Manufacturing Investment and Employee Earnings: Evidence from Accelerated Depreciation*

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Abstract

This paper assesses the effects of a tax policy designed to induce manufacturing investment on employee earnings. Following the accelerated depreciation policy in Canada, which lowers the costs of acquiring machinery or equipment, treated firms increase manufacturing investment and employment. However, earnings of incumbent workers at treated firms decrease on average, primarily driven by displaced or low fixed effects workers. Furthermore, both the skill ratio and skill premium increase within firm, providing evidence of skill-biased technological change. Our findings suggest that a tax policy promoting manufacturing investment may contribute to increases in wage inequality both within firm and across firms.

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1 Introduction

A tax policy is a popular tool to promote capital investment in the economy. 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 assess 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 or across-state 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; Tuzel and Zhang, 2021; Park, 2022; 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 reduced the cost of capital for machinery and equipment by accelerating their depreciation schedule. To assess the policy impacts, we compare firms that had any positive manufacturing investment with firms that never invested in those assets prior to the reform. 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 manufacturing industries, there are firms in non-manufacturing industries that invest in machinery or equipment. Furthermore, we

[1] While there are several 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 (2024), there is currently no paper that examines how an accelerated depreciation policy affects wages using employee-level data.

have firms within a manufacturing industry that do not buy machinery or equipment, but just rent it or outsource. This allows us to additionally exploit within-industry variation.

After the accelerated depreciation policy, firms that invested in machinery or equipment prior to the reform increase their investment in manufacturing assets, while firms without any manufacturing investment do not respond to the reform. On average, treated firms' investment rate on eligible assets increases 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 and equipment) increases by 13 percent on average, relative to control firms. Subsequently, treated firms increase employment and average payrolls by 4.5 percent and 2.2 percent, respectively.

To assess the policy impacts on worker-level outcomes, we compare the annual earnings of workers at treated firms with those of workers at control firms before and after the reform. Surprisingly, incumbent workers at treated firms experience a decline in their annual earnings by 1.4 percent on average. Since we find increases in both employment and average payrolls, the decline in incumbent workers' earnings is not explained by treated firms replacing workers with capital. The fact that treated firms raised average salaries, without increasing existing workers' average wages, also suggests that new hires are paid higher wages on average.

To see whether the changes in incumbent workers' earnings are connected with changes in firm-level characteristics, we estimate the policy impacts on other firm outcomes. 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 employment result in higher profitability. Taken together, these results suggest that the decline in incumbent workers' earnings is not driven by treated firms losing profits after the reform in the form of rent sharing.

Additionally, while we find that the annual earnings of workers who continue to stay at treated firms do not change much, 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 job transitions of treated workers relative to control workers. In other words, while workers at treated firms are equally likely to leave their firms 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.

We hypothesize that changes in incumbent workers' earnings are driven by the change in relative demand for high-skilled workers who complement the rise in capital investment better. Following the framework by Lindner et al. (2022), we illustrate a model predicting that the increased

capital investment following the accelerated depreciation policy can lead to increases in both the skill ratio (the employment share of high-skilled workers relative to low-skilled workers) and the skill premium (the earnings gap between high-skilled workers and low-skilled workers) at the firm level, assuming imperfect competition in the labor market. These results provide evidence of skill-biased technological change at the firm level. We find that both the skill ratio and the skill premium within each firm increase significantly after the reform. These results are consistent with the channel that the relative demand for high-skilled workers increases among treated firms in order to complement their increased capital investment.

To test whether changes in the earnings of job movers from treated firms are also consistent with the change in relative demand for high-skilled workers, we first examine whether these workers become unemployed or move to firms in different industries after leaving their firm. We find that job movers from treated firms are not more likely to become unemployed relative to job movers from control firms, and the share of workers moving to different industries after the reform does not differ much between treated and control groups. Therefore, unemployment or switching occupations (via switching industries) does not seem to be the main explanation behind the changes in job movers' earnings.

However, changes in the characteristics of new employers play an important role in explaining changes in job movers' earnings. In particular, most of these new employers are either treated firms or those outside of our analysis sample. On average, workers who leave treated firms after the reform move to new firms with 2.3 log points lower firm-specific wage premiums and to firms that are less likely to invest in machinery or equipment. Therefore, the reduced earnings of job movers can be explained by treated workers moving to firms with lower wage premiums relative to job movers from control firms after the reform. In other words, even if the reform does not reduce overall employment in our setting, it may lead them 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 job movers' earnings.

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 move voluntarily do not experience much changes in their earnings, while those displaced from treated firms experience large declines in earnings and firm-specific wage premiums. Furthermore, we find that workers at the lower quartile in the worker fixed effects distribution experience larger decreases in their earnings and move to firms with lower wage premiums, compared to workers at the top quartile. These results are consistent

with the idea that workers who are displaced or with low skills may face larger search frictions to find a new firm that would pay them a comparable wage when the relative demand for high-skilled workers increases to complement the rise in capital investment (Krusell et al. 2000).

We conduct several robustness tests to strengthen the internal validity of our results. Using within-industry variation ensures that treated and control firms share similar production technologies and absorbs any time-varying industry-specific shock. To address concerns that treated and control firms are different in terms of sizes and may be exposed to differential shocks, we show that our results remain qualitatively similar to matching treated firms with control firms based on their industries and sizes. Furthermore, our main results are robust to redefining treated firms and control firms based on their pre-reform share of manufacturing assets, thereby minimizing the concern that firms without any manufacturing investment might be exposed to differential shocks. We additionally find that the industrial producer price index for the manufacturing industry that produces machines or equipment remained stable relative to the average producer price index for the other manufacturing industries, suggesting that the price for capital did not change much in aggregate after the reform. This result addresses the concern that firms that rent machinery or equipment may be indirectly affected by the reform due to changes in the price of capital.

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 earlier studies exploit across-industry or across-state variation in bonus depreciation benefits to assess the effects on firm-level average payrolls (Curtis et al., 2022; Tuzel and Zhang, 2021), to the best of our knowledge, our paper is the first to assess the policy impacts on worker outcomes using employee-level data. Relative to Curtis et al. (2022) and Tuzel and Zhang (2021), 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 and track 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 reform affects reallocation of workers across different firms and sectors, so we can conduct a comprehensive analysis on how the policy 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 mechanisms behind changes in worker earnings. We find that firms affected by the accelerated depreciation policy significantly increased investment in both labor and capital, consistent with findings from prior studies. This increased investment subsequently leads to higher after-tax profits, so we can rule out decreases in firms' profitability as a potential channel behind the decline

in incumbent workers' earnings. Leveraging detailed firm balance sheet data, we show that the decline in earnings of job movers, particularly those displaced or with low worker fixed effects, is instead driven by treated firms increasing demand for high-skilled workers, consistent with the establishment-level results from Tuzel and Zhang (2021).

Second, our paper contributes to the long-standing literature that studies the connection between the rise in wage inequality and skill-biased technological change (Katz and Murphy, 1992; Juhn et al., 1993; Autor et al., 2003; Acemoglu and Autor, 2011). While prior studies infer the skill-biased technological change from changes in the skill ratio and premium at the aggregate level, we follow a similar approach by Lindner et al. (2022) to show that firm-level technological changes can lead to increases in both the skill ratio and skill premium at the firm level if the labor market is not perfectly competitive. While Lindner et al. (2022) shows that innovation shocks can drive firm-level technological changes, we show that the rise in capital induced by accelerated depreciation can lead to a similar change in our setting, and contribute to increases in wage inequality both within firm and across firms.

Third, our paper contributes to the research on how job transitions impact wages. A large literature studies the role of firm-specific components to explain the earnings losses of displaced workers (Lachowska et al., 2020; Schmieder et al., 2023). In this paper, we find that the accelerated depreciation policy does not lead to differential job displacement for workers at treated firms relative to workers at control firms on average. However, workers who get displaced from treated firms suffer from particularly large earnings declines, possibly because treated firms replace these workers with more skilled ones who can operate new machines or equipment. Furthermore, low-skilled workers are more likely to get displaced from treated firms and would suffer larger declines in earnings and firm-specific wage premiums when moving to other firms after the reform. Therefore, our paper shows there exists substantial heterogeneity in the earnings effects for displaced workers depending on their skills.

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 documents 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, 2024) and to payout taxes (Poterba and Summers, 1983; Moon, 2022), and large innovation responses to personal income taxes (Akcigit et al., 2022) and corporate income taxes (Lichter et al., 2021).

This paper is organized as follows. Section 2 provides institutional details on the capital cost allowance in Canada. Section 3 describes our empirical design and Section 4 shows our main results. Section 5 explores potential mechanisms behind our findings. Section 6 concludes.

2 Institutional Background

This section describes the institutional details regarding the capital cost allowance (CCA hereinafter) and the reform that accelerated the CCA schedule for a particular asset class relevant for our empirical design.

2.1 Tax Policy on Capital Cost Allowance

In general, firms that invest in capital cannot deduct the full capital costs from their taxable income immediately in the year of purchase. In Canada, the CCA system determines the amount of capital costs a firm may deduct each year. Analogous to the Modified Accelerated Cost Recovery System (MACRS) in the United States, the CCA system specifies the tax deduction schedule for each type of qualified capital. Under the system, firms claim tax deductions (CCA) based on the declining balance method and the half-year rule. The rates at which long-term assets are depreciated are set by the Canada Revenue Agency rules. For example, the CCA rate for asset Class 1 (buildings) is 4 percent, and the CCA rate for asset Class 43 (machinery or equipment for manufacturing goods or services) 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:

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

where d_c is the CCA rate for Class c and r is the discount rate. For example, with $r = 0.07$, the net present value of one dollar of CCA for buildings is $z_1 = 0.376$, and that for machinery and equipment is $z_{43} = 0.839$. The average net present value of CCA for each firm depends on its composition of investment across different asset classes. Therefore, changing the depreciation schedule for a particular asset class has different impacts across different firms, depending on the firms' reliance on that particular asset class. For instance, firms that primarily invest in machinery or equipment would benefit more from an increase in the CCA rate for machinery and equipment relative to firms that do not invest in machinery or equipment.[3]

[2]The most current information on statutory CCA rates can be found here.

[3]Moreover, firms across industries have different mixes of asset classes. For example, firms in the retail sector have a larger share of assets that depreciate slowly (e.g., buildings), whereas firms in the information technology sector have a larger share of assets that depreciate more quickly (e.g., computers). Thus, changing depreciation schedules across different asset classes has different impacts on the net present value of CCA across industries. In the United States, bonus depreciation policies yield greater changes in the net present value of deductions for industries with a larger share of long-duration assets (House and Shapiro, 2008; Zwick and Mahon, 2017; Curtis et al., 2022).

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 the capital cost deductions, ACCA allows firms to defer taxation and improves the financial returns from investment in particular assets. ACCA is analogous to the bonus depreciation policy in the United States, which accelerates the depreciation schedule by allowing firms to deduct a larger amount in the first year.

In 2007, the federal government of Canada proposed to re-target ACCA to reflect new economic challenges and provide tax incentives for investment in machinery or equipment. Specifically, the CCA rate for assets in Class 43 is accelerated to a 50 percent straight-line rate. Taking into account the half-year rule, assets in Class 43 can now be written off over a two-year period, starting at the mid-point of the acquiring year and ending at the mid-point of the second year after the acquisition. This results in an effective depreciation rate of up to 25 percent in the first calendar year, up to 75 percent in the second year (less any deduction claimed for the previous year), and up to 100 percent in the third and subsequent years (less any deductions claimed for previous years). This accelerated rate applies to eligible machinery or equipment purchased on or after 2007, and such new investment will be categorized under a new asset Class 29.

Assuming that firms claim the full eligible CCA each year, the net present value of one dollar of CCA is now

$$z_{29} = 0.5 \times 0.5 + \frac{1 - 0.5 \times 0.5}{1 + r} = \frac{1}{4} + \frac{0.75}{1.07} = 0.951,$$

which is about 11 percentage points (13 percent) higher than z_{43}. The federal government extended 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. Investment in machinery or equipment that would otherwise be included in Class 29 will now be categorized under a new asset Class 53. Based on the declining balance method, the net present value of one dollar of CCA becomes $z_{53} = 0.908$, which is still seven percentage points (eight percent) higher than the original z_{43} before 2007.

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

$$R = \frac{(1 - \tau z)r}{1 - \tau}, \tag{2}$$

where z represents the net present value of CCA for each dollar of investment, τ is the marginal corporate tax rate, and r is the interest rate. For an average firm investing in machinery or equip-

ment, the cost of capital decreases from 0.291 to 0.277 (by five percent) after the reform.[4]

While the reform (and the subsequent extension) aimed to induce investment in machinery or equipment, the depreciation schedules for other asset classes were generally not affected by the reform.[5] Firms that typically invest in Class 43 (concentrated in manufacturing industries) can claim larger deductions after the reform, so they would face greater tax incentives to increase investment. By contrast, firms that have never invested in Class 43 before the reform either rent, outsource or simply do not use machinery or equipment in their production, so they are less likely to invest even after facing greater incentives to do so. Therefore, these firms would be the least affected by the reform. This distinction forms the basis of the treated and control groups in our empirical design, which we explain in more detail in Section 3.

In practice, firms may not claim any CCA in a given year if they have no taxable income. If firms do not claim the full eligible deductions, the net present value of CCA would be lower. Since we are interested in all ACCA-eligible firms, including those not claiming full CCA in some years, the reduction in the cost of capital estimated above would be an upper bound of the actual reduction due to the reform. For example, Figure 1 shows the estimated changes in z (10.1 percentage points) and the cost of capital (1.1 percentage points or 3.8 percent from the baseline) for machinery and equipment, and they are smaller than the statutory changes as not all firms claim CCA in each year.

3 Empirical Strategy

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

[4]Note that this estimate is based on holding r and τ constant. While the top marginal corporate income tax rate changed during our sample period, its impact would be partly absorbed in a difference-in-differences framework (see Section 3) given that the vast majority of both treated and control firms face small business tax rates on most of their taxable income. However, changes in tax rates may still have differential impacts on the net present value of tax deductions, and therefore, on the cost of capital because treated and control firms invest in different asset classes. Therefore, we account for changes in corporate tax rates when estimating the changes in cost of capital and report our elasticities with respect to these estimated changes in cost of capital when interpreting our results.

[5]Three exceptions are (i) Class 1 (buildings used for manufacturing and processing goods), for which the CCA rate increased from 4 percent to 10 percent in 2007, (ii) Class 10 (computers), for which the CCA rate increased from 45 percent to 55 percent in 2007 and to 100 percent in 2009, and (iii) Class 43.1 (green technology), for which the CCA rate increased from 30 percent to 50 percent in 2007. However, the estimated changes in the net present value and cost of capital for these other assets are small for treated firms relative to control firms (in the sense of difference-in-differences) because control firms also invest in these asset classes and would benefit from them (see Appendix A.3). This suggests that changes in CCA rates for the other asset classes are unlikely important confounders in our setting.

3.1 Estimating Policy Impacts on Firm Outcomes

Since the reform accelerated the depreciation rate for Class 43 from 30 percent to 50 percent and switched to a straight-line rule, any firm (regardless of its sector or industry) that invests in machinery or equipment for manufacturing and processing goods after 2007 would benefit from this change. However, predicting which firm would invest in machinery or equipment after the reform is difficult in practice.

To identify the effects of the accelerated depreciation policy, we compare firms which had invested in Class 43 prior to the reform (from 2002 to 2006) with firms that *never* invested in Class 43 during the same period. Firms that never invested in Class 43 are likely the type of firms that either rent, outsource, or do not use machinery or equipment in their production.[6] Such firms are unlikely to purchase machinery or equipment in response to the reform and, therefore, are qualified as the control group. We verify that firms which never invested in Class 43 before the reform do not invest in machinery or equipment even after the reform.[7] Furthermore, firms that invested in Class 43 prior to the reform might not purchase machinery or equipment again after 2007, so our approach would identify the intent-to-treat (ITT) on the effects of accelerated depreciation.

