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Essays on Capital Taxation, CEO Networks, and Real Investment

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ESSAYS ON CAPITAL TAXATION,
CEO NETWORKS, AND REAL INVESTMENT

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Abstract

This collection of essays studies the effects of capital taxation and CEO networks on real investment and allocation of resources. Chapter 1 assesses the effects of capital gains taxes on firms' investment by exploiting a unique institutional feature in Korea, where capital gains tax rates vary by firm size, and a policy reform that reduced the tax rates for firms affected by the new regulations. I find that firms whose capital gains tax rates dropped from 24 percent to 10 percent experienced a large increase in their market value, investment, and equity issuances, consistent with a class of the "traditional-view" models predicting that lower capital taxes spur equity-financed investment by increasing the marginal returns on investment. In Chapter 2, my co-author (Sungki Hong) and I quantify the long-run effects of reducing capital taxes on aggregate investment. We develop a dynamic general equilibrium model with heterogeneous firms, and calibrate the model by targeting important micro moments as well as the difference-in-differences estimate of the capital elasticity based on our institutional setting in Korea. We find that the 2014 reform that reduced capital gains tax rates for the affected firms substantially increased investment in the short-run, but accounting for general equilibrium and long-run effects is important to understand the aggregate effects. While Chapters 1 and 2 focus on the role of capital taxation on real investment, Chapter 3 (co-authored with David Schoenherr) investigates the role of CEO networks on credit allocation in private markets. We find that being connected to a new president leads to an increase in social connections to the banking sector for private firms with CEOs from the president's alumni network. These firms obtain more credit at a lower rate from private banks that appoint an executive from the same alumni network after the election. Our findings suggest that the election of a president from their network increases the influence of the alumni network over resource

allocation in private markets, which leads to more resources being (mis-)allocated to firms run by fellow alumni.

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To my family

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Chapter 1

Capital Gains Taxes and Real Corporate Investment¹

1.1 Introduction

Investment is central for growth and job creation in the economy, and an unresolved question in economics is the degree to which tax incentives affect corporate investment.² A recurring topic in policy debates is whether reducing the top federal tax rate on individual income from capital gains would stimulate the economy by inducing corporate investment.³ A traditional class of models, sometimes referred

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²The estimated range of the investment elasticity with respect to the net of tax rate varies across several empirical studies, including Summers (1981), Auerbach and Hassett (1992), Cummins, Hassett and Hubbard (1996), Goolsbee (1998), Chirinko, Fazzari and Meyer (1999), Desai and Goolsbee (2004), House and Shapiro (2008), Yagan (2015), Zwick and Mahon (2017), and Ohn (2018).

³One of the elements in President Trump's proposal on the 2017 tax reform was lowering capital gains taxes and its intention was to create greater incentives for people to invest (Rappeport 2017).

to as the “Old View”, predicts that lowering capital tax rates would increase investment by increasing the marginal returns on investment (Harberger 1962; Feldstein 1970; Poterba and Summers 1983). By contrast, a competing theory, known as the “New View”, argues that lower capital taxes will have no effect on investment. The “New View” assumes that firms make marginal investment choices out of retained earnings, so lowering capital taxes would increase the marginal return on investment by the same degree as it increases the marginal incentive to increase payouts (King 1977; Auerbach 1979; Bradford 1981).

Empirically evaluating the tax effects on investment is challenging in part because it is difficult to find large and exogenous variation in tax rates across firms. To isolate the tax effects from business cycle effects, we need a control group of firms not impacted by the tax change. A recent influential study that uses such variation shows precise zero effects of dividend taxes on investment (Yagan 2015). However, despite the fact that capital gains taxes may have different effects than dividend taxes and feature prominently in policy debates on tax reforms, there is limited empirical evidence on the effects of capital gains taxes on corporate investment.

This paper estimates the effects of capital gains taxes on firms’ investment by exploiting a unique institutional feature in Korea, where capital gains tax rates vary across firms, and a policy reform that reduced the tax rates for firms affected by the new regulations. In Korea, capital gains tax rates vary primarily by firm size, which was mainly determined by revenue and labor thresholds prior to the reform in 2014. An investor in a small firm faces a tax rate of 10 percent when selling a stock, while an investor in a large firm faces a tax rate of 24 percent. This tax scheme generates a substantial difference in average tax rates between a small firm and a large firm. In 2014, the government unexpectedly changed the regulations on firm size by eliminating the labor threshold and setting a new revenue cutoff based on average revenues over the past three years. Due to this

change, a large number of firms that were initially above either of the old cutoffs, but below the new threshold, became reclassified as small firms, while firms above the new cutoff were unaffected by the reform. To identify the tax effects on real outcomes, I compare firms that experienced a tax reduction with unaffected firms in a difference-in-differences framework using proprietary data on publicly listed and private firms.

Comparing publicly listed firms that experienced a reduction in tax rates from 24 percent to 10 percent to unaffected listed firms, I find that the affected firms increased investment by 50 log points within three years after the reform, with the implied investment elasticity of 2.77 with respect to the net of tax rate. Additionally, I find that market value of the affected firms increased by 16 percent, with the implied elasticity of 0.87. Moreover, newly issued equity increased by 4 cents per dollar of lagged revenue for the affected firms, consistent with a class of the “traditional-view” models of capital taxation which predicts that lowering capital taxes reduces the cost of capital and spurs investment financed through equity. The estimates imply that an average firm with the tax cut increased investment and newly issued equity by 2.5 million and 2.4 million dollars per year after the reform, respectively, compared to an average unaffected firm.

I proceed to examine how the response to the tax cut varies by firm characteristics. Firms that were relatively cash-constrained, based on their retained earnings, exhibited a significantly higher investment response, with the implied elasticity of 3.55, than did firms that were relatively cash-rich, with the implied elasticity of 0.54. This finding suggests that the marginal cost of investment is higher for more cash-constrained firms that have to rely on external financing to raise investment funds.

Moreover, I investigate whether lowering capital gains tax rates generates different investment responses depending on CEO incentives by linking detailed

ownership data to accounting and financial data at the firm-level. This analysis is motivated by a recent theoretical model by Chetty and Saez (2010), which predicts that CEOs who own a lower fraction of firms' stock are more likely to increase investment following a capital tax cut compared to CEOs with higher ownership. To test the tax effects separately by CEOs' ownership type, I compare firms whose CEOs own a low (below-median) share with firms whose CEOs own a high (above-median) share in a triple-difference framework. I find that the investment elasticity with respect to the net of tax rate is 2.89 for firms with low ownership CEOs, while the investment elasticity is 0.69 for firms with high ownership CEOs. These findings are consistent with theories of agency conflicts and the predictions of the model developed by Chetty and Saez (2010).

I supplement the investment analysis by adding private firm data to the main analysis sample with publicly traded companies, and find that the affected firms increased investment by 36 log points on average, with the implied elasticity of 1.93 with respect to the net of tax rate, compared to unaffected firms. In terms of aggregate dollars of investment, reducing the capital gains tax rates from 24 percent to 10 percent for the affected firms led to about a 1.6 billion dollar increase in aggregate investment, which is roughly 3 percent of total investment on physical capital assets in the economy after the policy change. This is a large response in aggregate investment, considering that the reform was not intended as a stimulus and affected a relatively small portion of firms, whose investment comprised 10 percent of total investment in the economy prior to the reform.

