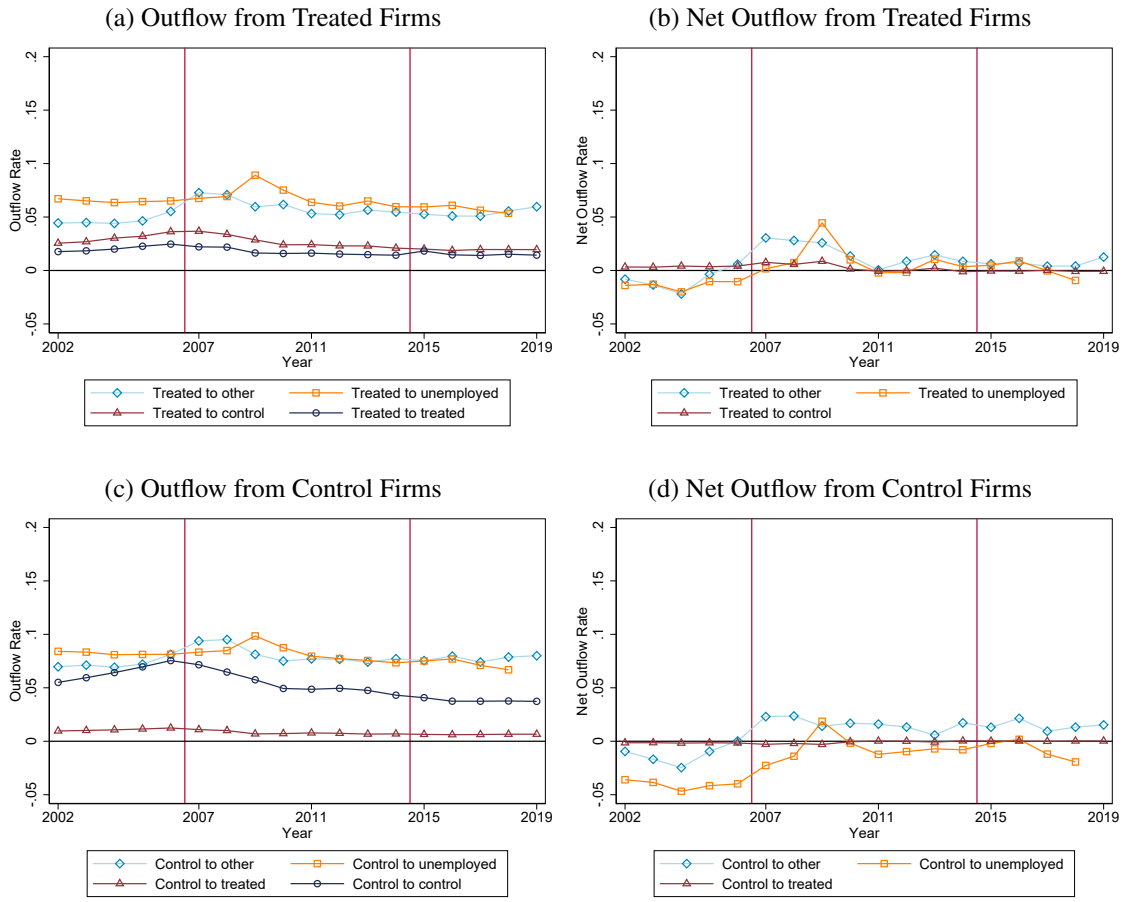
Notes: Panels (a) – (b) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for log(annual earnings) of all workers and of workers who leave their firms after 2007. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. The sample is obtained using exact matching by industry and nearest-neighbor propensity score matching by gender and age polynomials. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure B9: Within-firm Earnings Inequality



Notes: Panels (a), (c), and (e) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (1) for the 75th-25th percentile gap, the 75th percentile, and the 25th percentile, respectively, of workers' log(annual earnings) within each firm. The 75th-25th percentile gap is defined as $\log(\text{earnings})_{jt}^{p75}$ - $\log(\text{earnings})_{jt}^{p25}$. Panels (b), (d), and (f) show the estimates using workers who stay at their pre-reform employers after 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. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