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

$$Y_{jt} = \sum_{\tau=2002}^{2019} \theta_\tau \cdot \mathbb{1}_{\{t=\tau\}} \cdot Treated_j + \alpha_j + \phi_{k(j),t} + u_{jt}, \tag{3}$$

where Y_{jt} is the outcome of firm j in year t, $Treated_j$ is an indicator of a firm having positive investment in Class 43 from 2002 to 2006, α_j are firm fixed effects, and $\phi_{k(j),t}$ are industry (k) by year fixed effects. Each coefficient θ_τ measures the change in Y_{jt} for treated relative to control firms in year $t = \tau$ and θ_{2006} is normalized to zero. Standard errors are clustered at the firm level.

One potential concern is that firms investing in machinery or equipment are concentrated in particular industries. While the majority of treated firms are in the manufacturing industries, we still have a non-trivial share of control firms that rent or outsource in the manufacturing industries, as well as a decent share of treated firms in non-manufacturing industries, allowing us to exploit the within-industry variation. In fact, in terms of firm counts, we have almost as many control firms as

[6] Manufacturing firms that do not own machinery or equipment must rent it or outsource to produce goods or services; otherwise, they would not be classified into the manufacturing sector.

[7] The fraction of control firms that invest in machinery or equipment after the reform is so negligible that dropping these firms does not quantitatively affect our main results. This also addresses a concern that control firms may be misclassified if they invested in machinery or equipment prior to 2002 and need no further investment after 2002. In that case, such firms are not impacted by the accelerated depreciation policy as long as they do not invest in machinery or equipment after the reform.

treated firms within the manufacturing industries. In our main specification, we use within-industry variation by including industry by year fixed effects and drop a small share of industries that do not have either treated or control firms. In this way, both treated and control firms use machinery or equipment for their production especially within manufacturing industries, but control firms most likely rent it.[8] This addresses a concern that treated firms and control firms may use different production technologies, and therefore may be exposed to differential (i.e., industry-specific) shocks after the reform. However, exploiting purely across-industry variation in the treatment intensity, as in Zwick and Mahon (2017) and Curtis et al. (2022), shows pre-trends on the key outcomes in our setting. Controlling for industry by year fixed effects in our main specification absorbs any industry differences (i.e., in production technologies) between the treated and control groups in our sample and makes pre-trends on the key outcomes parallel between the two groups.[9]

We compute the average policy effects on firm-level outcomes by estimating the following difference-in-differences model:

$$Y_{jt} = \theta \cdot Post_t \cdot Treated_j + \alpha_j + \phi_{k(j),t} + u_{jt}, \tag{4}$$

where $Post_t$ is a dummy equal to one if year t is after the reform ($t \geq 2007$) and all 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. 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 fewer than five workers in 2006, thereby dropping very small businesses that rarely invest in machinery or equipment. These firms are also less comparable with treated firms which are on average larger in employment.[10] Third, we focus on four major provinces of Canada (Alberta, British Columbia, Ontario, and Quebec) that makeup almost 90 percent of the national economy (Duan and Moon, 2024). We also exclude firms in the agriculture and finance sectors (four percent of all firms in our sample) because most firms in these sectors do not buy machinery or equipment. In Appendix B.1, we show that our results are robust

[8] In Appendix A, we show separate results for firms in manufacturing industries and for firms in non-manufacturing industries, and find qualitatively similar results in both cases on the key outcomes.

[9] One concern for using the within-industry variation (as opposed to the across-industry variation) is the potential spillover effects on control firms given that treated and control firms compete in the input and output markets within the same industry. To address this concern, we perform a robustness check exploiting heterogeneity in the share of treated firms across industries. We find similar effects of the reform on the key outcomes in industries with a high share of treated firms and industries with a low share of treated firms, suggesting that the potential spillover effects appear to be minimal in our setting (Appendix B.6). This also addresses potential market-level effects on wages; for example, we expect the market-level effects on wages to be small or non-existent in industries where there is a low share of treated firms.

[10] We also need at least five workers in each firm to explore the within-firm heterogeneity on worker-level outcomes.

to including firms in these excluded provinces or sectors. Finally, we drop a small number of industries without any treated or control firm so that we can exploit the within-industry variation.

The main identifying assumption behind our empirical design is that the outcomes of treated and control firms would have trended similarly in the absence of the policy change. A key threat to this design is that time-varying shocks (specific to either treated or control firms) may coincide with the reform. We present several pieces of evidence that this threat is minimal. First, treated and control firms exhibit parallel trends for key outcomes prior to the reform. Second, our results are robust to various specifications with different controls and fixed effects. Third, within the manufacturing industries, control firms that rent machinery or equipment (instead of buying it) might be also affected by the reform since the accelerated depreciation may affect the price of machinery or equipment. In that case, control firms would be either positively or negatively affected by the reform because the rental price for machinery and equipment could also change in response to the accelerated depreciation. While this could lead to a 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.[11] Furthermore, the purchase price of machinery or equipment for the manufacturing sector stayed roughly flat compared to that of non-manufacturing sectors. These results (in Appendix B.5) provide suggestive evidence that the rental price likely stayed flat, consistent with findings in the United States where prices of capital goods did not change after the bonus depreciation policy possibly due to the growth of imported capital (House and Shapiro 2008; 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 rent machinery or equipment may be exposed to differential shocks relative to firms that buy these assets for their production due to differences in firm sizes or financial constraints. We use two different strategies to address this concern. First, we redefine treated and control groups based on the share of their manufacturing assets prior to the reform, thereby including firms that buy machinery or equipment as part of the control group. 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, and control firms as those whose manufacturing assets account for less than 30 percent of their total assets (Appendix B.2).[12] In other words, part of our control firms still invested in machinery or equipment prior to the reform, thereby minimizing the aforementioned concern. Second, since firms that buy machinery or equipment are larger on average relative to firms that rent or outsource

[11] While we include firms and workers in machinery manufacturing industries (NAICS 333) in our main analysis sample, our results remain similar even when we exclude these industries (see Appendix B).

[12] 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.

(Table 1), it is possible that firms of different sizes may be exposed to differential shocks. To address this concern, we use an alternative design where we match firms within the same industry based on their total assets, employment, and revenue using the coarsened exact matching so that treated firms and matched control firms are more comparable in terms of industry compositions and sizes (Table A3). We find qualitatively similar results even when their sizes are comparable (Appendix B.3).[13]

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:

$$Y_{it} = \sum_{\tau=2002}^{2019} \theta_\tau \cdot \mathbb{1}_{\{t=\tau\}} \cdot Treated_i + \alpha_i + \phi_{k(i),t} + u_{it}, \tag{5}$$

where Y_{ijt} is the outcome of worker i in year t, $Treated_i$ is an indicator that worker i works at a treated firm in 2006, α_i are worker fixed effects, and $\phi_{k(j),t}$ are industry (k) by year fixed effects. In particular, we define treated and control workers based on whether they worked at treated or control firms in 2006 (one year before the reform). Each coefficient θ_τ measures the change in Y_{jt} for treated relative to control workers in year $t = \tau$, and θ_{2006} is normalized to zero. Standard errors are two-way clustered at both firm and worker levels.

We estimate the average policy effects on worker-level outcomes using the following difference-in-differences model:

$$Y_{it} = \theta \cdot Post_t \cdot Treated_i + \alpha_i + \phi_{k(i),t} + u_{it}, \tag{6}$$

where $Post_t$ is a dummy equal to one if $t \geq 2007$. All the other variables are defined in equation (5). We report estimates from both equations (5) and (6) in Section 4.

We impose the following restrictions on our worker sample. First, we drop workers with multiple jobs in a given year and focus on full-time workers. Second, we restrict workers to have at least 4,000 CAD in annual earnings so that we study workers with stable income and attached to their firms (Card et al., 2013a; Sorkin, 2018). Finally, we require that workers were continuously employed in a treated or control firm during the entire pre-reform period (2002 – 2006). This tenure restriction, similar to that in the mass layoff literature (Jacobson et al., 1993; Von Wachter et al., 2009; Lachowska et al., 2020), is chosen to obtain a sample of workers with attachment to

[13] Note that we do not use the matched sample as our main analysis sample mainly because the sample size gets much smaller. However, all the other firm-level and worker-level results (besides the key results shown in Appendix B.3) remain quite similar, which can be provided upon request.

firms in our analysis sample. While these restrictions allow us to focus on full-time workers with stable jobs at either treated or control firms, we find qualitatively similar results in a robustness check including multiple-job holders, workers making below 4,000 CAD in annual earnings, or not satisfying the tenure restriction (see Appendix B.1).

3.3 Data and Variable Definition

For our empirical analysis, we use the Canadian Employer Employee Dynamics Database (CEEDD), a matched employee-employer dataset that covers the universe of workers and incorporated businesses in Canada from 2002 to 2019. The CEEDD draws information from both individual (T1) and corporate (T2) tax returns, merged with firm-level information from the National Accounts Longitudinal Micro-data File (NALMF) and job-level information from employee tax records (T4) and Record of Employment (ROE).

The main outcome variables in the firm-level analysis are employment, average payrolls, investment rates, total revenue, after-tax profits, and return on assets. Employment is defined as the yearly-average number of employees with T4 records. From a separate dataset on CCA claims, we observe firms' expenditures in each asset class by year. Then we define investment rates (ι_{jt}) as

$$\iota_{jt} = \frac{I_{jt}}{K_{j,t-1}} = \frac{K_{jt} - K_{j,t-1} + \delta_{jt}}{K_{j,t-1}}, \tag{7}$$

where K_{jt} is the book value of tangible assets of firm j in year t and δ_{jt} is the value of depreciation. After-tax profits are net income after taxes and extraordinary items, and return on assets is defined as net income divided by total assets. In addition, we identify each firm's industry at the 4-digit level according to the North American Industry Classification System (NAICS).

At the worker level, the key outcome is annual earnings, which are aggregated across all employers in a given year. While we include earnings from all employers, we associate workers with the "dominant" employer each year, from which the worker receives the highest pay in that year.

Finally, using our matched data, we estimate the firm-specific and worker-specific components of workers' annual earnings following the two-way fixed effect model (AKM hereinafter) following Abowd et al. (1999). Then we can associate changes in job movers' earnings after the reform with changes in employers' firm-specific wage premiums. Let y_{it} denote the log earnings of worker i in year t and $j(i,t)$ index the worker's employer. We regress y_{it} against the worker fixed effects ω_i, the employer fixed effects $\psi_{j(i,t)}$, and year fixed effects τ_t:

$$y_{it} = \omega_i + \psi_{j(i,t)} + \tau_t + u_{it}. \tag{8}$$

We can then assess the role played by the employers by estimating an analogue to equation (5), replacing the outcome variable by the estimated firm fixed effects $\hat{\psi}_{j(i,t)}$. We also use the estimated worker fixed effects $\hat{\omega}_i$ to categorize workers into high skills versus low skills. More details of the AKM model estimation is discussed in Appendix A.2.

To prevent the disclosure of confidential information, Statistics Canada requires rounding estimates and observation counts. All outcome variables are winsorized at the first and ninety-ninth percentiles, except for after-tax profits that are winsorized at the fifth and ninety-fifth percentiles.

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 in the pre-reform period (2002 – 2006), separately for treated firms and control firms. On average, treated firms are larger in terms of tangible assets, employment, average payrolls, and revenue.[14] Another key difference is that control firms have higher leverage ratios and lower retained earnings (scaled by assets), implying that they are more cash-constrained relative to treated firms. This may explain why control firms in the manufacturing sector rent machinery or equipment instead of buying it.[15] However, treated and control firms are similar in terms of investment rates and profitability, suggesting that pre-existing differences in these dimensions are too small to confound the effects of the reform. Furthermore, if differences in cash constraints or sizes are the main confounders, 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. Given that the vast of majority of both treated and control firms are small- to medium-sized companies in our analysis sample, only about 2 percent of them have multiple establishments across different provinces and dropping these firms does not quantitatively affect our main results.

In the treated group, the majority of firms are in the manufacturing sector (70 percent), with the rest in construction (4.1 percent), wholesale (8.8 percent), retail (5.0 percent), and other services (7.9 percent) which include administrative support, education, accommodation, and food services. By contrast, the majority of control firms are in the construction (14.9 percent), retail (20.1 percent), and other services (24.9 percent). Given the difference in the sectoral composition between treated and control firms, we include industry by year fixed effects in our main specification.

[14] 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 (Appendix B.4).

[15] Table A2 in Appendix A.1 shows separate descriptive statistics for the manufacturing and non-manufacturing sectors. The differences in leverage ratio and retained earnings (scaled by assets) are more pronounced in the manufacturing sector, where control firms must outsource or rent machinery or equipment to manufacture goods or services.

Panel B of Table 1 shows the pre-reform means for key variables in the worker sample in 2002 – 2006, separately for treated and control workers. On average, treated workers earn slightly more than control workers before the reform, although the average age is similar between the two groups. In Appendix B.4, we show qualitatively similar estimates on earnings using a matched sample of treated and control workers based on their industry and demographics.

4 Results

This section reports estimation results from Section 3 and discusses the interpretation of the results.

4.1 Capital Investment, Employment, and Average Payrolls

Panels (a) – (b) of Figure 2 plot estimates of θ_τ from equation (3) with investment rates on eligible assets and total tangible assets (including eligible assets) as the outcome variables. As explained in Section 2, assets eligible for accelerated depreciation are machinery and equipment used for manufacturing goods or services, so we define eligible assets as the sum of classes 43, 29, and 53 and construct the investment rate by equation (7). Panel (a) shows that the investment rate in machinery or equipment was relatively flat prior to the reform, dipped slightly in 2007, but began increasing the year after. Both the initial dip in 2007 and the second dip in 2016 reflect the reclassification of machinery and equipment (from Class 43 to Class 29, and then to Class 53), where investment in the old asset class mechanically shrinks as firms need to dispose existing capital, and investment in the new asset class starts being reflected in the tax records in the subsequent year. The quick recovery and increases are consistent with these firms raising investment in response to the policy.

Since the investment rate on eligible assets is mechanically zero for control firms, the above estimates capture raw changes in investment rates among treated firms. In other words, there is no control group to absorb the effects of macro-level shocks (e.g., the 2007 – 2009 global recession). To address this concern, we additionally look at the investment rate on total tangible assets, for which control firms have positive investment rates as they invest in other tangible assets. Panel (b) shows that treated and control firms shared parallel pre-trends in the investment rate prior to the reform. After 2007, the investment rate increased for treated firms relative to control firms, consistent with estimates for eligible assets. As Table A4 in Appendix A.1 shows, investment in machinery and equipment accounts for slightly more than a half of total capital investment in a given year for treated firms, so the increased investment rate in total tangible assets largely reflects investment in machinery or equipment.

Columns (1) – (2) of Table 2 present the difference-in-differences estimates from equation (4) for investment rates on eligible assets and total tangible assets, respectively. Column (1) shows that, on average, treated firms' investment rate in machinery or equipment increased by 5.4 percentage points (19 percent) after the reform. Column (2) shows that firms' investment rate in total tangible assets increased by 1.4 percentage points (13 percent). Using the estimated reduction in the cost of capital (3.8 percent) as shown in Figure 1, the elasticities of eligible investment and total tangible investment respect to the change in cost of capital are -5.0 and -3.4. Note that these investment elasticities are larger than the ones found in Curtis et al. (2022), possibly because firms in our analysis sample are small- to medium-sized firms on average, which may respond aggressively to tax incentives due to financial constraints (Zwick and Mahon 2017). Taken together, firms that had invested in machinery or equipment before the reform responded strongly to the policy by investing more in tangible assets (especially machinery or equipment), relative to control firms.

Panels (c) – (d) of Figure 2 plot estimates of θ_τ from equation (3) for firms' employment and average payrolls, respectively. For both outcomes, treated and control firms followed similar patterns before 2007. 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 average salaries. Columns (3) – (4) of Table 2 show that treated firms' average employment and average payrolls increased by 4.5 percent and 2.2 percent, respectively, suggesting a significant response in labor demand. The elasticity of employment with respect to the change in cost of capital is -1.2, which is comparable to the baseline estimate found in Curtis et al. (2022). Note that average payrolls stayed relatively flat for the first three years after the reform and began increasing afterward. In the following section, we assess worker-level earnings to check whether these dynamic responses in firm-level payrolls are driven by changes in worker composition.