The identifying assumption behind this research design is that the affected and unaffected firms' outcomes would have trended similarly in the absence of the reform. The key threat to this design is that time-varying shocks may coincide with the reform. I present four reasons why this threat is minimal. First, affected and unaffected firms exhibited parallel trends for key outcomes prior to the reform.

Second, stock price responses show that the reform was unanticipated, and there was no evidence of sorting at the new threshold for the first three years after the reform. Third, I conduct placebo tests defining a reform date with a year prior to the actual reform date and defining treated groups with random cutoff values, and I fail to reject the null hypothesis that the effects are not statistically different from zero in each of these tests. Lastly, the main results are robust across various specifications, such as with or without control variables, with or without using a balanced panel, and using different scales for investment. The investment elasticity with respect to the net of tax rate based on these alternative specifications falls within (2.2, 3.0) based on the main sample of publicly listed companies.

The estimates in this paper are much larger than the estimates in Yagan (2015) based on dividend taxes. I propose three reasons to explain the magnitude of my estimates. First, the affected group in my analysis sample mostly consists of small firms that recently became large. This implies that these firms are likely to be cash-constrained and to respond to a tax cut more aggressively than larger firms (Zwick and Mahon 2017). Second, the findings in the existing literature do not capture the heterogeneity in investment responses based on the ownership structure or CEO incentives. This paper is one of the few studies to empirically test the importance of this channel, and shows that the elasticity estimates much larger for firms whose CEOs own a lower fraction of the firms' stock. Third, the elective aspect of capital gains taxes may allow investors to exploit a reduction in tax rates before realizing their gains, potentially leading to a larger investment response.

The difference-in-differences estimates of the investment and capital stock elasticities are consistent with what we would expect from a static investment model. Holding other things constant and assuming firms retain their entire earnings, a 1 percent increase in the net of capital gains tax rate would decrease the user cost by roughly one percent. Since optimal levels of capital stock and investment are

directly inversely proportional to the user cost, the model predicts that a 1 percent increase in the net of capital gains tax rate would increase the capital stock by 1.3 percent, assuming reasonable values for the output elasticity of capital and labor, and the product demand elasticity. I find that one percent increase in the net of tax rate increased the capital stock by 0.56 percent. This estimate is lower than the model prediction, potentially because it captures the short-term capital response from firms facing adjustment costs. Furthermore, the larger investment elasticity of 2.8 is consistent with the fact that the capital depreciates and the optimal level of investment is lower than the optimal level of capital stock at the steady-state, yielding a higher percentage change in investment after experiencing a tax cut in the short-run.

This paper's main contribution to the existing literature is twofold. First, to the best of my knowledge, this paper is first to identify the effects of capital gains taxes on real corporate outcomes, and presents a set of estimates supporting the "Old View". Second, this paper tests the importance of CEO incentives as a channel to explain heterogeneous investment responses to capital gains taxes using detailed ownership data. These findings have policy implications that lowering capital gains tax rates spurs equity-financed investment and the effects are stronger for firms that appear to have more agency conflicts, so policymakers may benefit from considering firms' underlying ownership structure.

This paper complements a wide range of literature that has documented substantial effects of fiscal policies on real outcomes. Temporary reforms such as accelerated investment depreciation (House and Shapiro 2008; Zwick and Mahon 2017) and durable goods subsidies (Mian and Sufi 2012) have been shown to stimulate aggregate spending. Although a recent paper (Yagan 2015) shows that one of the largest tax cuts in the U.S. had no real effect on investment, this paper examines the impacts of another type of capital taxes and shows that reducing capital gains

taxes leads to a substantial increase in investment and that the ownership structure might be an important underlying mechanism.

The remainder of the paper is organized as follows. Section 1.2 describes the institutional background for the capital tax system in Korea. In Section 1.3, I discuss the theoretical framework to derive empirical predictions of the tax effects. I describe empirical strategy and data in Section 1.4. In Section 1.5, I present results and discuss economic interpretation and policy implications of my findings in Section 1.6. Section 1.7 concludes.

1.2 Institutional Background

This section describes the institutional background relevant for the capital gains tax system and the policy reform in Korea. The key institutional features that provide a clean empirical framework are that (1) the capital gains tax rates vary discretely by firm size, and (2) the government unexpectedly changed the regulations on firm size in 2014, reducing capital gains tax rates for firms that became reclassified as small due to the new regulations. Note that I use a conversion ratio of 1000 Korean Won to 1 U.S. dollar throughout the paper to describe the setting and interpret the findings.

1.2.1 Capital and Payout Taxes

In general, a firm faces two main types of capital taxes, explicitly and implicitly: (1) corporate income taxes on profits, and (2) payout taxes. The former taxes are based on net profits generated by a firm in a given year, and the firm explicitly pays these taxes. On the other hand, investors pay the latter taxes and firms may implicitly bear the tax burden as it could impact firms' investment, capital structure, and payout decisions.

There are two forms of payout taxes: (1) dividend taxes and (2) capital gains taxes. These taxes differ mostly in the sense that investors and managers can time dividend payouts and share repurchases to minimize overall capital tax burdens. For example, investors pay dividend taxes when firms pay out dividends, while investors pay capital gains taxes on their realized gains either when they sell their stock or their firms initiate share buybacks. In theory, an investor can indefinitely delay capital gains taxes by not selling the stock, just as firms can indefinitely delay either capital gains taxes or dividend taxes by not buying back shares or paying out dividends. However, in reality, investors may face liquidity shocks in each period or may have different discount rates, so we observe frequent stock trading and corporate payouts in each year.⁴

While previous studies have estimated the effects of dividend taxes on corporate payouts and investment (Chetty and Saez 2005; Yagan 2015), there is limited empirical evidence on the effects of capital gains taxes on firms' outcomes using data in the U.S. or in most other countries for two main reasons. First, the tax rates vary mostly at the investor level, and it is difficult to find variation in tax rates across firms. Second, it is difficult to find exogenous variation in the tax rates. By contrast, the settings in Korea are ideal; not only do the tax rates vary across firms based on firm size, but also there was a policy reform to exploit the time-series (within firm) variation in the tax rates.

⁴In Korea, an average publicly listed firm pays about 1.5 percent and 0.5 percent of total revenues as dividends and share buybacks, respectively. In the U.S., C-corporation companies pay about 1.2 percent and 0.3 percent of total (lagged) revenues as dividends and share buybacks, respectively, in the analysis sample of Yagan (2015). Given that dividend tax rates are generally higher than capital gains tax rates, it is theoretically and empirically ambiguous why firms would often pay out more dividends than they would buy back shares from existing investors (Black 1976).

1.2.2 Capital Gains Tax Rates in Korea

From 2005 to 2017, capital gains tax rates differed depending mainly on firm size.⁵ An investor in a large firm faces a capital gains tax rate of 24 percent on average.⁶ By contrast, an investor in a small firm faces a flat rate of 10 percent regardless of his share. In 2014, the government changed the regulations on firm size. However, the tax rates for small firms and large firms remained the same after this reform. Therefore, this rule generated time-series variation in tax rates within a given firm affected by the rule change. To identify the effects of capital gains taxes on corporate outcomes, I compare the outcomes of firms affected by this reform with the outcomes of unaffected firms for my identification strategy. I provide more details on the exact rules governing firm size prior to and after the policy change in the following subsection.