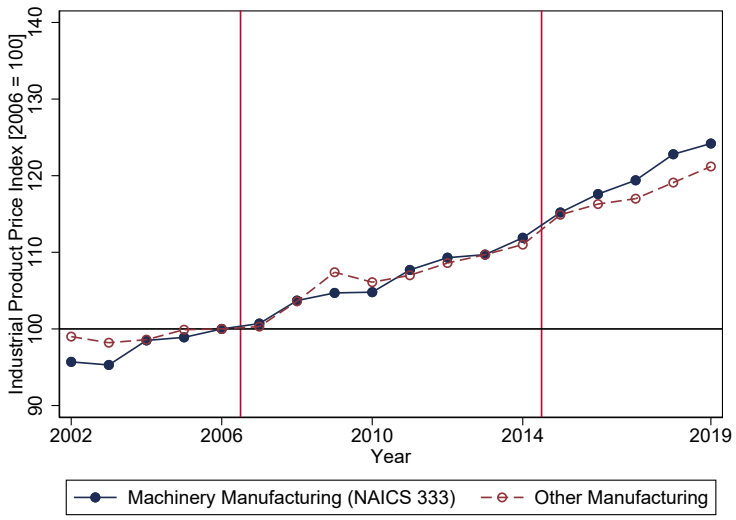
Figure B10: Worker Reallocation



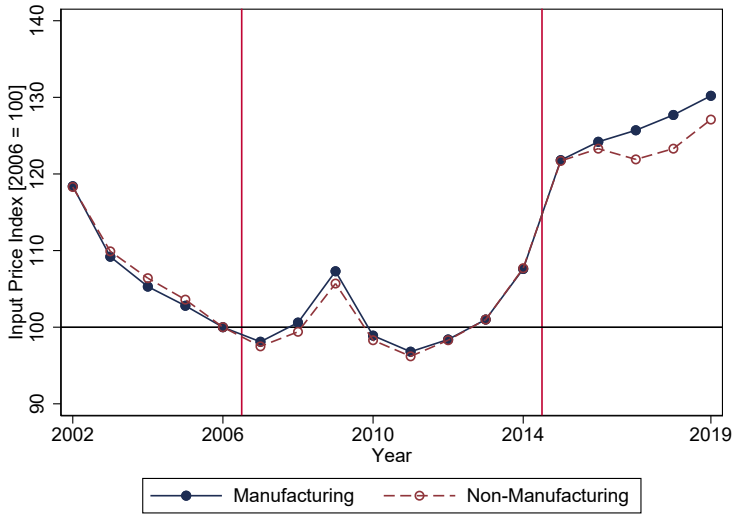
Notes: Panel (a) shows the outflow rates of workers from treated firms to (i) other treated firms, (ii) control firms, (iii) firms excluded from the sample, and (iv) unemployment by year during our sample period. Outflow rates are the number of workers moving from treated firms to destination firms, scaled by previous year's total number of workers in treated firms. Panel (b) shows corresponding net outflow rates, i.e., the number of workers moving from treated firms to destination firms net of those moving from destination firms to treated firms, scaled by previous year's total number of workers in treated firms. Panels (c) – (d) repeat panels (a) – (b) for outflow and net outflow rates from control firms. Treated and control firms are CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec, with at least five employees in 2006, and not in agriculture, finance, or real estate sectors. They are defined, respectively, as those with and without any investment in machinery or equipment for manufacturing goods in 2002 – 2006.

Figure B11: Changes in Price of Capital

(a) Industrial Producer Price Index



(b) Purchase Price Index of Machinery and Equipment



Notes: In Panel (a), the dark blue line shows the industrial producer price index (IPPI) for the machinery manufacturing industry (NAICS 333) and the red line indicates the IPPI for other manufacturing industries. In Panel (b), the dark blue line shows the purchase price index of machinery and equipment for the manufacturing sector (NAICS 31-33) and the red line shows that for non-manufacturing sectors. Indices in 2006 are normalized to 100.