4.2 Worker-level Earnings and Job Transitions

To estimate how the accelerated depreciation policy affects worker earnings, we turn to worker-level data, which allows us to track the same workers over time and, therefore, control for changes in the worker composition of firms. In panel (a) of Figure 3, we plot $\hat{\theta}_\tau$ from equation (5) with workers' annual earnings as the outcome variable. As the figure shows, the annual earnings of treated workers trended similarly to those of control workers in the years prior to 2007 but decreased significantly after 2007. Column (1) of Table 3 shows that the average earnings of treated workers decreased by 1.4 percent after the reform.

Next, we examine whether more workers moved out of treated firms after the accelerated dep-

recation policy. Panel (b) of Figure 3 and column (2) of Table 3 show that, for treated workers, the probability of moving to another firm remained unchanged relative to control workers. For workers who continued to stay at treated firms ("stayers") relative to stayers at control firms, their annual earnings dipped in the first few years after 2007 but eventually returned to the pre-reform level after 2013 (panel (c) of Figure 3). Column (3) of Table 3 shows that, on average, the annual earnings of stayers did not change much after the reform.

By contrast, workers who left treated firms after the reform ("movers") experienced a significant and persistent loss in earnings compared to movers from control firms (panel (d) of Figure 3). Column (4) of Table 4 shows that the annual earnings decreased by 3 percent on average for job movers from treated firms after the reform. Here, we allow job movers to transition to other firms (and more than once) throughout the entire post-reform period. However, nearly half of the movers from both treated or control firms had transitioned to other firms in the first three years after the reform, and our estimates on job movers' earnings remain similar when focusing on these workers (Appendix B.1). This result, in conjunction with the result for stayers, suggests that the decline in incumbent workers' earnings at treated firms was primarily driven by job movers. In the rest of the paper, we explore potential mechanisms behind the changes in incumbent workers' earnings.

4.3 Sales and Profitability

The increases in employment and average payrolls among treated firms suggest that the decline in worker-level earnings is not driven by a decrease in labor demand. We examine whether the decrease in worker earnings is driven by decreases in firms' sales and profitability, in the form of rent-sharing. Figure 4 plots estimates of θ_τ from equation (3) on firms' revenue, after-tax profits, and ROA. Panels (a) – (c) show that treated and control firms followed a similar pattern in these outcomes before 2007. After 2007, we observe significant increases in these outcomes for treated firms relative to control firms, suggesting that the accelerated depreciation policy led to greater sales, after-tax profits, and ROA. While the increase in after-tax profits partly reflects a mechanical effect of the tax deduction, the increase in 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, while the increased investment rates in tangible assets and employment occur right after the reform. The timing of these responses suggests that the increased investment and employment may be connected with increased sales and profitability.

Table 4 reports 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 after the reform. This implies

large and significant responses in firm growth and profitability. Therefore, 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. Second, we additionally include commuting zone by year fixed effects to absorb local labor market specific shocks that potentially drive our findings. Third, we include previously excluded provinces and sectors (agriculture and finance) and find qualitatively similar results. Fourth, in the preferred specification of equations (5) and (6), we impose tenure restrictions and drop multiple-job holders or those making below 4,000 CAD in annual earnings. We relax these restrictions and obtain qualitatively similar worker-level estimates. Furthermore, we use an alternative definition of treated and control firms, based on their shares of pre-reform investment in machinery or equipment being above or below 30 percent (instead of zero). Finally, we match firms based on their industry, total assets, employment, and revenue, and match workers based on their industry and demographics. In all cases, we find qualitatively similar effects on both firm-level and worker-level outcomes. Results from these robustness tests can be found in Appendix B.

5 Mechanisms and Economic Interpretations

While the accelerated depreciation policy led to positive firm outcomes, it also led to declines in incumbent workers' earnings, especially among those who left treated firms after the reform. This section discusses potential explanations behind the effects on incumbent workers' earnings. In particular, the key advantage of using the worker-level data is that we can separate incumbent workers into firm stayers and job movers, and separately look at their responses. In this way, not only we can see how the within-firm skill ratio and skill premium (defined in Section 5.2) change after the reform, but also we can test whether there are different responses among job movers depending on their skill level (proxied by the worker fixed effects estimated from equation (8)). Finally, we use our estimates to conduct a cost-per-job calculation for policy implications.

5.1 Conceptual Framework

In this section, we illustrate a model to derive empirical predictions and explore the mechanisms behind our findings, following closely the frameworks by Krusell et al. (2000), Curtis et al. (2022) and Lindner et al. (2022). Our model shows that following a reduction in the cost of capital, a profit-maximizing firm would increase both capital investment and employment. Due to the greater complementarity between capital and skilled labor (Krusell et al., 2000), the rise in capital investment would increase the demand for skilled labor relative to unskilled labor. In a monopsonistic labor market (Lindner et al., 2022), this further leads to increases in the skill ratio (employment of skilled labor relative to unskilled labor) and the skill premium (the wage gap between skilled and unskilled labor) at the firm level. Furthermore, these predictions imply that the firm-level technological change driven by the rise in capital is skill biased, which is the key mechanism behind changes in incumbent workers' earnings in our setting. The model has two key features. First, firms produce a final good with capital and two types of labor (skilled and unskilled), using a technology where capital is more complementary to skilled labor than to unskilled labor (Krusell et al., 2000). Second, we allow firms to set wages Card et al. (2003), consistent with our firm-level results. We assume perfect competition in the product market following Curtis et al. (2022).

5.1.1 Setup

Suppose that a firm produces output Y using unskilled labor L, skilled labor H, and capital K, according to the following production function F:

$$Y = F(L, H, K) = \left[\mu L^{\frac{\sigma-1}{\sigma}} + (1-\mu)(KH)^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}}, \quad \sigma > 1. \tag{9}$$

Note that equation (9) has a nested structure: H and K are first composited in a Cobb-Douglas form, and the output is composited again with L in a CES form. We assume that $\sigma > 1$ so capital complements skilled labor more relative to unskilled labor.[16]

Let W_L and W_H be the prices of unskilled labor and skilled labor, and R be the tax-inclusive

[16] Our specification differs from Curtis et al. (2022) and Lindner et al. (2022) in the following ways. Curtis et al. (2022) assume a flexible elasticity of substitution between capital and skilled labor σ_{KH} besides the elasticity between KH and unskilled labor (σ in our case). In comparison, we impose the restriction $\sigma_{KH} = 1$ in order to derive the relationship between capital investment, skill ratio, and skill premium. We maintain the assumption $\sigma > \sigma_{KH}$ so that increasing capital investment has a more positive effect on skilled labor relative to unskilled labor. Meanwhile, Lindner et al. (2022) consider a two-factor production function where the relative productivity of skilled labor versus unskilled labor is determined by an exogenous technology shifter (μ in our model). In contrast, we examine the role of capital K as a technology shifter and how it endogenously respond to changes in the capital cost. In other words, we extend Lindner et al. (2022) by endogenizing the firm-specific technological change with respect to labor productivity.

price of capital as in equation (2):

$$R = \frac{(1-\tau z)r}{1-\tau}.$$

The firm faces a competitive capital market where R is exogenously given, but it enjoys monopsony power in the labor markets. Following Card et al. (2003), we assume that the markets for skilled and unskilled labor are segmented and monopsony power arises from workers' idiosyncratic preferences for non-wage job attributes. Then the labor supply curves are given by

$$L = \alpha_L[(1-\tau_w)W_L]^{\beta}, \tag{10}$$

$$H = \alpha_H[(1-\tau_w)W_H]^{\beta}, \tag{11}$$

which are upward-sloping and micro-founded by workers' discrete choice problem (Card et al. 2003). τ_w is the labor income tax rate and β captures the labor supply elasticity with respect to the after-tax wages. (α_H, α_L) measure the average valuation for the firm's non-wage characteristics, by skilled and unskilled workers, respectively.

Finally, the firm's problem is to maximize profits

$$\Pi(L,H,K) = (1-\tau)F(L,H,K) - (1-\tau)W_L L - (1-\tau)W_H H - (1-\tau z)K,$$

subject to (10) – (11). The output is sold in a competitive product market at the unit price and serves as the numeraire (Curtis et al., 2022). We solve the firm's problem using backward induction. First, we fix K and solve for the equilibrium wages and employment. Second, given the labor market equilibrium conditions, we find the firm's optimal demand for capital K in response to the tax-inclusive cost R, following the accelerated depreciation policy.

5.1.2 Capital Investment, Skill Ratio, and Skill Premium

Given the the value of K, firms choose L and H according to its first order conditions

$$\frac{\partial \Pi}{\partial L} : W_L = \mu\left(\frac{L}{Y}\right)^{-\frac{1}{\sigma}}, \tag{12}$$

$$\frac{\partial \Pi}{\partial H} : W_H = (1-\mu)K^{\frac{\sigma-1}{\sigma}}\left(\frac{H}{Y}\right)^{-\frac{1}{\sigma}}, \tag{13}$$

which suggest that the firm's labor demand decreases in wages. Denote the natural logarithm of each variable in lower cases. Then (12) – (13) imply

$$w_L = \log \mu - \frac{1}{\sigma}(l - y), \tag{14}$$

$$w_H = \log(1 - \mu) + \frac{\sigma - 1}{\sigma}k - \frac{1}{\sigma}(h - y). \tag{15}$$

Subtracting (14) from (15) yields the relative demand for skilled versus unskilled labor

$$w_H - w_L = \frac{\sigma - 1}{\sigma}k - \frac{1}{\sigma}(h - l) + \log(1 - \mu) - \log \mu, \tag{16}$$

such that the relative demand for skilled versus unskilled labor $(h - l)$ is negatively correlated with the relative wage $(w_H - w_L)$. As the price of one input increases relative to the other input's price, firms will shift away from the more expensive input to the less expensive one.

On the supply side, rewrite (10) – (11) as

$$l = \log \alpha_L + \beta w_L, \tag{17}$$

$$h = \log \alpha_H + \beta w_H. \tag{18}$$

Combining (16) – (18), we obtain an expression for the equilibrium skill premium

$$w_H - w_L = \left(1 + \frac{\beta}{\sigma}\right)^{-1} \left[\frac{\sigma - 1}{\sigma}k - \frac{1}{\sigma}(\log \alpha_H - \log \alpha_L) + \log(1 - \mu) - \log \mu\right]. \tag{19}$$

For $\sigma > 1$, the skill premium increases in capital stock k. This is because capital increases the marginal product of skilled labor more than that of unskilled labor. The skill premium also increases in the relative importance of skilled labor in production $(1 - \mu)$ and decreases in the non-wage valuation by skilled workers relative to unskilled workers $(\log \alpha_H - \log \alpha_L)$. The latter reflects compensating wage differentials. It follows immediately that the equilibrium skill ratio

$$h - l = (log\alpha_H - log\alpha_L) + \beta(w_H - w_L), \tag{20}$$

increases in the skill premium and therefore, also increases in k. Hence, capital investment raises not only the wages but also the employment of skilled workers relative to unskilled ones. This is consistent with the effects of skill-biased technological changes, which are observed both at the macro level (Katz and Murphy, 1992) and at the firm level when firms enjoy wage-setting power (Lindner et al., 2022).

5.1.3 Accelerated Depreciation Policy and Firm-level Outcomes

Next, we examine the firm's choice of K given the exogenous tax-inclusive cost of capital R. When the firm decides on its capital investment, it anticipates three channels by which K may affect profits: (i) directly through the marginal product of capital; (ii) indirectly by changing the firm's optimal choice of labor inputs $(L^*(K), H^*(K))$ and the marginal product of labor; and (iii) indirectly by changing the equilibrium wages $(W_L^*(K), W_H^*(K))$ and the marginal cost of labor. As such, it would be challenging to derive the analytical relationship between K and R as we did in the previous subsection.

Despite the complications, we can show that the firm's optimal capital stock always decreases in the cost of capital. As such, ACCA would stimulate capital investment and, by equations (19) – (20), would increase both the skill ratio and the skill premium. The key idea is to show that increasing capital investment yields diminishing returns on profits, so that reducing the cost of capital leads to lower marginal returns by investing more.

Let $\phi(K) = \Pi(L^*(K), H^*(K), K; W_L^*(K), W_H^*(K))$ denote the firm's maximized profit conditional on its choice of K. By the envelope theorem,

$$\begin{aligned} \frac{d\phi(K)}{dK} &= \left.\frac{\partial \Pi(\boldsymbol{X}, K)}{\partial K}\right|_{\boldsymbol{X}=\boldsymbol{X}^*} \\ &= -R + \left.\frac{\partial F(L, H, K)}{\partial K}\right|_{L=L^*(K), H=H^*(K)}, \end{aligned} \tag{21}$$

where $\boldsymbol{X}$ summarizes the four labor market variables that depend on K. The second line follows from the specification of Π. As such, the production function F and the cost of capital R provides sufficient information to characterize the impact of K on equilibrium profits ϕ. Following from the properties of CES production functions, F is concave with respect to L and the composite (KH). Thus, F exhibits a diminishing marginal product of capital:

$$\frac{\partial^2 F}{\partial (KH)^2} < 0 \Longrightarrow \frac{\partial^2 F}{\partial K^2} < 0. \tag{22}$$

Combining equations (21) – (22), we conclude that

$$\frac{d^2\phi(K)}{dK^2} < 0,$$

i.e., increasing capital investment yields dinimishing returns on profits. Therefore, the firm will optimally choose a higher K when facing a lower R after the accelerated depreciation policy (see Section 2 on how ACCA lowers the tax-inclusive cost of capital).

To summarize, our model predicts that following the accelerated depreciation policy, the firm would increase capital investment, which subsequently increases both the skill ratio and the skill premium. To test the model predictions, we examine whether there exist heterogeneous effects on the earnings of incumbent workers depending on their skill type in the next section.

5.2 Changes in Within-firm Skill Ratio and Skill Premium

We conjecture that the decline in incumbent workers' earnings is driven by skill-biased technological change, resulting from the rise of capital investment in response to accelerated deprecation. In the previous section, we illustrate a model similar to Lindner et al. (2022), where firm-level skilled biased technological change can result in changes in both the skill ratio and the skill premium when the labor market is not perfectly competitive. A key difference is that in the case of Lindner et al. (2022), what drives the firm-level technological change is the innovation shock (i.e., R&D policy), while it is the increased capital investment in our setting (Krusell et al. 2000). Based on the model predictions, increases in the skill ratio and the skill premium at the firm level would imply that the change in the technology is skill biased and explain the changes in incumbent workers' earnings.

To test this channel, we first check how the skill ratio, defined as the number of high-skilled workers relative to the number of low-skilled workers at the firm level, changes after the reform. We use the worker fixed effects estimated from the equation (8) to proxy for their skill level, and define high-skilled workers as those at the top quartile and low-skilled workers as those at the bottom quartile of the within-firm distribution. Panel (a) of Figure 5 shows that the skill ratio for treated firms remained flat relative to that of control firms prior to the reform, and significantly increased after the reform. Column (1) of Table 5 shows that the skill ratio increased by 4.4 percentage points (about 4 percent) on average after the policy.

Second, we check how the skill premium, defined as the earnings gap between high-skilled workers and low-skilled workers, changes after the reform. Within each firm j and year t, we calculate the difference between the top and the bottom quartile of log annual earnings using worker-level data, and define the skill premium as:

$$\text{Skill Premium}_{jt} = \log\left(\frac{\text{Annual Earnings}_{jt}^{p75}}{\text{Annual Earnings}_{jt}^{p25}}\right).$$

Panel (b) of Figure 5 shows that the skill premium for treated firms relative to that of control firms remained flat prior to the reform, and increased significantly afterward. Since this increase in the earnings gap can be driven by changes in worker composition, Panel (c) of Figure 5 shows

these estimates only for workers who continue to stay at their original firm after the reform, thereby holding the worker composition fixed. Panel (d) shows the result for new entrants. Columns (2) – (4) of Table 5 show that the skill premium increased by 0.58 log points, 4.77 log points, and 1.23 log points among all workers, stayers, and entrants, respectively. Overall, our findings suggest that among incumbent stayers and among entrants, changes in their earnings are likely driven by skill-biased technological change induced by the rise in capital investment. Next, we turn to workers who leave treated firms after the reform, and explore whether changes in their earnings are also consistent with skill-biased technological change.