1.2.3 Rules on Firm Size

From 2009 to 2014, the government had enforced the following rules for determining firm size. For the main sectors (manufacturing, construction, production and information services) used in the analysis, a firm has to jointly satisfy the following criteria to be classified as small: (1) total revenues below 100 million dollars, (2) average employees below 300, (3) total capitals below 100 million dollars, and (4)

⁵In Korea, there are four types of firm size: (1) small, (2) small-medium, (3) medium-large, and (4) large. Investors in small or small-medium firms (Small-Medium Enterprises or SMEs) face the lower capital gains tax rate of 10 percent, while investors in medium-large or large firms face the higher capital gains tax rates of 20 percent to 26 percent. Since I focus on the differences between small-medium and medium-large firms in my analysis sample, I label small-medium firms as "small" and medium-large firms as "large (or big)" for simplicity.

⁶An investor in a large firm who owns less than 50 percent of the firm's stock faces a tax rate of 20 percent, while the largest shareholder in a large firm that owns less than 50 percent of the share faces a rate of 24 percent (and 26 percent if owning more than 50 percent). On average, the largest shareholders in large firms own less than 50 percent, so the top capital gains tax rate for large firms is 24 percent on average. Furthermore, if a large-firm investor sells his share within one year, then his gain is considered as short-term and the tax rate is 30 percent regardless of his ownership rate. More details on the historical capital gains tax rates in Korea can be found at this website: www.nts.go.kr/eng.

total assets below 500 million dollars.⁷ The term, "average employees", is defined as the sum of daily workers employed over the sum of operating days, divided by the total operating days. For example, if a factory employed 1000 workers every single day for 100 days, then the average employees is 1000. However, if the factory employed 1000 workers in the first day, and 10 workers every day for the rest of 99 days, then the average employees is 19.9 ($= \frac{1000 \times 1 + 10 \times 99}{100}$). Firms keep track of their employees on a monthly basis and have to report their employees used and total operating days to the government every quarter.⁸

To figure out which of the four criteria is most binding for firm size, I first examine the conditional probabilities, as shown in Table 1 in Appendix A. As illustrated in the table, the most binding running variable is total revenues, the second one is average employees, and the last one is total capitals. For example, 99 percent of firms that were jointly below the revenue and labor thresholds were classified as small firms. Total assets are not binding, in the sense that once I condition that a firm is above on any other cutoff, the probability of assets being above the cutoff is close to 1. Therefore, I build my empirical strategy around the two most binding running variables, revenue and labor. The main advantage of focusing on only revenue and labor thresholds is reducing the complexity of the pre-reform rules governing firm-size classification to build my empirical framework. Furthermore, focusing on these two main thresholds does not quantitatively affect the main results, given that less than one percent of firms jointly below the labor and revenue cutoff were classified as large firms prior to the reform and either

⁷Top five sectors in my analysis sample are (1) Manufacturing, (2) Construction, (3) Production and Information Services, (4) Retail, and (5) Science and Technology Services, which account for about 96 percent and 91 percent of the entire sample of publicly listed and private firms, respectively.

⁸The definition of employees excludes managers, board of directors, researchers, and outsourced workers. In theory, manufacturing firms can outsource factory workers using other firms (non-subsidiary since workers in subsidiary firms are computed as part of parent firms' employees) in order to avoid this regulation on labor size. However, it can be costly and potentially inefficient to rely heavily on outsourced workers, as evidenced by firms bunching at the labor cutoff in my sample (Appendix A).

including or excluding these firms as part of the treated group does not affect my results (see Section 1.4).

In 2014, the government unified the regulations on firm size by eliminating labor and total capital thresholds and by setting a new threshold, namely, "average revenue" based on the current and past two years. The government still enforced the asset threshold of 500 million dollars, but the asset cutoff was not binding either before or after the policy change. The primary intention of the reform was to simplify the rules surrounding firm size. This reform was discussed by government officials and policymakers in early 2014, its approval was announced in August, 2014, and the reform was implemented by the end of 2014; therefore, the policy change came as a shock to affected firms. Moreover, investors did not know which firms were actually affected by this reform until firm size was publicly announced through annual audit reports in the first quarter (March) of 2015. This is evidenced by stock price responses for affected firms, as compared to unaffected firms (see Section 1.5). I describe how I use this reform for identification in Section 1.4.

The reform had different impacts for different sectors. Even though the reform eliminated the labor threshold for all sectors as a requirement to remain small, and further changed the revenue threshold into an average over past three years, it increased the average revenue threshold to 150 million dollars only for the manufacturing sector. Since the reform increased the average revenue threshold only for that sector, after the reform, there were more firms that became reclassified as small firms within the manufacturing sector compared to other sectors. Moreover, even though the average revenue threshold did not increase to 150 million dollars for the construction or the production and information service sectors, many firms within these sectors became reclassified as small firms because these sectors were labor intensive and had many firms above the labor cutoff, but below the revenue cutoff, prior to the reform. Furthermore, the manufacturing, construction, and

production and information services sectors account for approximately 80 percent of all publicly listed companies in my sample period, and hence I use these as the main sectors. I provide more details on how the reform differentially affected different sectors, and the sectoral and industrial compositions of firms in Appendix A.

When measuring a firm's accounting variables, such as revenue and labor size, for tax purposes, a parent firm's accounting values are measured as follows if the firm has at least one subsidiary firm. If the parent firm has at least 50 percent ownership of the subsidiary firm, then the subsidiary's accounting variables are directly added to the parent firm. However, if the parent firm has less than 50 percent ownership, then the subsidiary's accounting variables are added by multiplying their values by the ownership rate. For example, if firm A with labor size of 300 owns firm B with labor size of 100, and if the ownership rate is 50 percent, then firm A's labor size is 400 and firm B's labor size is 400 for tax purposes. However, if the ownership rate is 20 percent, then firm A's labor size is 320 and firm B's labor size is 160. The subsidiary's accounting variables also account for the parent firm's accounting values in the same way, except for the double counting. Moreover, if the parent firm owns another subsidiary through its subsidiary, then the parent firm's ownership of that firm is calculated by multiplying the two ownership rates together. For example, if parent firm A owns 50 percent of a subsidiary B, and B owns 50 percent of a subsidiary C, then A's ownership of C is 50 percent. However, if firm C owns its own subsidiary - say, D - firm A's ownership of D is zero, as this indirect ownership does not extend further. More details with examples are included in Appendix A.

1.3 Conceptual Framework

In this section, I describe a simple theoretical framework to derive comparative statics on how capital gains taxes affect firms' investment, equity issuance, and payout decisions.

1.3.1 Two-period Investment Model: The “Old View” & the “New View”

I begin with a two-period investment model that nests both the “Old View” and the “New View”, closely following Chetty and Saez (2010). I use this model because it has predictions for these competing views, but other models, such as the Q-theory, can be used to derive similar predictions (Desai and Goolsbee 2004). Consider a firm that has initial cash holdings of C at the first period. The manager can use C to (1) do share repurchases, R , (2) pay out dividends, D , or (3) invest in a project, I , that yields revenues in the second period. The firm can raise additional funds by issuing new equity, E .⁹ In period 2, the firm generates net profits $f(I)$, where f is strictly concave. The firm then returns its profits (through share repurchases) and principal to shareholders. Those gains are taxed at the capital gains (τ_g) and corporate (τ_c) tax rates. Assume that firms pay investors only through share buybacks, so $D = 0$.¹⁰

⁹Firms can also raise funds through borrowing. Although firms in my setting hold loans, I assume that the only source of financing is through issuing new equity in order to shut down the trade-off between debt and equity financing in this simple model. In theory, lower capital gains tax rates may increase both newly issued equity and debts, but the equity to debt ratio should increase because it becomes marginally cheaper to finance through equity than debts. Debts can also increase since lower tax rates may increase firm value, which may lower the interest rates at which firms borrow from banks. However, explicitly modeling how firms make their capital structure and financing decisions is beyond the scope of this paper.