Table B1: Controlling for Sector by Year Fixed Effects

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
Post × Treated	-0.0169***	0.0429***	0.0124***
	(0.0055)	(0.0062)	(0.0020)
Mean Dep. Var.	52.0	39.7	0.112
Observations	17,809,335	1,956,395	1,840,290
Treated Workers/Firms	400,675	15,480	14,930
Control Workers/Firms	791,735	141,330	126,120
Adjusted R^2	0.698	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 and measured in thousand CAD in column (1). 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 B2: Controlling for Commuting Zone by Year Fixed Effects

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
Post × Treated	-0.0115***	0.0424***	0.0129***
	(0.0037)	(0.0062)	(0.0020)
Mean Dep. Var.	52.0	39.7	0.112
Observations	17,809,335	1,956,395	1,840,290
Treated Workers/Firms	400,675	15,480	14,930
Control Workers/Firms	791,735	141,330	126,120
Adjusted R^2	0.702	0.822	0.145

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 and measured in thousand CAD in column (1). Column (1) includes worker fixed effects, industry by year fixed effects, and commuting zone by year fixed effects. Columns (2) and (3) include firm fixed effects, industry by year 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 B3: Including All Provinces

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
Post × Treated	-0.0169***	0.0448***	0.0135***
	(0.0045)	(0.0058)	(0.0019)
Mean Dep. Var.	50.9	40.2	0.111
Observations	20,011,455	2,292,265	2,153,130
Treated Workers/Firms	443,215	17,385	16,765
Control Workers/Firms	893,810	168,385	149,940
Adjusted R^2	0.705	0.823	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 and measured in thousand CAD in column (1). 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 B4: Including All Sectors

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
Post × Treated	-0.0134**	0.0442***	0.0137***
	(0.0057)	(0.0062)	(0.0020)
Mean Dep. Var.	52.7	40.2	0.112
Observations	20,145,645	2,099,520	1,970,310
Treated Workers/Firms	424,310	15,655	15,095
Control Workers/Firms	922,885	154,840	137,965
Adjusted R^2	0.708	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 sectors. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD in column (1). 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 B5: Manufacturing Sector and Non-manufacturing Sectors (Separately)

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
Manufacturing			
Post × Treated	-0.0189***	0.0551***	0.0173***
	(0.0041)	(0.0085)	(0.0028)
Mean Dep. Var.	50.5	41.4	0.109
Observations	5,181,035	248,015	239,000
Treated Workers/Firms	303,480	10,760	10,395
Control Workers/Firms	43,295	7,835	7,090
Adjusted R^2	0.658	0.873	0.126
	(4) log(Annual Earnings)	(5) log(Employment)	(6) Investment Rate
Non-manufacturing			
Post × Treated	-0.0111*	0.0345***	0.0108***
	(0.0057)	(0.0090)	(0.0029)
Mean Dep. Var.	56.5	35.7	0.120
Observations	12,628,300	1,708,380	1,601,290
Treated Workers/Firms	97,195	4,720	4,535
Control Workers/Firms	748,445	133,495	119,025
Adjusted R^2	0.714	0.809	0.146

Notes: Columns (1) and (4) report 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. The specification includes 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. Columns (2) – (3) and (5) – (6) 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 the manufacturing sector (columns (1) – (3)) and in non-manufacturing sectors (columns (4) – (6)), separately. The specification includes firm fixed effects and industry by year fixed effects. Standard errors are clustered at the firm level and reported in parentheses. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD in columns (1) and (4).