5.3 Changes in Job Movers' Earnings

As shown in Section 4, the earnings losses of workers at treated firms is largely driven by those who move to other firms after the reform. In this section, we examine whether changes in firm characteristics or firm-specific wage premiums can explain the decline in earnings of the job movers. Here, control workers are also job movers from control firms.

5.3.1 Becoming Unemployed or Switching Industries

We first test whether the decline in job movers' earnings is due to these workers becoming unemployed after the reform. Panel (a) of Figure 6 and Column (1) of Table 6 show that the job movers from treated firms are not more likely to become unemployed relative to job movers from control firms. This result is important as it suggests that the decline in earnings of job movers from treated firms is not driven by these workers becoming unemployed after leaving their firm. Second, we test whether the decline in earnings of job movers can be explained by these workers switching industries (and potentially their occupations). Panel (b) of Figure 6 shows that a small share of treated workers moves to firms in different industries. Column (2) of Table 6 shows that the share of workers who switches industries increases by 4.7 percentage points after the reform among the job movers. The share of workers switching industries, however, is not large enough to be the primary explanation behind the decline in job movers' earnings.

5.3.2 Changes in Firm Characteristics

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. Tracking changes in firm-specific wage premiums 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 exam-

ple, if workers are moving to firms with lower wage premiums, then the loss of the employer wage premium from the old firm may help explain the decline in workers' earnings (Lachowska et al. 2020). We estimate the share of earnings losses following job transitions that can be attributed to the change in wage premiums $\hat{\psi}_j$ between the mover's new employer and the previous employer. Panel (a) of Figure 7 shows that workers who leave treated firms after the reform move to firms with lower wage premiums relative to job movers from control firms after the reform. Column (1) of Table 7 shows that these workers move to firms with 2.3 log points lower firm-specific wage premiums on average, implying that more than three quarters of the earnings losses can be attributed to decreases in firm-specific wage premiums.

Next, we assess whether workers who leave treated firms move to other firms that are less likely to invest in new machinery or equipment. Panel (b) of Figure 7 and Column (2) of Table 7 show that these workers move to firms with a much lower likelihood of investing in any manufacturing assets. This result implies that workers who leave treated firms move to other firms with relatively lower demand for machinery and equipment on average. In Appendix B.4, we show that most of these other firms are either (smaller) treated firms or those outside of our analysis sample. In other words, there is no change in reallocation of workers between treated firms and control firms after the reform, so our difference-in-differences estimates on firms' employment or average payrolls are not biased due to workers moving from control firms to treated firms (or vice versa). Next, we explore mechanisms behind the decline in earnings and firm-specific wage premiums of job movers by looking at the type of job separations and worker characteristics.

5.3.3 Heterogeneity by Types of Job Separation

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 become displaced after the reform.[17] If the decline in earnings of job movers from treated firms is driven by their skills being incompatible with new machinery or equipment, we would expect displaced workers to experience larger declines in earnings and firm-specific wage premiums, compared to workers who voluntarily leave their firm. However, since control workers are also displaced from their firm after the reform, it suggests that job movers from treated firms face even larger frictions 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 reform.

Panel (a) of Figure 8 compares the earnings of workers moving out of treated firms after the reform, relative to those out of control firms, by reasons for separation (involuntary displacement

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

or voluntary quit). Relative to their control counterparts, those who get displaced experience a large decline in earnings, whereas those who move voluntarily experience almost no changes in their earnings on average. Panel (b) shows that displaced workers in particular move to firms with lower firm-specific wage premiums. Table 8 confirms that the long-term average decline in worker earnings and firm-specific wage premiums are much larger for workers who get displaced than workers who voluntarily move to other firms. Therefore, these results are consistent with the idea that displaced workers from treated firms face particularly larger search frictions, likely due to their skills incompatible with new machinery or equipment, following the increase in relative demand for high-skilled workers among treated firms after the policy.

5.3.4 Heterogeneity by Worker Skills

Next, we consider heterogeneity by the type of workers. Intuitively, low-skilled workers who get separated may suffer from greater losses in their earnings and firm-specific wage premiums, likely due to the lack of skills to operate new machines or equipment. To proxy the skill type, we exploit the worker fixed effects ω_i estimated from equation (8). In general, workers at the upper quantile of the within-firm distribution of ω_i have skills that are valued more across all employers. In Figure 9, we plot the impacts for job movers across different quartiles of the within-firm worker fixed effects distribution. Panel (a) shows that the decline in earnings is concentrated among individuals at the bottom 50 percentile, and we find even an increase in earnings for workers at the top quartile.[18]

Additionally, we find that bottom-quartile workers move to worse-paying firms on average. As shown in Panel (b) of Figure 9, job movers at the bottom 50 percentile are moving to firms with lower wage premiums relative to job movers at the top 25 percentile. Furthermore, Panel (c) of Figure 9 shows that lower fixed effects workers are more likely to get displaced from treated firms relative to workers with higher fixed effects.

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. By contrast, job movers at the top quartile experience 3.9 log points increase in their earnings and move to firms with comparable wage premiums after the reform. Our worker-level heterogeneity results are consistent with the idea that those with lower worker fixed effects suffer from larger earnings losses after the reform, likely due to their skills incompatible with new manufacturing investment at treated firms. Taken together, our overall results are consistent with the idea that low-skilled workers who move from treated firms experience wage losses due to the change in labor demand driven by skill-biased technological change.

[18] We get qualitatively similar results (which can be provided upon request) when we define high-skilled workers and low-skilled workers using the entire worker fixed effects distribution (instead of using the within-firm distribution).

5.4 Cost-per-Job Estimate

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

$$\text{Cost-per-Job} = \frac{\sum_{j \in Treated} \sum_{t=2007}^{2019} \frac{\tau_t}{(1+r)^t} \cdot (CCA_{jt}^{accelerated} - CCA_{jt}^{normal}) \cdot \mathbb{1}[Taxable]_{jt}}{\text{Total Number of Jobs Created}}.$$

We estimate the fiscal cost using historical statutory corporate income tax rates τ_t from 2007 to 2019 and a constant interest rate $r = 0.07$. For each treated firm j, $CCA_{jt}^{accelerated}$ and CCA_{jt}^{normal} represent the tax benefits accruing to the firm due to investment in machinery or equipment in year t, with and without accelerated depreciation, respectively. Both quantities are computed using the firm's observed CCA claims, federal and provincial corporate income tax rates, and CCA schemes with and without accelerated depreciation described in Section 2. Finally, whether 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 it seems that the Canadian acceleration depreciation policy was more efficient at creating jobs, several alternative factors could explain the discrepancy in estimates across different settings. First and foremost, Garrett et al. (2020) do not observe $CCA_t^{accelerated} - CCA_t^{normal}$ but instead use past (conservative) estimates to approximate its value. This may lead to an overestimation of the cost-per-job. By contrast, we directly estimate the term using firm-level tax records, which allows us to measure the fiscal cost more accurately. Second, differences in firm characteristics or institutional settings may result in different responses to tax incentives, potentially causing treated firms in Canada to hire more workers following the same change in the cost of capital. Admittedly, understanding the precise source of differences between these estimates is difficult and beyond the scope of our study. Nevertheless, 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 or 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.

While firms affected by the policy increase capital investment and employment, we find that incumbent workers' earnings decrease significantly after the reform, largely driven by those displaced from treated firms. In addition, we find the decrease in earnings is concentrated among job movers at the lower quantile of the skill distribution. Furthermore, the within-firm wage inequality increases significantly among treated firms relative to control firms after the reform, providing evidence of skill-biased technological change induced by the rise in capital investment. 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, leading to earnings losses of low-skilled incumbent workers.

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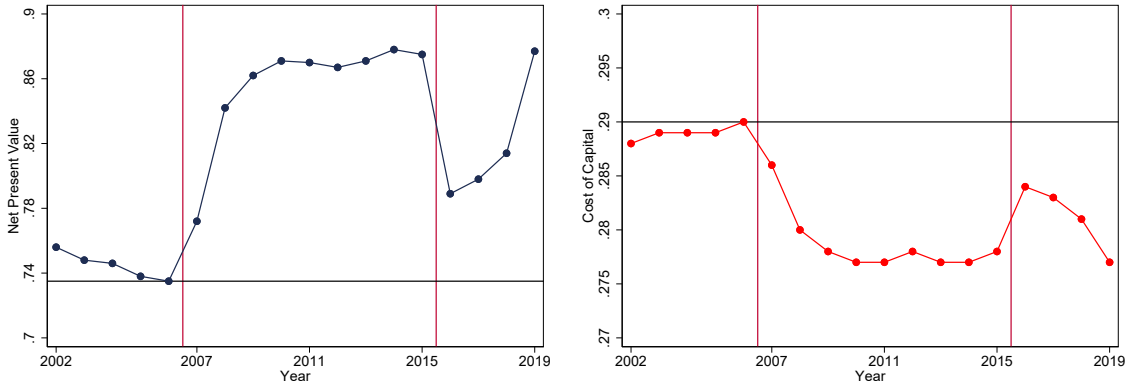
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Figure 1: Net Present Value of CCA and the Cost of Capital

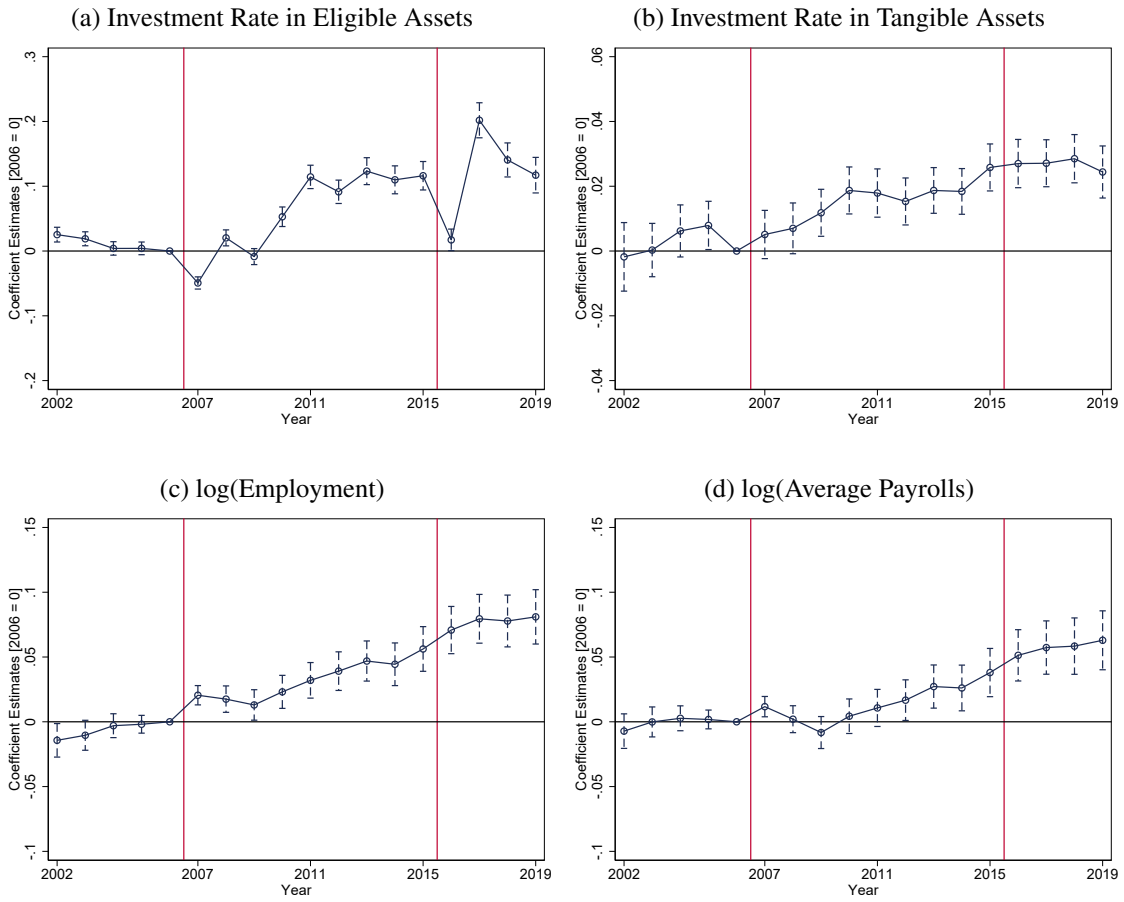
(a) Net Present Value of CCA

(b) Cost of Capital



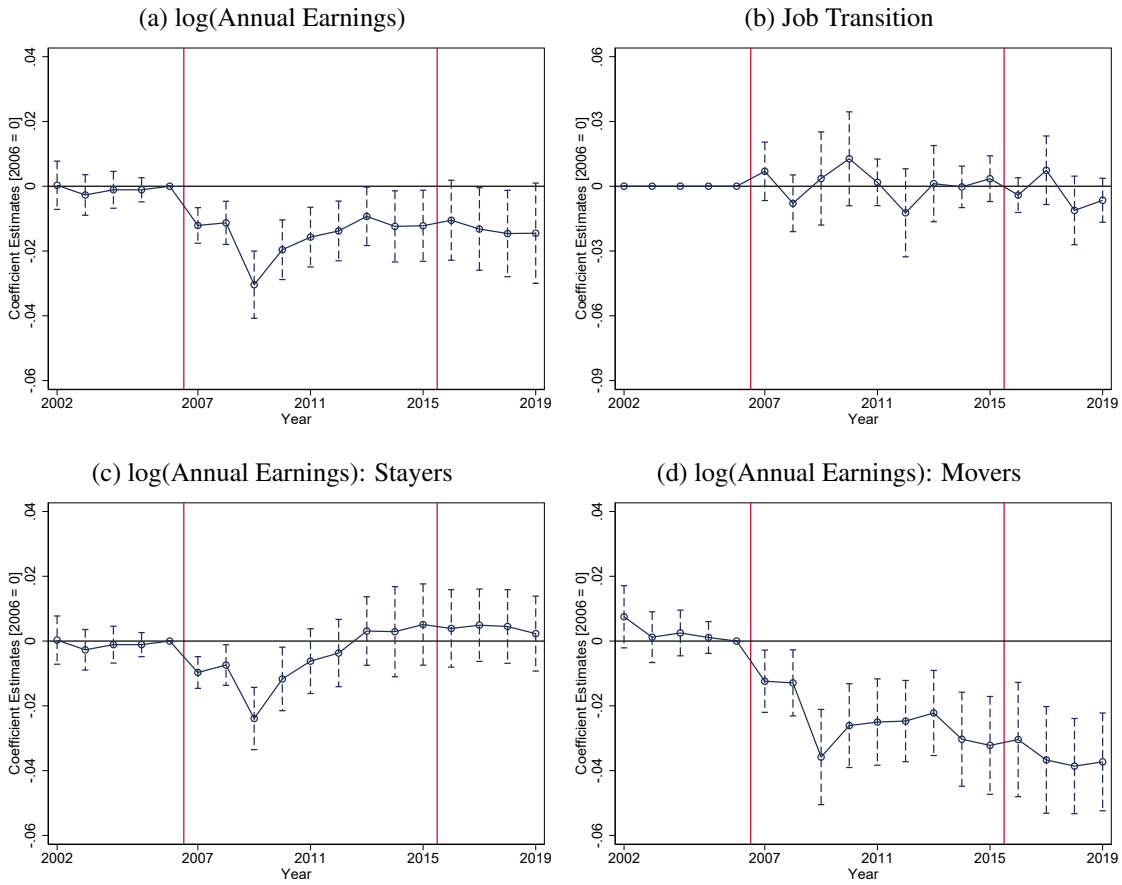
Notes: Panels (a) – (b) show the net present value (NPV) of one dollar of capital cost allowance (CCA) and the cost of capital, respectively, for machinery and equipment among treated firms in our sample. In 2007, the federal government increased the CCA rate for machinery and equipment from 30 percent to 50 percent and switched from the declining balance method to the straight-line rule. In 2016, the CCA rate remained at 50 percent, but switched back to the declining-balance method. Since firms may not claim CCA depending on their taxable income, 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 firms' claims for capital cost allowance scaled by their capital stock at the end of the year, averaged across asset classes by firms' expenditure share, as reported in firms' T2 Schedule 8. The changes in NPV and cost of capital after the reform are 10.1 percentage points and -1.1 percentage points on average, respectively.