¹⁰Based on this problem, dynamic trading incentives exist based on the relative dividend tax rate to the capital gains tax rate. In the extreme case where the capital gains tax rate is too high, the firm will not invest and instead will use the entire cash to pay dividends. I assume that the only way to pay shareholders is through share buybacks, so I shut down this trade-off. Moreover, the effective dividend tax rate is higher than the effective capital gains tax rate in Korea, and there was no change in dividend tax rates around the reform in 2014. Nonetheless, I extend the model to incorporate the dividend payout option in Appendix A.

The manager can also buy a government bond that yields a fixed, untaxed interest rate of $r > 0$.¹¹ In period 1, the firm's manager chooses $\{I, R, E\}$ to maximize firm value such that $I + R = C + E$. In period 2, net-of-tax profits are distributed to shareholders. Therefore, the manager's problem is:

$$Max V = \underbrace{(1 - \tau_g)R - E}_{\text{period 1 cash flow}} + \frac{\overbrace{(1 - \tau_g)[(1 - \tau_c)f(I) + C - R] + E}^{\text{net-of-tax return to shareholders}}}{1 + r} \quad (1.1)$$

1.3.2 Model Predictions

The "New View" Prediction

The "New View" considers firms that have retained earnings C such that $(1 - \tau_c)f'(I) \leq r$. In this case, a firm will never set $E > 0$ and $R > 0$ at the same time.¹²

Then we have the following first order condition:

$$\frac{\partial V}{\partial E}(R = 0) = \frac{(1 - \tau_g)(1 - \tau_c)f'(C) - r}{1 + r} \leq 0 \quad (1.2)$$

which implies that the firm's optimal level of E^* is 0, and the optimal choice of R^* satisfies:

$$(1 - \tau_c)f'(C - R^*) = r \quad (1.3)$$

Therefore, the "New View" predicts that the capital gains tax rate does not distort R , E , or I . Instead, the corporate tax rate, τ_c , impacts the firm behavior by changing the marginal benefit of investment.

¹¹Like Chetty and Saez (2010), I abstract from general-equilibrium effects through which a lower τ_g may affect the equilibrium r^* .

¹²If a firm both issues equity and initiates share buybacks, it can strictly increase its firm value by reducing both E and R and lowering its tax bill by $\frac{\tau_g r}{1+r}$.

The “Old View” Prediction

The “Old View” considers firms that have retained earnings C such that $(1 - \tau_c)f'(I) > r$. In this case, a firm will set $R^* = 0$ since the marginal value of buying back shares when $E = 0$ is strictly negative. Intuitively, the cash-constrained firms do not initiate share buybacks because the marginal revenue of investment exceeds the marginal benefit of payouts. Then the optimal choices of equity and investment are given by:

$$(1 - \tau_g)(1 - \tau_c)f'(C) < r \implies E^* = 0 \quad (1.4)$$

$$(1 - \tau_g)(1 - \tau_c)f'(C) \geq r \implies (1 - \tau_g)(1 - \tau_c)f'(C + E^*) = r \quad (1.5)$$

In contrast to the “New View”, the “Old View” predicts that the capital gains tax rate distorts firms’ investment and equity issuances. Intuitively, lower capital gains tax rates increase the marginal returns on investment, so reducing τ_g induces higher I through higher E . Corporate taxes have similar effects because they also impact the marginal returns on investment.

1.4 Empirical Strategy

This section describes my empirical strategy and data to identify the effects of capital gains taxes on real corporate outcomes. Identifying tax effects on investment is challenging in part because the tax rate is potentially correlated with firms’ unobservables which may impact their investment decisions. Imagine estimating the effects of capital gains taxes on corporate outcomes by using the following linear regression model:

$$y_{it} = \alpha + \theta Small_{it} + \mathbf{X}_{it}\beta + \alpha_t + \alpha_i + \epsilon_{it} \quad (1.6)$$

where $Small_{it} = 1$ if a firm i is small and faces lower capital gains tax rates at time t , α_t and α_i are time and firm fixed effects. The estimate of θ captures the relationship between the tax rate and the main outcomes. The issue for inference is that OLS estimates of θ may be biased if firm size (tax rate) is correlated with unobservable determinants of firm outcomes, such as investment: $E[\epsilon_{it}|Small_{it}] \neq 0$. For example, a firm that becomes big might have a higher productivity, which is typically unobserved by econometricians, and would invest more, so the OLS estimate will be downward biased. On the other hand, a firm's unobserved capital structure could be positively associated with the lower tax rate, so in this case, the OLS estimate will be upward biased. Controlling for all observable characteristics of a firm may reduce some of these sources of bias, but in general, whether the OLS estimate of θ is upward or downward biased is ambiguous.

The capital tax system in Korea provides a unique empirical framework; the capital gains tax rates differ across firms based on firm size, where the difference is at the average tax rate. Until the reform in 2014, firm size was mainly determined by the revenue threshold of 100 million dollars and the average employee threshold of 300. If a firm has an incentive to minimize capital gains taxes, then one would expect to see firms sorting below each threshold.¹³ In public finance literature, one would use this tax notch to estimate the bunching in order to compute the corresponding elasticity (Kleven and Waseem 2013; Bachas and Soto 2018). However, to back out the investment elasticity from the labor elasticity or revenue elasticity

¹³Panel A of Figure A.2 in Appendix A illustrates firm density around the labor cutoff, conditional that the firms are below the other thresholds. I use average employees of five as the bin size. In this figure, we observe not only a jump in firm density at the cutoff but also that the McCrary (2008) test rejects the null hypothesis that the jump is statistically not different from zero at the 5 percent significance level. Panel B of Figure A.2 illustrates firm density around the revenue cutoff, conditional that the firms are below the other thresholds. I use revenues of 5 log points as the bin size. Similarly, we observe a jump in firm density at the cutoff, and the McCrary (2008) test rejects the null hypothesis that the jump is statistically not different from zero at the 5 percent significance level. These density graphs provide suggestive evidence that firms want to avoid facing higher tax rates by trying to stay below each of the cutoffs, although I observe some firms right above the cutoff, either because of adjustment costs or inability to control firm size precisely.

with respect to the net of capital gains tax rate, I have to make additional assumptions about the complementarity between labor and capital investment and how capital investment depends on the revenue growth. In other words, the bunching strategy alone would not yield the investment elasticity, which is the central focus of this paper. Another reason why I do not use the bunching strategy is that the bunching estimation requires a large number of observations to achieve precision (Kleven 2016). Therefore, I estimate the elasticity with respect to the net of tax rate combining this cross-sectional variation with the policy reform, as described in the following subsection.

1.4.1 Estimating Tax Effects on Main Outcomes

To identify the tax effects on corporate outcomes, I compare firms that became reclassified as small and experienced a tax cut by 14 percentage points after the reform in 2014 to a set of unaffected firms. I first define which firms were affected and which firms fell into a control group. Then I describe my empirical model and key assumptions necessary for identification.