Table B6: Include Part-timers, Moonlighters, and Dropping the Tenure Restriction

	(1) log(Annual Earnings)	(2) log(Annual Earnings): Movers
Post × Treated	-0.0858***	-0.0980***
	(0.0209)	(0.0201)
Mean Dep. Var.	38.8	35.9
Observations	66,830,215	52,053,900
Treated Workers	1,113,390	788,985
Control Workers	3,418,610	2,631,585
Adjusted R^2	0.533	0.505

Notes: The table 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 included. The mean for the dependent variable is based on years 2002 – 2006 and measured in thousand CAD. The specification includes 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 B7: Alternative Definition of Treated Firms/Workers

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
Post × Treated	-0.0226***	0.0414***	0.0058**
	(0.0049)	(0.0074)	(0.0023)
Mean Dep. Var.	52.9	41.6	0.113
Observations	16,586,335	1,817,545	1,714,270
Treated Workers/Firms	305,535	10,010	9,605
Control Workers/Firms	804,330	133,160	119,590
Adjusted R^2	0.701	0.823	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. A firm (and its workers) is defined as treated if its investment in manufacturing asssets exceeds 30 percent of its total investment in any year prior to the reform. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD in column (1). 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 B8: Matched Worker Sample

(a) Summary Statistics

	(1) Treated	(2) Control
Annual Earnings ('000)	47.2	48.7
Male	0.429	0.640
Age	44.9	44.2
Sectors:		
Utility/Mining	0.103	0.103
Construction	0.052	0.052
Manufacturing	0.352	0.352
Wholesale Trade	0.201	0.201
Retail Trade	0.147	0.147
Transportation	0.008	0.008
Professional Services	0.044	0.044
Education/Healthcare	0.018	0.018
Other Services	0.075	0.075
Number of Workers	122,680	122,680

(b) Difference-in-Differences Estimates on Earnings

	(1) log(Annual Earnings)	(2) log(Annual Earnings): Movers
Post × Treated	-0.0208***	-0.0239***
	(0.0046)	(0.0066)
Mean Dep. Var.	47.2	46.4
Observations	3,589,425	2,020,115
Treated Workers	122,680	61,950
Control Workers	122,680	66,060
Adjusted R^2	0.716	0.669

Notes: Panel (a) reports summary statistics of the matched worker sample. The sample is obtained using exact matching by industry and nearest-neighbor propensity score matching by gender and age polynomials. 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 (b), columns (1) and (2) show coefficient estimates on *Post* × *Treated* in equation (4) for log(annual earnings) of all workers and of workers who leave their firms after 2007. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD. The specification includes 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 B9: Changes in Within-firm Earnings Inequality

	(1) 75-25th Percentile Gap log(Annual Earnings)	(2) 75th Percentile log(Annual Earnings)	(3) 25th Percentile log(Annual Earnings)
All Workers			
Post × Treated	0.0058*	0.0084***	0.0026
	(0.0030)	(0.0027)	(0.0034)
Mean Dep. Var.	0.722	42.0	21.3
Observations	1,924,090	1,924,090	1,924,090
Treated Workers	15,470	15,470	15,470
Control Workers	140,425	140,425	140,425
Adjusted R^2	0.344	0.798	0.700
	(4) 75-25th Percentile Gap log(Annual Earnings)	(5) 75th Percentile log(Annual Earnings)	(6) 25th Percentile log(Annual Earnings)
Stayers			
Post × Treated	0.0477***	0.0385***	-0.0092**
	(0.0044)	(0.0042)	(0.0043)
Mean Dep. Var.	0.520	52.4	30.8
Observations	1,431,635	1,431,635	1,431,635
Treated Workers	12,795	12,795	12,795
Control Workers	98,770	98,770	98,770
Adjusted R^2	0.484	0.766	0.675

Notes: Columns (1) – (3) show coefficient estimates on *Post* × *Treated* in equation (2) for the 75th-25th percentile gap, the 75th percentile, and the 25th percentile, respectively, of workers' log(annual earnings) within each firm. The 75th-25th percentile gap is defined as $\log(\text{earnings})_{jt}^{p75}$ - $\log(\text{earnings})_{jt}^{p25}$. Columns (4) – (6) show the estimates using workers who stay at their pre-reform employers after 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. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD for columns (2), (3), (5), and (6). All specifications include firm fixed effects and industry by year fixed effects. Standard errors are clustered at the firm level and reported in parentheses.