Figure 2: Firms' Capital Investment, Employment, and Average Payrolls



Notes: Panels (a) – (d) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for firms' investment rate in ACCA-eligible assets (machinery or equipment), investment rate in 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.

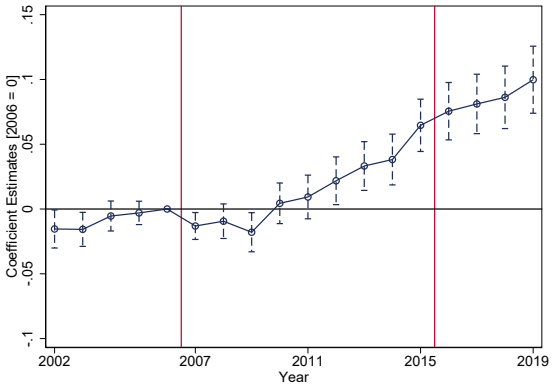
Figure 3: Workers' Earnings and Job Transition



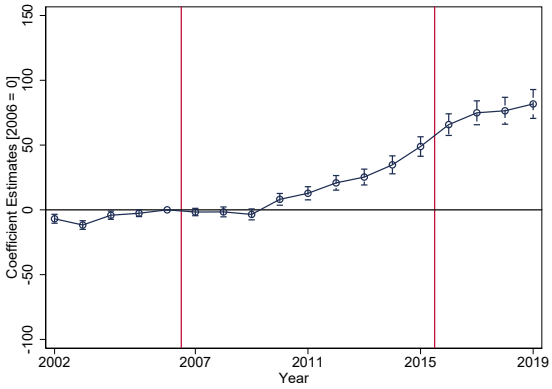
Notes: Panels (a) – (b) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (5) for workers' log(annual earnings) and job transition probabilities, respectively. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously 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 2007 and tracks them until they leave. Panel (d) shows the estimates on log(annual earnings) for workers who leave their pre-reform employers after 2007. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 4: Firms' Sales and Profitability

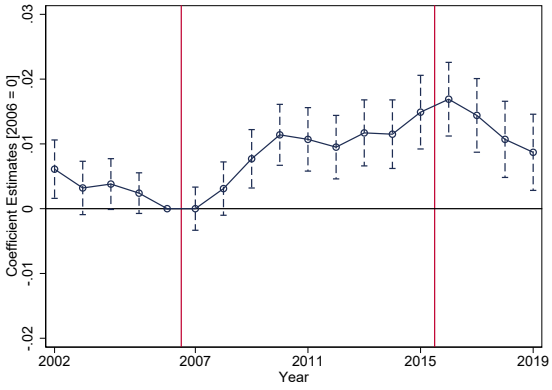
(a) log(Revenue)



(b) After-tax Profits

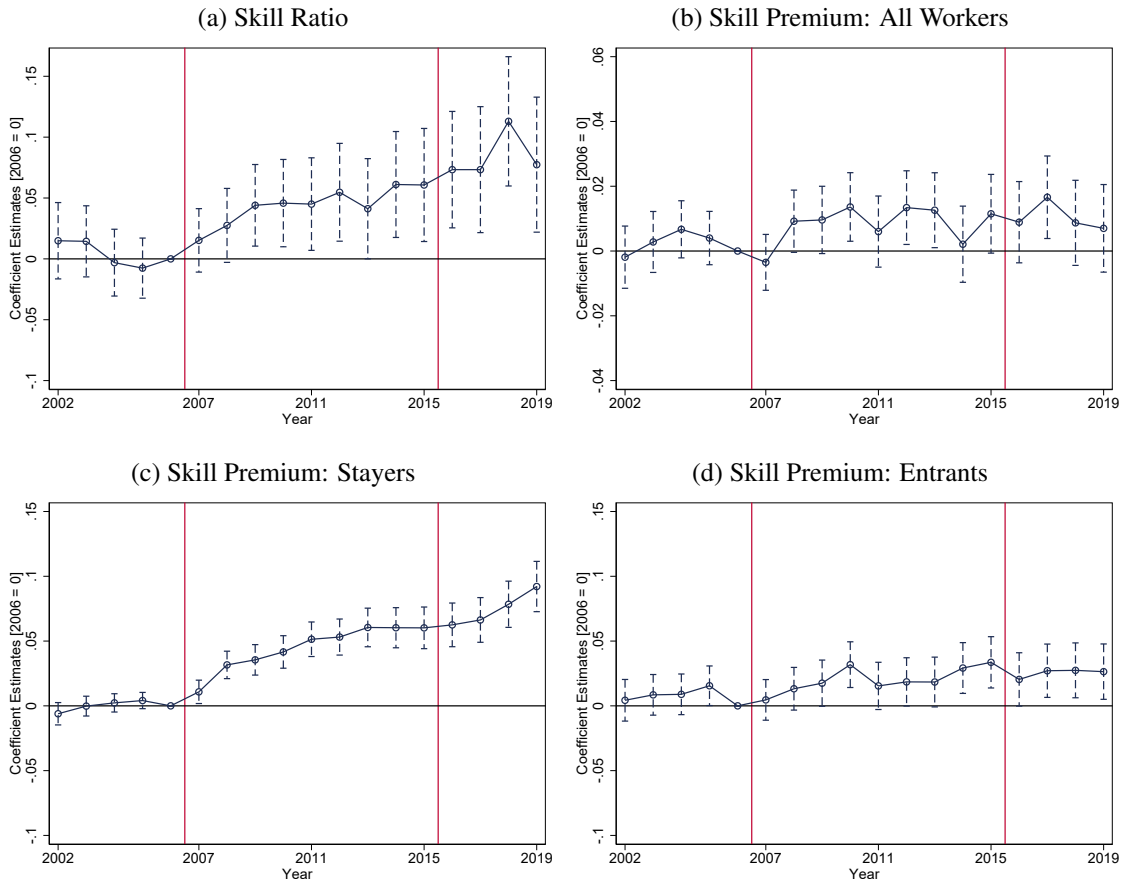


(c) Return on Assets



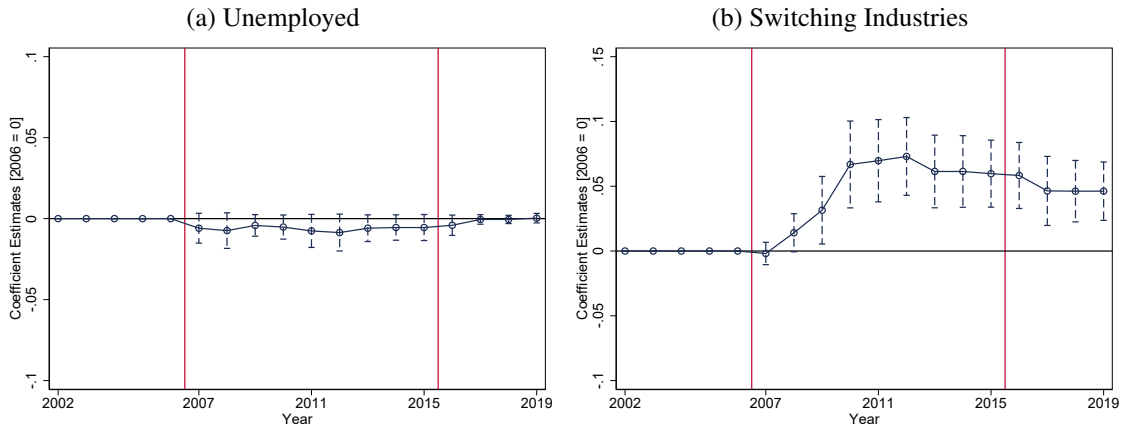
Notes: Panels (a) – (c) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (3) for firms' log(revenue), after-tax profits, and return on assets, respectively. After-tax profits are firms' net income after taxes and extraordinary items and measured in thousand CAD. Return on assets is net income scaled by total assets. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 5: Firms' Skill Ratio and Skill Premium



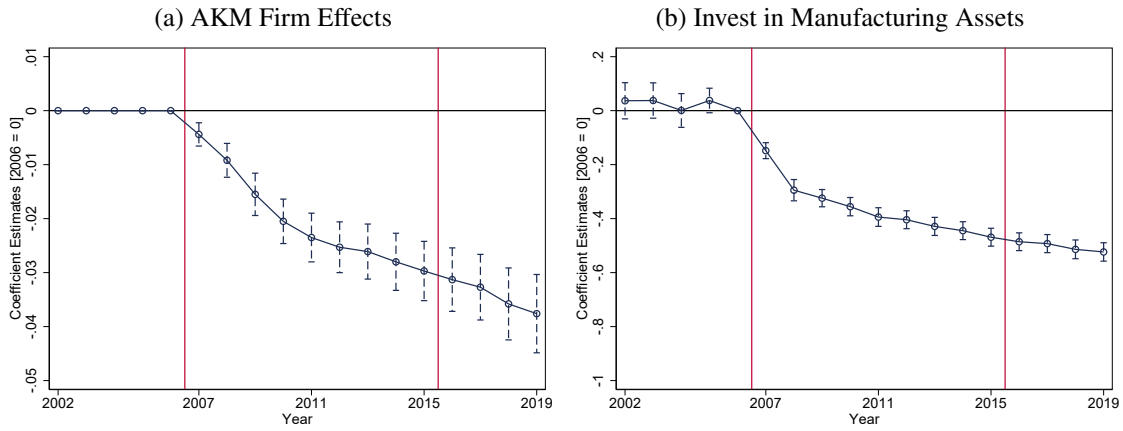
Notes: Panels (a) – (d) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times \textit{Treated}$ in equation (3) for the skill ratio and skill premium for all workers, stayers, and entrants, respectively, calculated using worker-level data and aggregated at the firm level. Skill ratio is the number of workers at the top 25 percentile of AKM worker fixed effects divided by the number of workers at the bottom 25 percentile in each firm. Skill premium is the 75th-25th percentile gap in workers' log(annual earnings) in each firm. The stayer sample consists of workers who stay with their pre-reform employers after 2007 and tracks them until they leave. 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 entrant sample consists of new workers in each firm and year. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 6: Job Movers Becoming Unemployed or Switching Industries



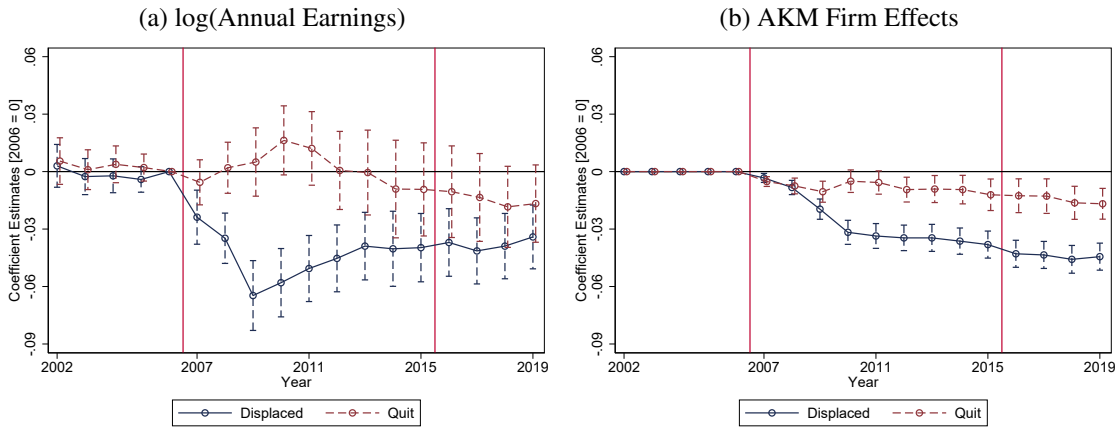
Notes: Panels (a) – (b) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (5) for whether a worker becomes unemployed or moves to different 4-digit NAICS industries from the ones in 2006, respectively. The sample consists of workers who leave their pre-reform employers after 2007. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 7: Firm Characteristics of Job Movers



Notes: Panels (a) – (b) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (5) for the AKM firm fixed effects and whether a firm invests in machinery or equipment, respectively, for the current employers of job movers. The sample consists of workers who leave their pre-reform employers after 2007. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

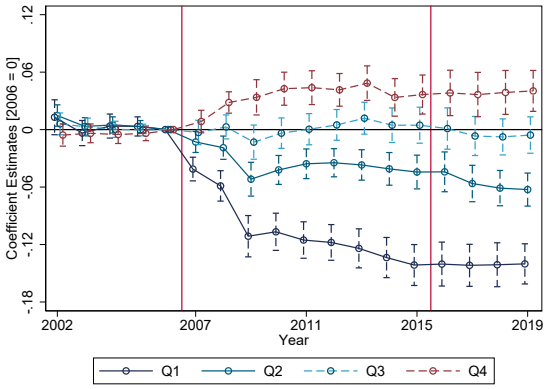
Figure 8: Heterogeneity by Type of Job Separations



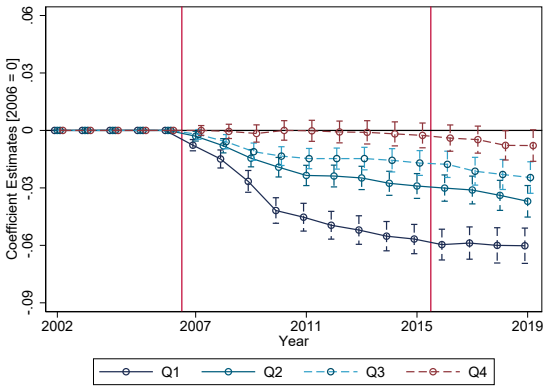
Notes: Panels (a) – (b) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (5) for workers' log(annual earnings) and AKM firm fixed effects of the current employers, respectively. The sample consists of workers who leave their pre-reform employers after 2007. Part-time 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. Workers with unknown reasons for job separation are excluded. We estimate equation (5) separately for workers who involuntarily leave their pre-reform employers ("Displaced") and who voluntarily leave ("Quit"). The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Figure 9: Heterogeneity by AKM Worker Fixed Effects

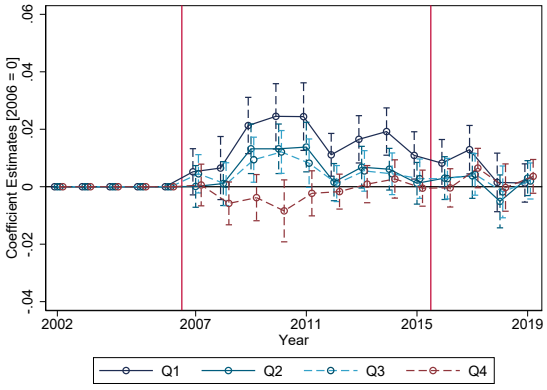
(a) log(Annual Earnings)



(b) AKM Firm Effects



(c) Displaced



Notes: Panels (a) – (c) show coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (5) for workers' log(annual earnings), AKM firm fixed effects of the current employers, and being involuntarily displaced, respectively. The sample consists of workers who leave their pre-reform employers 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. We estimate equation (5) separately for workers with AKM worker fixed effects in each quartile of the within-firm distribution in 2006. The solid vertical lines indicate the reform years. The dashed lines represent 95 percent confidence intervals.