To define the treated and control groups, I use the reform on firm size regulations in 2014 and the thresholds that determine firm size in the following way. Firm size was mainly determined by revenue and labor cutoffs until 2014, when the government unified the criteria. The reform brought three major changes. First, it eliminated labor and total capital thresholds, so firms initially above the labor cutoff but below the other thresholds experienced a tax rate drop of 14 percentage points. Second, the revenue threshold became the average of revenues over the current and past two years. Lastly, the average revenue cutoff increased from 100 million to 150 million dollars, so firms initially above the original revenue threshold but below the new average revenue cutoff experienced a 14 percentage points reduction in

tax rates.¹⁴ I define these firms that experienced a tax reduction as the main type of treated firms for the main results.

Furthermore, due to this reform, firms below and close to the labor and original revenue cutoffs may face an incentive to increase investment, given that there was evidence of bunching at both thresholds. If labor and capital were complementary, then eliminating the labor constraint should provide a similar tax incentive to increase investment as a reduction in the tax rate. Hence, I define these firms that were close to the labor cutoff, but 5 percent below it, as constituting the second type of treated firms.¹⁵ Additionally, firms that were close to the revenue cutoff, but 10 percent below it, fall into the second type of treated firms because they were bunching precisely to avoid higher tax rates; so, the removal of this cutoff may provide a similar incentive to increase investment as reducing the tax rates for these firms.

On the other hand, firms whose size was unaffected by the reform serve as the control group, given that there was no change in their incentive to invest.¹⁶ Therefore, my main analysis sample consists of the first type of treated firms that experienced a tax cut by 14 percentage points, while the control firms were

¹⁴The new revenue threshold did not increase to 150 million dollars for certain industries within the manufacturing sector and other sectors. Therefore, firms in these excluded industries that were above the initial revenue cutoff, but below the new revenue threshold, are defined as part of the control group. I provide more details on how the reform affected different industries in Appendix A.

¹⁵I chose firms 5 percent below the labor cutoff (between labor size of 285 and 300) and 10 percent below the revenue cutoff (between revenue size of 90 and 100 million dollars) as part of the additional treated group. The reason is that the growth rate of labor size for firms below the labor cutoff was about 5 percent on average, and the growth rate of revenues for firms below the revenue cutoff was about 10 percent on average prior to the reform. My results are qualitatively similar if I use a larger or smaller sample below each cutoff to define the treated group. Choosing an optimal sample size for these additional treated firms is compatible with choosing an optimal bandwidth in a regression discontinuity design, where I trade off consistency for efficiency. In other words, I can include more firms below the old cutoff as part of bunching firms to increase sample size, but my estimates will become smaller if these added firms do not increase investment as much as firms that are just below the cutoff.

¹⁶Firms that were above, but close to, the new cutoff might have an incentive to decrease investment to go below the threshold. Therefore, I drop 5 percent of firms above the new average revenue cutoff to mitigate this potential issue. My results are quantitatively similar to dropping firms in the range of 1 to 10 percent right above this new cutoff.

unaffected by the reform because they were above the new threshold and still remained as large firms after the reform. I run a separate analysis for the second type of treated (bunching) firms in Appendix A, along with the analysis where I combine both types of treated firms. Figure 1.1 illustrates the reform, and its effects on the two types of treated groups as well as the control group.

To validate my empirical design and graphically show the reform effects on firms' real outcomes, I estimate the following model:

$$y_{it} = \sum_{\tau=2009}^{2017} \theta_{\tau} \mathbb{1}[t = \tau] \times Treated_i + \alpha_i + \alpha_t + X_{it}\beta + \epsilon_{it} \quad (1.7)$$

where α_i and α_t are firm and year fixed effects, $Treated_i$ is a dummy equal to 1 if the firm experienced a reduction in capital gains tax rate from 24 percent to 10 percent, and X_{it} is a vector of firm characteristics, which consists of (1) basic controls, such as quartics in firm age and industry dummies interacted with year dummies, and (2) additional controls, such as dummies for each pre-reform (2014) operating profit quintile interacted with dummies for each year. I include quartics in age to control for baseline financial constraints of firms among treated and control groups. Furthermore, industry composition is different between treated and control firms, so I include industry dummies interacted with year dummies to flexibly control for any time-varying industry-specific shocks. Additionally, to absorb any non-tax trends driven by baseline differences in productivity across groups, I include dummies for pre-reform (2014) operating profits (revenues minus operating costs) quintiles interacted with dummies for each year. In other words, I allow each quintile of the operating profits to have its own non-parametric time trends unrelated to the firm-size reform in 2014. I cluster standard errors at the firm-level. Each coefficient θ_{τ} measures the change in the outcome variable y_{it} for

affected firms relative to unaffected firms in the τ -th year before or after the reform became effective in 2014. Note that θ_{2014} is normalized to be zero.

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

$$y_{it} = \alpha + \theta Treated_i \times Post_t + \alpha_i + \alpha_t + X_{it}\beta + \epsilon_{it} \quad (1.8)$$

where $Post_t$ is a dummy equal to 1 if it is after the reform year of 2014, and all the other variables are as defined as in equation (1.7). I report the estimates from this equation (1.8), as well as those from equation (1.7) in Section 1.5.

I fix the dummy for $Treated_i$ at the time of the reform. In principle, treated firms in my sample may cross the new threshold within three years after the reform and face a higher capital gains tax rate again, which could attenuate my estimates since they may not increase investment as much as they would have had they remained small throughout the post-reform period. Furthermore, control firms in my sample may go below the new cutoff and face a lower capital gains tax rate, which could attenuate my estimates as well, since they may increase investment after a tax cut. If either of these cases were prevalent, then my difference-in-differences estimates would yield a lower bound on the investment elasticity by holding the definition of $Treated_i$ fixed throughout the sample period. I discuss a potential issue related to this approach and its solution in Appendix A.

The main identifying assumptions behind the difference-in-differences design is not the random assignment of firms into treated or control groups. Instead, it is that the affected and unaffected firms' outcomes would have trended similarly in the absence of the policy change. The key threat to this design is that time-varying shocks may coincide with the reform. I present three reasons why this threat is minimal. First, affected and unaffected firms exhibited parallel trends for

key outcomes prior to the reform. Second, stock price responses show that the reform was unanticipated, and there was no evidence of sorting at the new cutoff for the first three years after the reform. Lastly, I conduct placebo tests defining a reform date with a year prior to the actual reform date and defining treated groups with random cutoff values. I fail to reject the null hypothesis that the effects are not statistically different from zero in each of these tests.

1.4.2 Data and Analysis Sample

For empirical analysis, I use firm-level data on publicly listed companies in Korea from 2009 to 2017, where I observe detailed accounting, financial, ownership, and management information about the firms. I acquired this data set from a data company called Korea Listed Company Association (KLCA). I focus on the following sectors: (1) Manufacturing, (2) Construction, and (3) Production and Information Services. I focus on the 2009-2017 time period because the rules for determining firm size remained the same throughout that period, except in 2014. In my sample period, firms in these sectors account for about 90% of all publicly listed companies and 80% of all private firms. Furthermore, firms in these sectors account for about 80% of total revenues in the entire sample. Moreover, for private firms, expenditures on physical capital investment are easier to measure and observe for these sectors compared to other sectors, such as retail. I run a separate analysis including firms in other sectors and find qualitatively similar results (see Appendix A).

I also use an accounting data set for private firms, which I acquired from another data company called Korea Information Service (KIS). One of the main differences between this data set and the other data set is the coverage rate: because private firms report this information only when they have assets worth at least 10 million dollars and are audited by the government, I have missing information on

accounting variables for certain firms and for certain years. Another important difference is that for private firms, many variables related to firms' capital and ownership structure, such as equity issuances, payouts, and CEOs' stock share, are missing, so I use private firm data primarily to supplement my analysis of the tax effects on investment. Finally, I use data on firms' ownership rates of their subsidiaries to adjust accounting values for accurately measuring firm size.