Table 1: Descriptive Statistics on Firms and Workers

	Firm Sample		Worker Sample	
	(1)	(2)	(3)	(4)
	Treated	Control	Treated	Control
A. Firm Characteristics				
Tangible Assets ('000)	2,543.0	766.2		
Investment Rate	0.112	0.123		
Employment	39.7	17.9		
Average Payrolls ('000)	124.2	50.1		
Revenue ('000)	7,799.1	3,188.7		
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		
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
Number of Workers			400,675	791,735
C. Sectors				
Utility/Mining	0.006	0.008	0.062	0.022
Construction	0.041	0.149	0.016	0.131
Manufacturing	0.700	0.058	0.757	0.055
Wholesale Trade	0.088	0.087	0.070	0.106
Retail Trade	0.050	0.201	0.045	0.185
Transportation	0.004	0.045	0.003	0.122
Professional Services	0.026	0.122	0.017	0.117
Education/Healthcare	0.006	0.081	0.007	0.082
Other Services	0.079	0.249	0.023	0.181

Notes: Columns (1) – (2) and (3) – (4) 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 workers in 2006. Treated and control firms are defined, respectively, as those with and without investment in machinery or equipment during the pre-reform period (2002 – 2006). Industries (4-digit NAICS) without any treated or control firms are excluded. The worker sample consists of workers at treated or control firms in 2006. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. All variables are measured as averages in 2002 – 2006. Other services sector includes real estate, rental and leasing, management, administrative support, arts and entertainment, accommodation and food services.

Table 2: Firms' Capital Investment, Employment, and 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) – (4) report coefficient estimates on *Post* × *Treated* in equation (4) for firms' investment rate in ACCA-eligible assets (machinery or equipment), investment rate in 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: Workers' Earnings and Job Transition

	(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) – (2) report coefficient estimates on *Post* × *Treated* in equation (6) 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 stay with their pre-reform employers after 2007 and tracks them until they leave. Column (4) shows the estimates on log(annual earnings) for workers who leave their pre-reform employers after 2007. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD for columns (1) and (3) – (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: Firms' 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 $Post \times Treated$ in equation (4) for firms' log(revenue), after-tax profits, and return on assets, respectively. After-tax profits are firms' net income after taxes and extraordinary items. Return on assets is 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: Firms' Skill Ratio and Skill Premium

	(1) Skill Ratio	(2) Skill Premium: All Workers	(3) Skill Premium: Stayers	(4) Skill Premium: Entrants
Post × Treated	0.0441***	0.0058*	0.0477***	0.0123***
	(0.0126)	(0.0030)	(0.0044)	(0.0039)
Mean Dep. Var.	1.218	0.722	0.520	0.739
Observations	1,146,265	1,924,090	1,431,635	1,558,750
Treated Firms	14,080	15,470	12,795	15,395
Control Firms	112,905	140,425	98,770	136,345
Adjusted R^2	0.703	0.344	0.484	0.157

Notes: Column (1) – (4) shows coefficient estimates on $\mathbb{1}_{\{t=\tau\}} \times Treated$ in equation (4) for the skill ratio and skill premium for all workers, stayers, and entrants, respectively, calculated using worker-level data and aggregated at the firm level. Skill ratio is the number of workers at the top 25 percentile of AKM worker fixed effects scaled by the number of workers at the bottom 25 percentile in each firm. Skill premium is the 75th-25th percentile gap in workers' log(annual earnings) in each firm. The stayer sample consists of workers who stay with their pre-reform employers after 2007 and tracks them until they leave; 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 entrant sample consists of new workers in each firm and year. 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.

Table 6: Job Movers Becoming Unemployed or Switching Industries

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

Notes: Columns (1) – (3) report coefficient estimates on *Post* × *Treated* in equation (6) for whether a worker becomes unemployed or moves to different 4-digit NAICS industries from the ones in 2006, respectively. The sample consists of workers who leave their pre-reform employers after 2007. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. The mean for each dependent variable is based on years 2002 – 2006. All specifications include worker fixed effects and industry by year fixed effects. Standard errors are two-way clustered at the firm level and worker level and reported in parentheses.

Table 7: Firm Characteristics of Job Movers

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

Notes: Columns (1) – (3) report coefficient estimates on *Post* × *Treated* in equation (6) for the AKM firm fixed effects and whether a firm invests in manufacturing equipment, respectively, for the current employers of job movers. The sample consists of workers who leave their pre-reform employers after 2007. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. The mean for each dependent variable is based on years 2002 – 2006. All specifications include worker fixed effects and industry by year fixed effects. Standard errors are two-way clustered at the firm level and worker level and reported in parentheses.

Table 8: Heterogeneity by Type of Job Separations

	(1) log(Annual Earnings)	(2) AKM Firm Effects
Post × Treated (Quit)	-0.0050	-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) – (2) report coefficient estimates on *Post* × *Treated* in equation (6) for workers' log(annual earnings) and AKM firm fixed effects of the current employers, respectively. The sample consists of workers who leave their pre-reform employers 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. Workers with unknown reasons for job separation are excluded. We estimate equation (5) separately for workers who involuntarily leave their pre-reform employers ("Displaced") and who voluntarily leave ("Quit"). 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 AKM Worker Fixed Effects

	(1) log(Annual Earnings)	(2) AKM Firm Effects	(3) Displaced
Post × Treated (1st Quantile)	-0.1149***	-0.0417***	0.0127***
	(0.0088)	(0.0028)	(0.0024)
Post × Treated (2nd Quantile)	-0.0454***	-0.0218***	0.0054***
	(0.0059)	(0.0024)	(0.0020)
Post × Treated (3rd Quantile)	-0.0024	-0.0138***	0.0036*
	(0.0065)	(0.0024)	(0.0019)
Post × Treated (4th Quantile)	0.0393***	-0.0019	-0.0012
	(0.0083)	(0.0024)	(0.0019)
Mean Dep. Var. (1st Quantile)	34.2	0.160	0.000
Mean Dep. Var. (2nd Quantile)	41.8	0.138	0.000
Mean Dep. Var. (3rd Quantile)	50.0	0.139	0.000
Mean Dep. Var. (4th Quantile)	74.1	0.128	0.000
Observations	8,705,060	8,001,105	8,295,650
Treated Workers	199,555	199,275	199,275
Control Workers	347,805	347,015	347,020
Adjusted R^2	0.646	0.805	0.072

Notes: Columns (1) – (3) report coefficient estimates on *Post* × *Treated* in equation (6) for workers' log(annual earnings), AKM firm fixed effects of the current employers, and being displaced, respectively. The sample consists of workers who leave their pre-reform employers 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. We estimate equation (5) separately for workers with AKM worker fixed effects in each quartile of the within-firm distribution in 2006. 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.

ONLINE APPENDIX:

Manufacturing Investment and Employee Earnings: Evidence from Accelerated Depreciation

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A Additional Descriptive Statistics and Analyses

In Appendix A, we provide further descriptive statistics of our data and auxiliary analyses related to our main findings.

A.1 Additional Descriptive Statistics

Figure A1 describes the distribution of treated and control firms, respectively, in the top 10 industries in our analysis sample. For example, the "printing" industry (NAICS 3231) accounts for the largest share of firms in the treated group, while the "building equipment contractor" industry (NAICS 2382) accounts for the largest share in the control group.

Relatedly, Figure A2 shows the distribution for manufacturing and non-manufacturing sectors separately. Within the manufacturing sector, the "printing" industry accounts for the largest share of firms among both treated and control groups. Within non-manufacturing sectors, the "grocery store" industry (NAICS 4451) accounts for the largest share of treated firms, while the "building equipment contractor" industry (NAICS 2382) accounts for the largest share of control firms. Among the top five industries in the manufacturing sector, three industries overlap between the treated group and the control group: "printing", "furniture and kitchen cabinet" (NAICS 3371), and "architectural and structural metal" (NAICS 3323). Among the top five industries in non-manufacturing sectors, two industries overlap: "grocery stores" and "restaurants" (NAICS 7221).

Figure A3 shows the share of firms exiting the sample in each year, separately for the treated group and the control group. As mentioned in Section 3, firms in our analysis sample are required to be active and have at least five employees in 2006, before which the exit rate is mechanically zero. We find that the exit rates increased after 2006 for both treated and control groups by a similar magnitude. Therefore, the reform did not lead treated or control firms to disproportionately exit the market, which could generate a sample selection issue.

Table A1 describes the federal and provincial corporate income tax rates from 2002 to 2008. Alberta, British Columbia, Ontario, and Quebec experienced some changes in the general or small business tax rates over this period. There were also changes after 2008, which can be found in Appendix A of Duan and Moon (2024). Importantly, these changes in tax rates would affect both the treated and control groups, and their impacts would be reflected on the estimated changes in the cost of capital. In Appendix B, we show robust estimates including other provinces besides Alberta, British Columbia, Ontario, and Quebec.

Table A2 shows descriptive statistics for firms and workers in our analysis sample, separately for manufacturing and non-manufacturing sectors. Consistent with Table 1, there are differences

in firm sizes (i.e., tangible assets, employment, average payrolls, and revenue) between treated and control firms in both manufacturing and non-manufacturing sectors. However, differences in leverage ratio and retained earnings (scaled by total assets) are more pronounced in the manufacturing sector, consistent with the idea that manufacturing firms in the control group likely rent machinery or equipment due to cash constraints. Otherwise, treated and control firms are still comparable in terms of investment rate, profit margins, and firm age.

Table A3 shows descriptive statistics for firms and workers in the matched samples, on which we conduct robustness tests (see Appendix B.3). On the firm side, we use exact matching by firms' industry and coarsened exact matching (CEM, Iacus et al., 2012) by firms' total assets, revenue, and employment, in the two years before the reform. On the worker side, we use exact matching by industry and propensity score matching (PSM) by workers' demographics (i.e., age) in the year before the reform. Note that the original worker sample imposes the tenure restriction, so matching by an earlier year does not provide additional information. In addition, we do not use CEM for worker-level matching because it is computationally demanding due to the large sample size. Table A3 shows that treated and control firms become significantly more comparable after matching, and so do workers' pre-reform earnings.

Table A4 describes the type of assets that treated and control firms invest in prior to the reform. On average, treated and control firms have similar shares of buildings relative to their total assets. Control firms in the manufacturing sector invest a larger share in buildings, whereas treated firms in non-manufacturing sectors invest a larger share in buildings. Furthermore, control firms have a much larger share of assets in computers, data network infrastructure equipment, and systems software, while having a zero share in machinery or equipment by our construction.

A.2 AKM Assumptions

Figure A4 provides supportive evidence for the exogenous mobility assumption of the AKM model. Following Card et al. (2013b), we plot the earnings trajectories of movers between high-paying firms and low-paying firms. The sample consists of job movers with at least three years of job tenure at the same firm before they move, and at least two years of job tenure at the same firm after they move. Part-time workers with annual earnings below 4,000 CAD and multiple job holders are excluded. Consistent with the worker-level analysis, we associate each worker with a "dominant employer" with the highest annual income in a given year. For job movers, we define year $t = -1$ as the last year a worker is linked to the previous employer and $t = 0$ as the first year the worker is linked with the new employer. Then for each worker in the sample, we use average log(annual earnings) among their coworkers as a proxy for firm-specific wage premiums.

For workers moving out of firms at the top quartile of earnings premiums, their annual earnings followed parallel trends up to year $t = -2$, regardless of the earnings premiums at their destination firms (see solid lines). Similarly, workers moving out of firms at the bottom quartile of earnings premiums see parallel trends in their earnings until year $t = -2$ (see dashed lines). A potential issue with the earnings data is that the exact date of moving is unobserved; i.e., a worker may start on her new job in either year $t - 1$ or year $t = 0$. Hence, workers moving from high-paying to low-paying firms may exhibit an early decline in their earnings in year $t = -1$. Nevertheless, these non-parallel earnings trajectories do not rule out exogenous mobility. In fact, such early responses are also observed in the job displacement literature, where workers' annual earnings start dropping during the calendar year when job displacement takes place (e.g., Schmieder et al., 2023).

Furthermore, the AKM model is identified by workers moving across firms, so precise estimates of the firm and worker fixed effects require sufficient worker mobility. Across the entire sample, 84 percent of workers make at least one move to a different firm during our sample period, and the average number of job movers per firm is about 8. Therefore, the mobility in our sample is high and comparable to the ones observed in Lachowska et al. (2020) and Arnold et al. (2024).

A.3 Additional Analyses

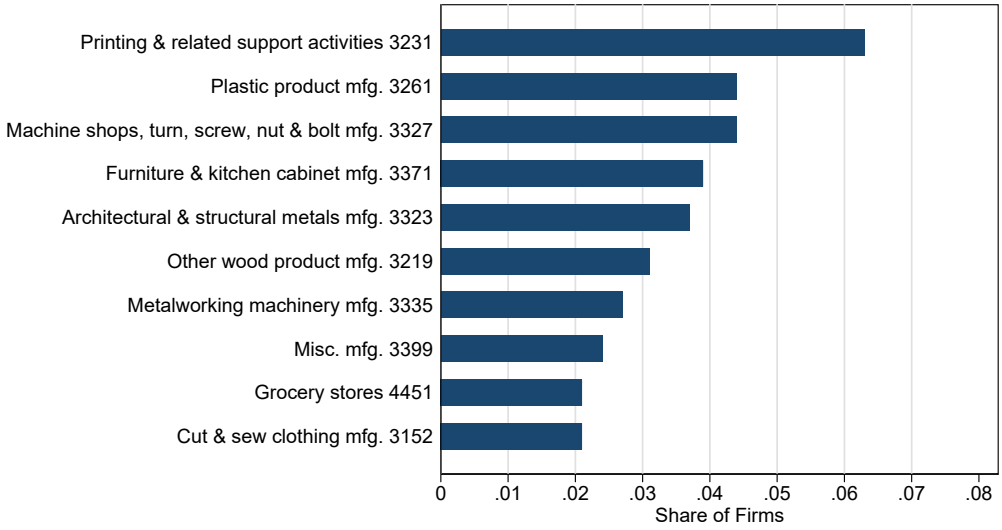
In this subsection, we report several analyses in addition to our main findings. First, we examine firms' investment in other asset classes affected by the 2007 accelerated deprecation policy, which could potentially confound our findings. In Table A5, columns (1) – (2) show changes in the net-present value of one dollar of capital cost allowance and the cost of capital for investment in buildings, for treated firms relative to control firms, after the reform. Columns (3) – (4) show the same estimates for investment in computers and software. Analogous to Figure 1, 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. For treated firms relative to control firms, the estimated changes in the net-present value for buildings and computers is less than one tenth of the change in the NPV for machinery and equipment, and changes in the cost of capital for these two asset classes are also much smaller relative to the change in the cost of capital for machinery and equipment. This is because control firms also invest in buildings and computers and would therefore benefit from accelerated depreciation on these asset classes. These results suggest that, in our setting, the impacts of ACCA on other asset classes (buildings and computers) are at most marginal in the difference-in-differences framework.

Moreover, we rule out two alternative explanations for firms' responses to the reform. First, there was a coinciding accelerated depreciation policy in 2006 targeting firms' investment in clean

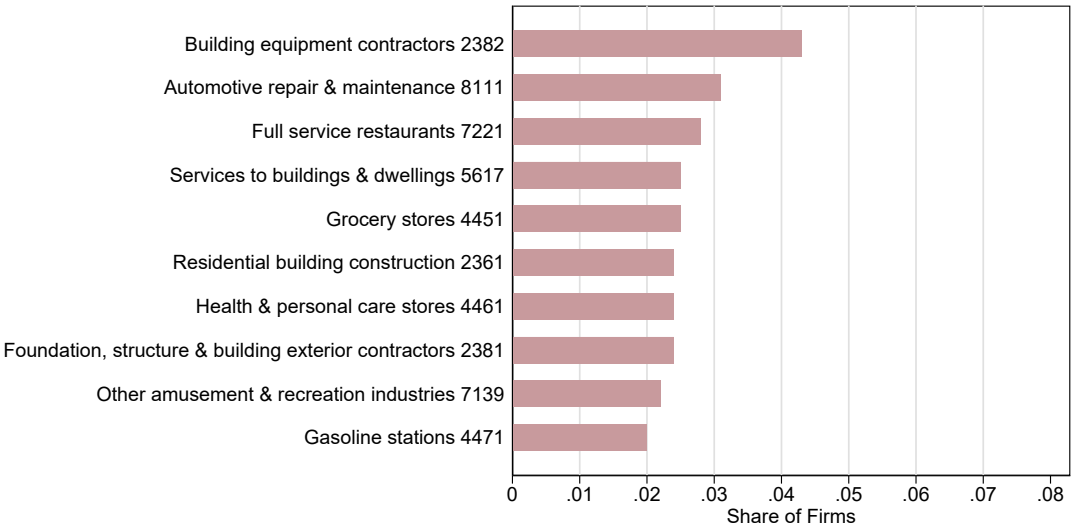
technologies. Second, our results could be driven by automation, where firms replace workers by industrial robots. While it is possible that firms shifting to clean technologies or industrial robots change their demand for labor (especially toward skilled workers), we find that very few firms invest in clean technologies or robots either before or after the reform, and the aggregated value of investment is minimal relative to that of machinery or equipment. As shown in columns (1) – (2) of Table A6, there was no significant change in firms' investment in clean technologies, either in the extensive or the intensive margin. Columns (3) – (4) show similar results for investment in industrial robots.

Figure A1: Distribution of Industries

(a) Treated Firms



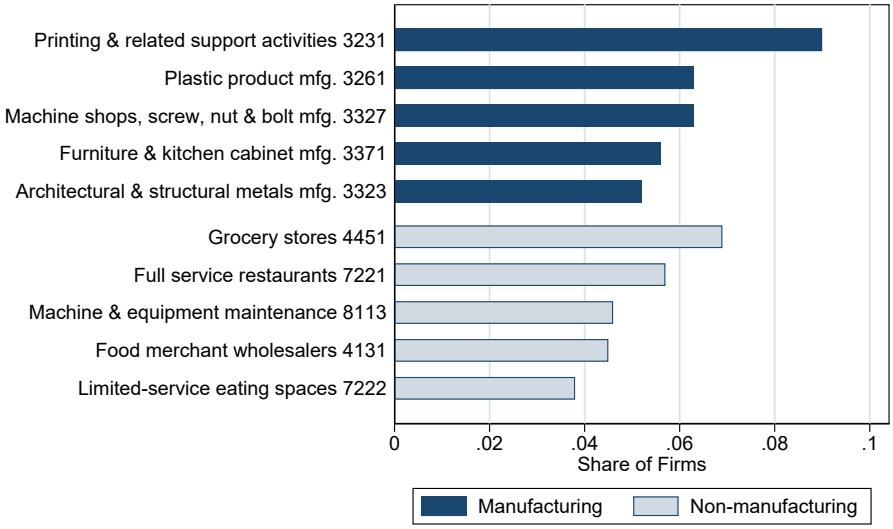
(b) Control Firms



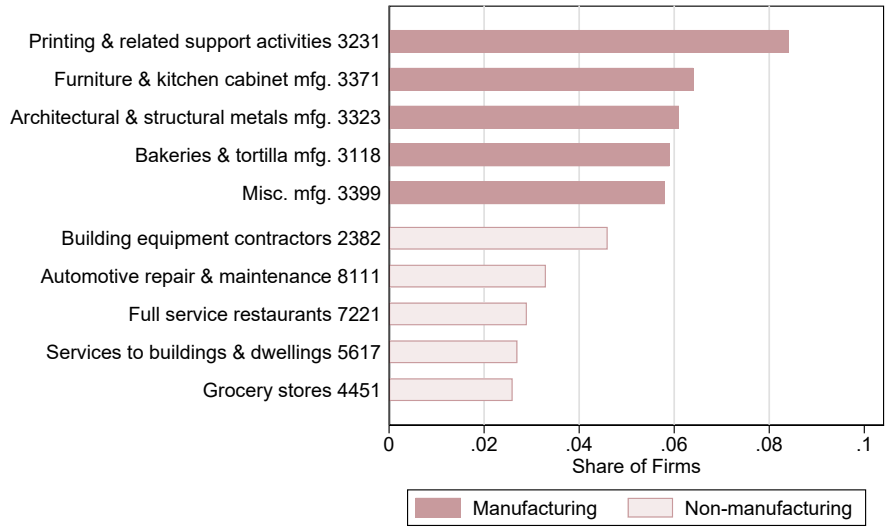
Notes: Panels (a) – (b) show the distribution of firms in the top 10 industries (4-digit NAICS) in treated and control groups, respectively. The sample consists of CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec with at least five workers in 2006. Treated and control firms are defined, respectively, as those with and without investment in machinery or equipment during the pre-reform period (2002 – 2006). Industries without treated or control firms are excluded.

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

(a) Treated Firms

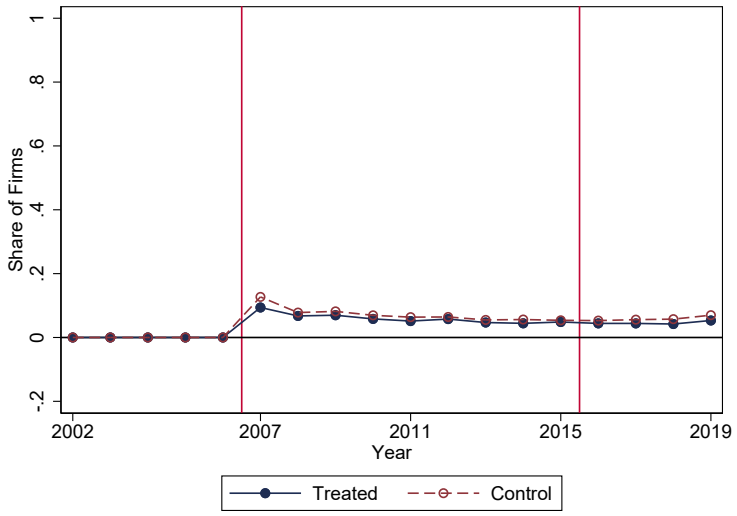


(b) Control Firms



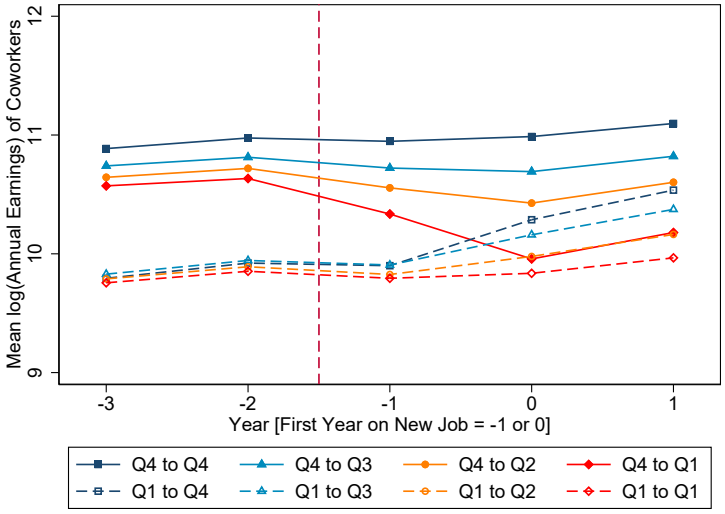
Notes: Panels (a) – (b) show the distribution of firms in the top five industries (4-digit NAICS) in treated and control groups, by manufacturing and non-manufacturing sectors, respectively. The sample consists of CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec with at least five workers in 2006. Treated and control firms are defined, respectively, as those with and without investment in machinery or equipment during the pre-reform period (2002 – 2006). Industries without treated or control firms are excluded.

Figure A3: Share of Firms Exiting the Sample



Notes: The figure shows the number of firms exiting our analysis sample by year, scaled by the number of firms in the previous year. The dark blue lines represent treated firms and the dashed red lines represent control firms. The sample consists of CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec with at least five workers in 2006. Treated and control firms are defined, respectively, as those with and without investment in machinery or equipment during the pre-reform period (2002 – 2006). Industries without treated or control firms are excluded.

Figure A4: The Exogenous Mobility Assumption



Notes: The figure plots the average log(annual earnings) of job movers, in the three years before moving and two years after moving. The sample consists of job movers with at least three years of job tenure at the same firm before they move, and at least two years of job tenure at the same firm after they move. Part-time workers with annual earnings below 4,000 CAD and multiple job holders are excluded. For each worker in the sample, we use the average log(annual earnings) of their coworkers as a proxy for the firm-specific earnings premiums. Since the exact date of moving is unobserved in the yearly data, a worker may start on her new job in either year -1 or year 0. The solid and dashed lines represent, respectively, job movers from firms in the highest and lowest quartile of earnings premiums to firms in each quartile. Annual earnings are measured using the 2006 prices.

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

Notes: The table reports Canadian federal corporate income tax rates and provincial corporate income tax rates in four major provinces (Alberta, British Columbia, Ontario, and Quebec) for manufacturing and non-manufacturing firms, from 2002 to 2008. General and small business rates apply to CCPCs (domestic firms) with taxable capital below and above 10 million CAD, respectively. See Duan and Moon (2024) for details of the corporate income tax system and additional tax rates after 2008.

Table A2: Descriptive Statistics on Firms and Workers

	Manufacturing		**Non-manufacturing**	
	(1)	(2)	(3)	(4)
	Treated	Control	Treated	Control
A. Firm Characteristics				
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. Worker Characteristics				
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 – B report summary statistics for firms and workers in our analysis sample, respectively. Columns (1) – (2) report the statistics for the manufacturing sector, and 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 workers in 2006. Treated and control firms are defined, respectively, as those with and without investment in machinery or equipment during the pre-reform period (2002 – 2006). Industries (4-digit NAICS) without treated or control firms are excluded. The worker sample consists of workers at treated or control firms in 2006. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. All variables are measured as averages in 2002 – 2006. Other services sector includes real estate, rental and leasing, management, administrative support, arts and entertainment, accommodation and food services.

Table A3: Descriptive Statistics on Matched Firms and Workers

	Firm Sample		**Worker Sample**	
	(1)	(2)	(3)	(4)
	Treated	Control	Treated	Control
A. Firm Characteristics				
Tangible Assets ('000)	744.3	535.5		
Investment Rate	0.112	0.113		
Employment	16.0	15.0		
Average Payrolls ('000)	42.1	38.4		
Revenue ('000)	2,207.5	1,989.8		
After-tax Profits ('000)	57.7	49.1		
Profit Margins	0.037	0.032		
Leverage Ratio	0.673	0.714		
Retained Earnings / Assets	0.271	0.243		
Firm Age	10.7	10.4		
Number of Firms	7,405	7,405		
B. Worker Characteristics				
Annual Earnings ('000)			47.2	48.7
Male			0.429	0.640
Age			44.9	44.2
Number of Workers			122,680	122,680
C. Sectors				
Utility/Mining	0.006	0.006	0.103	0.103
Construction	0.066	0.066	0.052	0.052
Manufacturing	0.573	0.574	0.352	0.352
Wholesale Trade	0.098	0.099	0.201	0.201
Retail Trade	0.073	0.072	0.147	0.147
Transportation	0.005	0.005	0.008	0.008
Professional Services	0.040	0.039	0.044	0.044
Education/Healthcare	0.010	0.010	0.018	0.018
Other Services	0.131	0.130	0.075	0.075

Notes: Columns (1) – (2) and (3) – (4) report summary statistics for firms and workers in our matched analysis sample, respectively. The original firm sample consists of CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec with at least five workers in 2006. Treated and control firms are defined, respectively, as those with and without investment in machinery or equipment during the pre-reform period (2002 – 2006). Industries (4-digit NAICS) without treated or control firms are excluded. We match treated and control firms using exact matching by industry and coarsened matching by firms' total assets, employment, and revenue in 2005 and 2006. While we only show that sectoral compositions are similar between treated firms and control firms for this matched sample, we find that 4-digit industry compositions are also similar between these two groups and we can provide these descriptive statistics upon request. The original worker sample consists of workers at treated or control firms in 2006. Part-time workers with annual earnings below 4,000 CAD, multiple-job holders, and workers who are not continuously employed by the same firm in 2002 – 2006 are excluded. We match treated and control workers using exact matching by industry and nearest-neighbor propensity score matching by workers' demographics (i.e., age) in 2006. 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 A4: Pre-reform Investment Share by Asset Classes

	All		**Manufacturing**		**Non-manufacturing**	
	(1)	(2)	(3)	(4)	(5)	(6)
	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/ 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 firms' expenditure shares in each asset classe in 2002 – 2006. The sample consists of CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec with at least five workers in 2006. Treated and control firms are defined, respectively, as those with and without investment in machinery or equipment during the pre-reform period (2002 – 2006). Industries (4-digit NAICS) without treated or control firms are excluded. Columns (1) – (2) include all firms in our analysis sample. Columns (3) – (4) focus on firms in the manufacturing sector, and columns (5) – (6) focus on firms in non-manufacturing sectors. Miscellaneous assets are investment with missing asset class information or not belonging to any of the classes listed in the table.

Table A5: 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) – (2) report coefficient estimates on *Post* × *Treated* in equation (4) for firms' net present value of one dollar of capital cost allowance and the cost of capital, respectively, for investment in buildings. Columns (3) – (4) show 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, as reported in firms' T2 Schedule 8. 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.

Table A6: Investment in Clean Technologies and Industrial Robots

	(1) Any Investment in Clean Tech	(2) Investment in Clean Tech	(3) Any Investment in Robots	(4) Investment in Robots
Post × Treated	0.0087	-2.4014	0.0005*	-0.0495
	(0.0054)	(4.5362)	(0.0003)	(0.0484)
Mean Dep. Var.	0.001	4.1	0.003	0.5
Observations	1,956,395	1,956,395	1,956,395	1,956,395
Treated Firms	15,480	15,480	15,480	15,480
Control Firms	141,330	141,330	141,330	141,330
Adjusted R^2	0.010	0.361	0.275	0.349

Notes: Columns (1) – (2) report coefficient estimates on *Post* × *Treated* in equation (4) for whether a firm invests in clean technologies in a given year and the value of clean technology investment, respectively. Columns (3) – (4) report estimates for whether a firm invests in industrial robots in a given year and the value of robot investment, respectively. The mean for each dependent variable is based on years 2002 – 2006 and measured in thousand CAD for columns (2) and (4). All specifications include firm fixed effects and industry by year fixed effects. Standard errors are clustered at the firm level and reported in parentheses.

B Robustness Checks and Internal Validity

In Appendix B, we present robustness tests mentioned in Sections 3 – 5. We confirm that our estimates are robust to different model specifications, sample restrictions, and definitions of treated and control groups (Appendices B.1 and B.2). We also rule out concerns that our estimates are confounded by different characteristics of treated and control firms (and workers) prior to the reform (Appendix B.3) or spillover effects between treated and control groups due to input or product market competition (Appendices B.4 to B.6).

B.1 Different Specifications

In our main specification, we include industry by year fixed effects to account for unobserved shocks at the industry level that might confound our results. In Table B1, we include sector by year fixed effects instead to control for time-varying sector-specific shocks. In Table B2, we additionally include commuting zone by year fixed effects to absorb unobserved shocks at the local labor market level. As shown in the tables, our estimates are robust to controlling for different fixed effects.

In our main analysis sample, we include firms and workers in Alberta, British Columbia, Ontario, and Quebec, which jointly account for 90 percent of all firms in Canada. We also exclude firms in agriculture and finance sectors. Alternatively, Table B3 includes all provinces and territories in Canada, and Table B4 includes the excluded sectors. In both cases, the estimates are similar to our main results.

Additionally, Table B5 shows separate estimates for firms and workers in the manufacturing sector versus non-manufacturing sectors. Table B6 excludes those in the machinary manufacturing industry (NAICS 333). We find qualitatively similar results for both subsamples, implying that our results are not driven by firms and workers in the manufacturing sector or the machinary manufacturing industry alone.

In the worker-level analysis, we impose tenure restrictions and drop multiple-job holders and those making below 4,000 CAD in annual earnings. In Table B7, we relax these restrictions and include those workers. While this greatly increases the sample size, the estimated effect on worker earnings is qualitatively similar to (if anything, larger in magnitude than) our main estimate.

Furthermore, Table B8 reports worker-level estimates focusing on job movers who leave their pre-reform employers within the first three years after the reform (2007 – 2009). After 2009, workers are more likely to move to other firms due to other reasons than firms' responses to the reform, so including such workers could complicate the interpretation of our results. We find that about a half of job movers actually moved within the first three years after the reform, and the

estimate on these workers is quite similar to the estimate using all job movers (Table 3).