1.4.3 Variable Definitions

The main data set based on listed firms contains accounting and financial variables necessary for empirical analysis: assets, revenues, average employees, physical capital (tangible) assets, intangible assets, capital expenditures on physical assets and intangible assets, employee salaries, dividends, equity issued, profits, total capitals, debts, and stock prices. Furthermore, the data set can be matched to a separate data set on firms' ownership structure and management characteristics: ownership rates for anyone with more than 5 percent share, foreign ownership rate, and CEO ownership rate.

The key outcome variables are investment, equity issuances, payouts, and firm value. I define investment as the log of the firm's capital expenditures on tangible assets (including plants, properties, and equipment). I also use different measures of investment, such as scaling capital expenditures by lagged tangible assets or lagged total assets, and find results that are qualitatively similar across different measures. I define equity issuances as non-negative annual changes in total paid-in capital (Yagan 2015), and use measures of payouts, such as dividends and share buybacks, directly from the balance sheet data. I define firm value as the price to book value ratio. Since equity issuances and payouts are missing for most of private firms in my sample, I run the analysis on capital structure for only publicly listed companies. Similarly, I use publicly listed firms to run the heterogeneity

analysis by firms' cash constraints and CEO ownership rates, along with all related and additional tests in Appendix A. I use both publicly listed and private firms for analyzing tax effects on investment. I winsorize main outcome variables at the ninety-ninth percent level, and do robustness checks by winsorizing main outcomes at the ninety-fifth percent level (see Appendix A).¹⁷

1.4.4 Descriptive Statistics

I summarized the main variables, such as revenues, assets, labor size, and capital expenditures in Table 1. There are economically and statistically significant differences in these variables between treated and control firms. Treated firms' revenues are below \$150 million on average, while the control firms' revenues are above \$150 million on average. Even though expenditures on physical capital assets are lower for treated firms than for control firms, the difference in their expenditures, when scaled by lagged physical capital assets, is not statistically different from zero. Finally, listed firms are larger than private firms on average, in terms of their revenue, labor, capital, and asset size.

1.5 Results

This section shows the results from the estimation of the difference-in-differences models in Section 1.4, and presents additional tests supporting the interpretations of the results.

¹⁷By winsorizing (top-coding) at the ninety-ninth percentile, I replace any observations with values above the ninety-ninth percentile with the ninety-ninth percentile value. Winsorizing helps ensuring that results are not driven by data coding errors, which are possible even after the data companies scrutinize every audit report. When estimating means in finite samples from skewed distributions (i.e. investment), winsorizing can be optimal as one trades off bias with minimizing mean squared error (Rivest 1994).

1.5.1 Investment and Capital Structure

Panels A and B of Figure 1.2 plot the raw means of $\log(\text{investment})$ and equity issuances, scaled by lagged revenue, from year 2009 to 2017, where the solid line indicates the mean of treated firms and the dashed line indicates the mean of control firms. Each outcome is normalized to be zero in year 2014, when the reform was implemented. As each panel shows, the trend of each outcome looks parallel prior to the reform without any controls or fixed effects. Furthermore, increases in treated firms' investment and equity issuance relative to control firms after 2014 suggest that the reform had effects on firms that were reclassified as small and that experienced a reduction in capital gains tax rates from 24 percent to 10 percent.

Panel A of Figure 1.3 plots the coefficients θ_τ , where $\tau \in (2009, \dots, 2017)$, for $\log(\text{investment})$ as in equation (1.7). As the graphs shows, the parallel pre-trend on investment between the affected and unaffected firms is satisfied, as the coefficient estimates are close to zero prior to the reform. Moreover, positive and statistically significant coefficients after the year 2014 indicate that lower tax rates induced the affected firms to increase investment.

Table 2 presents the difference-in-differences estimation results on investment, equity issuance, and payouts, using the sample of publicly listed companies. I winsorize (top-code) the main outcomes at the ninety-ninth percentile. Column (1) shows that the coefficient is 0.511, with the 95 percent confidence interval of (0.295, 0.727). These estimates imply that an average publicly listed firm among the treated group increased investment by roughly 2.5 million dollars per year after the reform, compared to an average firm in the control group.

I compute the implied investment elasticity with respect to the net of capital gains tax rate in the following way:

$$\epsilon_{y,1-\tau} = \frac{\% \Delta y}{\% \Delta(\text{net of tax rate})} = \frac{\Delta y}{y_0} * \frac{(1 - \tau_0)}{\Delta \tau} \quad (1.9)$$

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The estimated elasticity is 2.77, with the 95 percent confidence interval of (1.69, 3.86), which implies that a 1 percent increase in the net of tax rate would increase investment by 2.8 percent.

Columns (2) of Table 2 shows that the affected firms increased new equity issuances by 4 cents per dollar of lagged revenue, which is consistent with the investment response. Panel B in Figure 1.3 plots the coefficients θ_τ , where $\tau \in (2009, \dots, 2017)$, for equity issuances. As the graphs show, the parallel pre-trend for the affected and unaffected firms holds reasonably well, as the coefficient estimates are close to zero prior to the reform. Moreover, positive and statistically significant coefficients after 2014 indicate that lower capital gains tax rates induced the affected firms to increase equity issuances. These estimates imply that an average publicly listed firm among the treated group increased newly issued equity by roughly 2.4 million dollars per year after the reform, compared to an average firm in the control group. Note that the increase in dollar amounts of newly issued equity was roughly equal to the increase in dollar amounts of investment for the affected firms after the reform.

By contrast, the effects of lower capital gains tax rates on dividend payouts and share buybacks are neither economically nor statistically different from zero, implying that lower capital gains taxes did not affect firms' payout decisions. These results are consistent with the mechanism postulated by the "Old View", which argues that firms make marginal investment choices through issuing new equity and lower capital taxes induce equity-financed investment, without affecting corporate payouts in the short to medium run.

I supplement the investment analysis by adding private firms to the main analysis sample. Table 3 presents the difference-in-differences estimation results on investment, using publicly listed and private firms separately, as well as combining them. Column (1) shows that the estimated coefficient is 0.356, with the implied

elasticity of 1.93 with respect to the net of tax rate, which is smaller than the estimate based on the sample of only listed companies. Figure 1.4 plots the coefficients θ_τ , where $\tau \in (2009, \dots, 2017)$, for $\log(\text{investment})$. As the graph shows, the parallel pre-trend on investment between the affected and unaffected firms is satisfied, as the coefficient estimates are close to zero prior to the reform. Moreover, positive and statistically significant coefficients after the year 2014 indicate that lower tax rates induced the affected firms, whether publicly listed or private, to increase investment.

1.5.2 Stock Prices and Firm Value

In theory, lowering capital gains tax rates for a given stock would increase demand for that stock, holding everything else equal, by increasing the net present value of future returns on the stock.¹⁸ However, if investors did not care about capital gains taxes or were unaware of the reform in 2014, then the reform would have no effects on the stock prices of the affected firms. To test the effects of the tax cut on stock prices, I estimate equations (1.7) and (1.8), replacing the outcome variable as $\log(\text{Stock Prices})$ and $\log(\text{firm value})$, replacing year dummies with month dummies, and restricting the time interval from the beginning of 2014 to the end of 2015 (two-year window).