B.2 Redefining Treated and Control Groups

In our main specification, we define treated firms as those with positive investment in machinery or equipment prior to the reform, while control firms either rented, outsourced, or never bought any machinery or equipment. A potential concern with this definition is that treated and control firms may not be comparable and exposed to differential shocks that may bias our estimates. As such, we redefine treated and control firms as those with the share of investment in machinery or equipment above and below 30 percent of total investment, respectively. In other words, we allow control firms to have positive investment in manufacturing assets, but with a smaller share than treated firms. Table B9 shows that the results based on this new definition are qualitatively similar to our main findings.

B.3 Matched Samples

To further address the concern that treated and control firms (or workers) may not be comparable, we estimate equations (3) – (4) using a matched sample of firms and equations (5) – (6) using a matched sample of workers (see Table A3 in Appendix A for descriptive statistics on the matched samples). After matching, treated firms become more comparable to control firms in terms of their observable characteristics. Furthermore, treated workers and control workers also share similar average earnings prior to the reform. Table B10 shows that the estimates are similar to our main findings. Therefore, the estimated changes in both firm-level and worker-level outcomes are unlikely confounded by differences in their pre-reform characteristics. In particular, the earnings decline of treated workers relative to control workers is not driven by mean reversion.

B.4 Worker Reallocation

We examine whether there was reallocation of workers across firms after the reform, and how much this reallocation can explain our results on employment. This analysis implicitly assumes that treated firms and control firms compete in the same labor market. 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.

In Figure B1, panel (a) plots the gross rates of outflow from treated firms to (i) other treated

firms, (ii) control firms, (iii) out-of-sample firms (e.g., in other provinces or in excluded sectors), and (iv) non-employment (e.g., unemployment, retirement, leaving Canada). The gross rates are computed as the number of job movers to each destination scaled by the total number of workers at treated firms in the year before. Panel (b) plots the corresponding net outflow rates, defined as the net loss of workers from treated firms to each destination scaled by the total number of workers at treated firms in the year before. We find that the gross outflow from treated firms to other treated firms and control firms remained flat, and the net outflow from treated to control firms stayed around zero, both before and after the reform. Panels (c) – (d) repeat the analysis for gross and net outflows from control firms, respectively. We confirm that the flow of workers between treated and control firms hardly responded to the reform. Overall, these results indicate that our estimate on employment is not driven by worker reallocation between treated firms and control firms.

B.5 Changes in the Price of Capital

In Section 4, we show that ACCA increased the demand for machinery or equipment by treated firms. Since control firms (especially those in the manufacturing sector) may rent machinery or equipment, they could also be indirectly affected by ACCA if the prices for machinery or equipment changed after the reform. To rule out this concern, we show that the prices of manufacturing assets follow similar trends relative to those of other assets both before and after the reform.

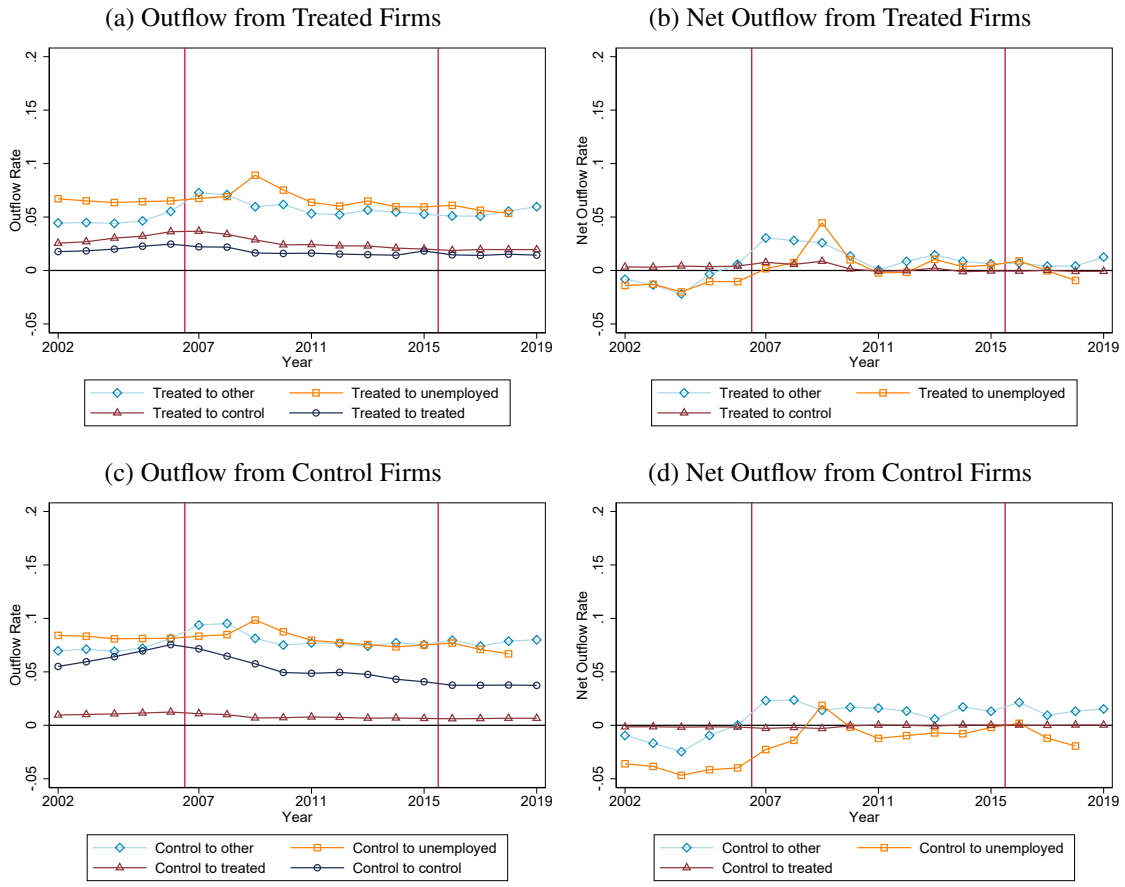
In Figure B2, Panel (a) compares the output price (industrial producer price index, IPPI) from the machinery manufacturing industry (NAICS 333) against the output price from other industries in the manufacturing sector. Panel (b) compares the input (purchase) price index for machinery and equipment in the manufacturing sector relative to non-manufacturing sectors. Both indices measure the price of capital, and they do not change much for manufacturing assets relative to non-manufacturing assets after the reform. These results imply that the rental price for machinery and equipment is likely unaffected by the reform.

B.6 Spillover Effects

To further rule out potential spillover effects of ACCA on control firms, we exploit the across-industry variation in the share of treated firms and workers to conduct a heterogeneity analysis. The intuition is that, if more firms and workers are treated in a given industry, the remaining control firms and workers would be more likely to be affected by spillover effects due to competition in input or product markets. In Table B11, we find that the effects of ACCA on our key outcomes do not differ much between industries with a higher share of treated firms or workers and industries

with a lower share. Overall, these results suggest that our estimates are unlikely contaminated by potential spillover effects of ACCA on control firms and workers.

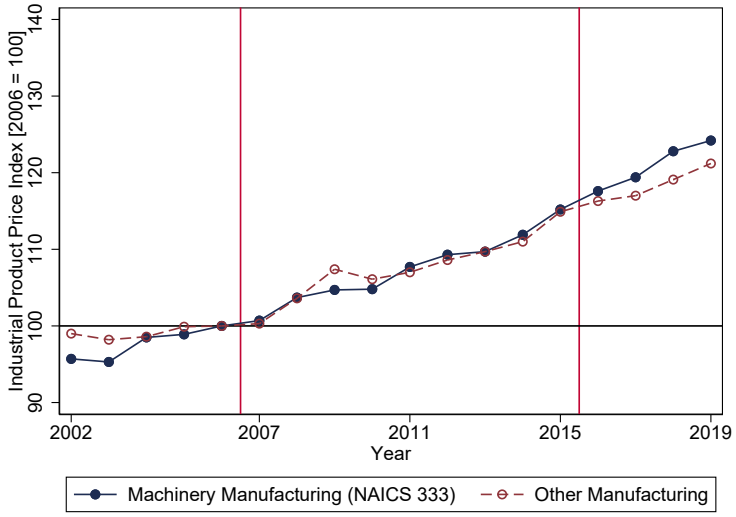
Figure B1: Worker Reallocation



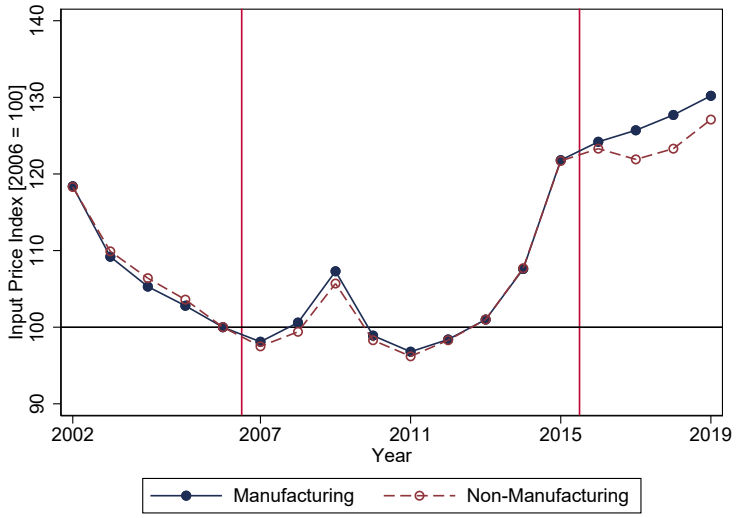
Notes: Panel (a) shows the outflow rates of workers by year, from treated firms to (i) other treated firms, (ii) control firms, (iii) firms excluded from the sample, and (iv) unemployment. Outflow rates are the number of workers moving from treated firms to destination firms, scaled by the total number of workers at treated firms in the previous year. Panel (b) shows corresponding net outflow rates, i.e., the number of workers moving from treated firms to destination firms net of those moving in the opposite directions, scaled by the total number of workers at treated firms in the previous year. The net outflow to treated firms is zero by definition and thus omitted. Panels (c) – (d) repeat panels (a) – (b) for outflow and net outflow rates from control firms. The net outflow to control firms is omitted. The firm sample consists of CCPCs (domestic firms) in Alberta, British Columbia, Ontario, and Quebec with at least five workers in 2006. Treated and control firms are defined, respectively, as those with and without investment in machinery or equipment during the pre-reform period (2002 – 2006). Industries (4-digit NAICS) without any treated or control firm are excluded.

Figure B2: Changes in the Price of Capital

(a) Industrial Producer Price Index (IPPI)



(b) Purchase Price Index of machinery or 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 shows the IPPI for other manufacturing industries. In Panel (b), the dark blue line shows the purchase price index of machinery or equipment for the manufacturing sector (NAICS 31-33) and the red line shows the index 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 (6) 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) – (3) report coefficient estimates on *Post* × *Treated* in equation (4) 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) – (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) – (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 (6) 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) – (3) report coefficient estimates on *Post* × *Treated* in equation (4) 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) – (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) – (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 (6) 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) – (3) report coefficient estimates on *Post* × *Treated* in equation (4) for firms' log(employment) and investment rate, respectively. The sample consists 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) – (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) – (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 (6) 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) – (3) report coefficient estimates on *Post* × *Treated* in equation (4) for firms' log(employment) and investment rate, respectively. The sample consists 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) – (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) – (3), and reported in parentheses.

Table B5: Manufacturing versus Non-manufacturing Sectors

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
A. 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
B. 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: Column (1) reports coefficient estimates on *Post* × *Treated* in equation (6) 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) – (3) report coefficient estimates on *Post* × *Treated* in equation (4) for firms' log(employment) and investment rate, respectively. The samples comprise workers and firms in the manufacturing sector. 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) – (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) – (3), and reported in parentheses. Columns (4) – (6) repeat columns (1) – (3) using workers and firms in non-manufacturing sectors.

Table B6: Excluding Machinary Manufacturing Industry

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
Post × Treated	-0.0152***	0.0429***	0.0131***
	(0.0041)	(0.0063)	(0.0021)
Observations	17,435,250	1,932,295	1,817,240
Mean Dep. Var.	51.8	39.7	0.113
Treated Workers/Firms	380,610	14,360	13,845
Control Workers/Firms	787,395	140,635	125,505
Adjusted R^2	0.702	0.820	0.144

Notes: Column (1) reports coefficient estimates on *Post* × *Treated* in equation (6) 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) – (3) report coefficient estimates on *Post* × *Treated* in equation (4) for firms' log(employment) and investment rate, respectively. All estimates exclude firms and workers in the machinary manufacturing industry (NAICS 333). The sample consists 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) – (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) – (3), and reported in parentheses.

Table B7: 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 (6) 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 B8: Movers within the First Three Years

	(1)
	log(Annual Earnings)
Post × Treated	-0.0238***
	(0.0066)
Observations	4,353,570
Mean Dep. Var.	48.8
Treated Workers	94,920
Control Workers	182,835
Adjusted R^2	0.158

Notes: The table reports coefficient estimates on $Post \times Treated$ in equation (6) for workers' log(annual earnings). The sample consists of job movers who leave their pre-reform employers in 2007 – 2009. 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. Among movers from treated firms, 16.7 percent, 15.8 percent, and 11.8 percent moved in 2007, 2008, and 2009, respectively. Among movers from control firms, 17.8 percent, 15.9 percent, and 11.9 percent moved in 2007, 2008, and 2009, respectively. 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 B9: Alternative Definition of Treated Firms and 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 (6) 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) – (3) report coefficient estimates on *Post* × *Treated* in equation (4) for firms' log(employment) and investment rate, respectively. Treated and control firms are defined, respectively, as those with investment in machinery or equipment above or below 30 percent of total investment in 2006. 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) – (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) – (3), and reported in parentheses.

Table B10: Matched Sample of Firms and Workers

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment Rate
Post × Treated	-0.0208***	0.0372***	0.0081***
	(0.0046)	(0.0092)	(0.0030)
Mean Dep. Var.	47.2	16.0	0.112
Observations	3,589,425	197,365	190,280
Treated Workers/Firms	122,680	7,405	6,970
Control Workers/Firms	122,680	7,405	6,795
Adjusted R^2	0.716	0.783	0.14

Notes: Column (1) reports coefficient estimates on *Post* × *Treated* in equation (6) 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) – (3) report coefficient estimates on *Post* × *Treated* in equation (4) for firms' log(employment) and investment rate, respectively. The matched firm sample is obtained using exact matching by industry and coarsened exact matching by firms' total assets, employment, and revenue in 2005 – 2006. The matched worker sample is obtained using exact matching by industry and nearest-neighbor propensity score matching by workers' demographics (i.e., age) in 2006. 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) – (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) – (3), and reported in parentheses.

Table B11: Spillover of Treatment Effects

	(1) log(Annual Earnings)	(2) log(Employment)	(3) Investment rate
Post × Treated (Low Spillover)	-0.0145***	0.0452***	0.0116***
	(0.0044)	(0.0104)	(0.0035)
Post × Treated (High Spillover)	-0.0135**	0.0451***	0.0155***
	(0.0059)	(0.0077)	(0.0025)
Difference	0.0010	-0.0001	0.0039
	(0.0073)	(0.0129)	(0.0043)
Mean Dep. Var. (Low Spillover)	56.0	39.8	0.109
Mean Dep. Var. (High Spillover)	46.6	39.4	0.119
Observations	17,809,335	1,956,395	1,840,290
Treated Workers	400,670	15,480	14,930
Control Workers	791,735	141,330	126,120
Adjusted R^2	0.701	0.821	0.144

Notes: Column (1) reports coefficient estimates on *Post* × *Treated* in equation (6) 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) – (3) report coefficient estimates on *Post* × *Treated* in equation (4) for firms' log(employment) and investment rate, respectively. We estimate equations (3) and (5) separately for industries with high and low spillover effects and test the difference in a single regression. For each industry (4-digit NAICS) in the manufacturing sector, we compute the share of treated firms and define high- and low-spillover industries as those with the share below and above the median of all manufacturing industries, respectively. High- and low-spillover industries in non-manufacturing sectors are defined analogously. 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) – (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) – (3), and reported in parentheses.