Figure 1.5 plots the coefficients on the *Treated*×*Time* dummies for $\ln(\text{Stock Prices})$ in each month. As the graph shows, the parallel pre-trend on stock prices between the affected and unaffected firms is satisfied prior to the reform date, as the coefficient estimates are close to zero prior to the reform. Furthermore, the coefficients are positive and statistically significant after the reform, implying that firms with

¹⁸In an equilibrium asset pricing model, reducing capital gains tax rates may increase equity prices as demand increases (capitalization effects), but it may also decrease equity prices as supply from existing investors increases (relaxing lock-in effects). Therefore, in theory, the effects of reducing tax rates on stock prices are ambiguous (Dai et al. 2008). On the net, lower capital gains tax rates may increase stock prices if the demand effect outweighs the supply effect.

the tax cut experienced an increase in their market value. Moreover, these results confirm that the reform was unexpected, as the stock prices started reacting only after the reform was implemented.

Table 4 reports the difference-in-differences estimates of the tax effects on stock prices and firm value. Odd-numbered columns have basic controls, such as quartiles in firm age and industry-specific time trends, and even-numbered columns additionally have dummies for pre-reform (2014) operating profit quintiles interacted with dummies for each month. Column (2) shows that a tax reduction is associated with a 16 percent increase in stock prices, with the implied elasticity of 0.87 with respect to the net of capital gains tax rate. Similarly, column (4) shows that firm value increased by 11 percent following the tax reduction, with the implied elasticity of 0.60. These results suggest that lowering capital gains taxes positively affects firms' market value.

To summarize, I find that lowering capital gains tax rates from 24 percent to 10 percent significantly increased investment, equity issuance, and market value of the affected firms, without impacting their payouts. These results are consistent with the "Old View" that lower capital taxes reduce firms' cost of capital and induce equity-financed investment. In the following sections, I conduct several robustness tests to strengthen the internal validity of my results, and show heterogeneity tests to explore potential mechanisms behind these findings.

1.5.3 Robustness and Internal Validity

I conduct several robustness checks to strengthen the internal validity of my results. First, I repeat the main analysis in equation (1.8) without any basic or additional controls and with only basic controls and find qualitatively similar results. Second, I repeat the analysis using different levels of winsorizing and find that the results are quantitatively similar when winsorizing at 95 percent level. Third, I repeat the

main analysis using a balanced panel, finding results that are qualitatively similar. Fourth, I repeat the main analysis by including firms in other sectors and find results that are qualitatively similar. Lastly, I use different scales for investment, such as scaling capital expenditures by lagged tangible assets or by lagged total assets and find qualitatively similar results. The investment elasticity based on this set of robustness tests falls within (2.2, 3.0), where the main estimate is 2.8. Results from these robustness checks are reported in Appendix A.

Assuming that the first type of treated firms with a tax cut initially made an optimal choice to pass either of the thresholds in order to grow big prior to the reform in 2014, their investment responses following the tax reduction due to the changes in firm size regulations might be smaller than the ones that would have happened following pure tax rate changes. By contrast, if there was an interaction between their growth potential and a lower cost of capital, then lowering capital gains taxes would amplify their investment responses. To address this concern, I run an additional test using a subsample of firms that passed either of the thresholds by 2010, so that their investment response would be mostly driven by the tax cut, and find quantitatively similar results (Appendix A).

Moreover, I use firms that were bunching at either of the old cutoffs and compare their difference-in-differences estimates to the ones from the affected firms with a tax cut. In the absence of optimization frictions, comparing changes in investment for firms that bunched at the old cutoffs to the control firms after the cutoffs are removed is comparable to using the bunching strategy to estimate the investment response by comparing firms that bunch at the thresholds to those at the counterfactual density (where the old cutoffs are no longer binding). Firms that were bunching might have had fewer investment opportunities (otherwise, they would have just crossed the old cutoffs), so their investment response after the reform may provide a lower bound on the investment elasticity with respect to the net of capital gains

tax rate. On the other hand, assuming optimization frictions, firms that were bunching may yield an upper bound on the investment elasticity compared to firms that passed the old cutoffs, if firms crossed the old thresholds because of either adjustment costs or inability to control firm size precisely. Using a sample of both publicly listed and private firms, I find that the investment elasticity with respect to the net of capital gains tax rate is slightly higher for the bunching firms than the one from firms with a tax cut, although the difference between the two estimates is not statistically different from zero (see Appendix A).

A potential threat to the internal validity of this research design is that contemporary changes to other tax policies might affect the results. To account for this potential bias, I conduct a placebo test defining the reform year as the year 2012, instead of the year 2014, and fail to reject the null hypothesis that the effects on the main outcomes are not statistically different from zero. I also conduct another placebo test defining treated firms with random cutoff values and fail to reject the null hypothesis that the effects are not statistically different from zero. The placebo test results are reported in Appendix A.

1.5.4 Heterogeneity Analysis

In this subsection, I discuss and empirically test potential mechanisms for heterogeneous investment responses to a reduction in capital gains tax rates. The first channel is the cash-constraint: cash-constrained firms that raise funds through new equity potentially face a higher marginal cost of investment than do firms that use internal funds, so the effects of lower capital gains taxes might be larger for relatively cash-constrained firms. The second mechanism is the CEO incentive channel, according to which lowering capital tax rates induces firms with low-ownership CEOs to invest more by reducing agency problems. There could be other related or independent mechanisms that can drive different investment responses to a change

in capital gains taxes, one of which I discuss in Appendix A. Understanding potential mechanisms behind different investment responses might be important for policymakers designing an effective capital tax system, and exploring other various channels would be a valuable extension to this paper.

Cash Constraints Channel

My main results are consistent with the “Old View” predictions that lowering capital tax rates would induce firms to increase investment by issuing new equity. However, the “New View” assumes that firms use retained earnings to make marginal investment, and predicts that lowering capital tax rates increases the marginal return on investment by the same degree as it increases the marginal incentive to increase payouts. Compared to cash-rich firms, cash-constrained firms face a higher marginal cost of investment, given that external financing is costly, and therefore, cash-constrained firms may react more aggressively to lower capital gains taxes.

However, measuring whether a firm is cash-constrained is difficult in practice. One major problem with using popular measures of cash constraints in my setting is that those measures, such as revenues, profit margin, and leverage ratio, are directly or indirectly correlated with whether firms are treated during the reform period. Therefore, I use a measure similar to the one used in Yagan (2015), namely retained earnings divided by assets averaged over three years at the time of the reform. I define a dummy variable equal to 1 if a firm is cash-rich (above the median) and equal to 0 if a firm is cash constrained (below the median).¹⁹ Then I estimate the following triple-difference model, where I interact the dummy with

¹⁹As in Yagan (2015), this definition is used to avoid strong parametric assumptions about whether these characteristics should be in the regression linearly or in logs. The results are qualitatively similar whether I use different percentiles (i.e. tercile) for the dummy variable.

the difference-in-differences model (1.8):

$$y_{it} = \alpha + \theta_1 Treated_i \times Post_t + \theta_2 Treat_i \times Post_t \times CR_i + \theta_3 CR_i \times Post_t + \alpha_i + \alpha_t + X_{it}\beta + \epsilon_{it} \quad (1.10)$$

where $CR_i = 1$ if a firm i is above the median (cash-rich) in the measure of cash constraint (fixed at the reform year of 2014), and the rest of variables are as defined in equation (1.7). θ_1 captures the tax effects for cash-constrained firms ($CR_i = 0$) and θ_2 captures the difference in the tax effects between the two firm types.²⁰

Panel A of Figure 1.6 plots the coefficients on the $Treated \times Time$ dummies for $\ln(\text{Investment})$ in each year, for cash-constrained firms. As the graph shows, the parallel pre-trend on investment for the affected and unaffected firms holds reasonably well, as the coefficient estimates are close to zero prior to the reform. The effects of the lower tax rates on investment are positive and statistically significant. Panel B of Figure 1.6 plots the estimates for cash-rich firms. As shown in the graph, the line is weakly parallel both before and after the reform, with a wide confidence interval.

Table 5 shows the results for this triple-difference estimation. Column (1) shows that the investment response is greater for cash-constrained firms, with the implied elasticity of 3.55. On the other hand, the coefficient on the triple interaction term is negative and statistically significant, implying that the investment elasticity is smaller for cash-rich firms. Moreover, increases in equity issuances are larger for more cash-constrained firms. These results are consistent with the channel that

²⁰Popular indices for measuring financial constraints, such as those developed by Kaplan-Zingales, Whited-Wu, and Hadlock-Pierce, are used in empirical work, but a recent study shows evidence that those measures do not accurately capture whether firms are constrained (Farre-Mensa and Ljungqvist 2016). Nevertheless, I run the same triple difference model using each of these indices as a measure of financial constraints, and find results that are qualitatively consistent with the main heterogeneity result using retained earnings as a proxy. Additional results based on these measures of financial constraints are reported in Appendix A.

cash constrained firms may face a higher marginal cost of investment, and may react more aggressively to lower capital gains taxes than do cash-rich firms.

CEO Incentive Channel

A separate channel can generate heterogeneous investment responses to a capital tax cut, potentially independent of the cash constraint mechanism. A recent theoretical model builds on the traditional two-period investment model and embeds agency conflicts to test whether tax effects differ depending on CEOs' ownership rates of their firms (Chetty and Saez 2010). The intuition behind the model is that when CEOs own a lower fraction of firms' stock, they put a higher weight on private consumption than on firms' profit maximization, so lowering tax rates helps align CEOs' incentives with firms' profit maximization. This model predicts that lowering capital tax rates would change investment responses differently depending on CEOs' ownership rates.

The CEO incentive channel predicts that low-ownership CEOs will increase investment more after a tax cut, regardless of whether their firms are cash constrained. When a firm is cash-rich and its CEO owns a large share, lowering the tax rate increases the marginal return on investment by the same degree as it increases the marginal incentive to increase payouts. By contrast, when a CEO owns a low share in a cash-rich firm, lowering the tax rate only shifts incentives away from his private consumption towards profits maximization, so investment will increase. When a firm is cash constrained, lowering the tax rate increases the marginal return on investment for both types of CEOs, so investment will increase regardless of CEOs' ownership rates. However, since a high-ownership CEO already invests at a level closer to profit-maximizing, his investment response will be smaller than a low-ownership CEO.²¹ Therefore, this model predicts that CEOs with low own-

²¹This prediction relies on an assumption that both types of cash-constrained firms (low or high ownership) have a medium level of initial cash. Then cash-constrained firms with high

ership will increase investment after a tax reduction more than will CEOs with a larger share, independent of the cash-constraint channel. I formalize this intuition and describe the details of the model in Appendix A.

To identify the tax effects on main outcomes separately by CEO's ownership type, I estimate the following triple difference model:

$$y_{it} = \alpha + \theta_1 Treated_i \times Post_t + \theta_2 Treat_i \times Post_t \times CEO_i + \theta_3 CEO_i \times Post_t + \alpha_i + \alpha_t + X_{it}\beta + \epsilon_{it} \quad (1.11)$$

where $CEO_i = 1$ if CEO of the firm i has a stock share above the median at the reform year of 2014 (fixed at the reform year of 2014), and the rest of variables are as defined in equation (1.7).²² θ_1 captures the tax effects for low-ownership CEOs ($CEO_i = 0$) and θ_2 captures the difference in the tax effects between the two CEO types.

Panel A of Figure 1.7 plots the coefficients on the $Treated \times Time$ dummies for $\ln(\text{Investment})$ in each year, for firms with low ownership CEOs. As the graph shows, the parallel pre-trend on investment for the affected and unaffected firms holds reasonably well, as the coefficient estimates are close to zero prior to the reform. The effects of the lower tax rates on investment are also positive and statistically significant. Panel B in Figure 1.7 plots the estimates for firms with high ownership CEOs. As shown in the graph, the line is weakly parallel both before and after the reform, with a wide confidence interval.

ownership CEOs would initially invest at a level closer to the optimal than cash-constrained firms with low ownership CEOs. For a given increase in the weight on firms' profits after a tax cut, firms whose CEOs own a lower fraction of the firms' stock would increase investment more than their counterparts, which may not hold if the initial cash was so low that even high ownership CEOs had to invest at a level far from profit-maximizing.

²²In my analysis sample, low-ownership CEOs have around 2 percent of their firms' stock on average, while high-ownership CEOs have around 22 percent on average. I also find qualitatively similar results when using different cuts (i.e. tercile) of CEO ownership rates.

Table 6 presents the triple difference coefficient estimates. Column (1) shows that the investment response is greater for firms with CEOs who own a lower fraction of firms' stock, with the implied elasticity of 2.89. On the other hand, the coefficient on the triple interaction term is negative and statistically significant, implying that the investment elasticity is smaller for firms with high ownership CEOs. The triple difference coefficient, along with the difference-in-differences coefficient, is consistent with predictions made by the investment model with agency conflicts (Chetty and Saez 2010).

1.5.5 External Validity

Arguably, this paper's findings might be particular to the analysis sample and may not necessarily apply to the overall firms. Although affected firms in my sample are relatively small compared to unaffected firms among publicly listed companies, these treated firms are comparable to an average manufacturing (private) firm in Korea. Firms in my sample are representative of the general firms in the Korean economy, as shown by the average firm characteristics in Table 24 of Appendix A. Also, I report the main results on investment using both listed and private firms.

Another external validity concern is that these effects might be local to only Korean firms. I use firms in Korea because the institutional setting may provide reasonable variation to identify the tax effects. I suggest the following reasons for why the results from this study are relevant for other developed countries, such as the United States. First, in the U.S., the current top federal capital gains tax rate is set at 20 percent, which is comparable to the top capital gains tax rate in Korea. Second, capital gains taxes are an important source of tax revenues in the U.S., accounting for approximately 5 percent of total tax revenues on average in the past decade. Furthermore, an average manufacturing firm in Korea based on the revenue size is comparable to its counterpart in the U.S, as shown in Table

25 of Appendix A. Therefore, the fact that Korea has a unique capital gains tax system does not necessarily imply that firms in Korea and firms in other countries differ fundamentally such that results from this setting cannot have implications for another setting. Nonetheless, extrapolating any local average treatment effects to a broader population in a different setting should be done with caution.

1.6 Economic Interpretation and Policy Implications

The previous section showed that the implied investment elasticity with respect to the net of capital gains tax rate is both economically and statistically significant. This section considers potential explanations for the magnitudes of these estimates and compare them to other estimates from existing literature.

1.6.1 Economic Interpretation

The hypothesis that a capital tax cut would significantly increase investment is based on a class of models representing the “Old View” (Harberger 1962; Feldstein 1970; Poterba and Summers 1983). In these models, a capital tax cut would reduce the cost of capital for firms that finance marginal investment with newly issued equity or risky borrowing. For example, a reduction in the capital gains tax rate reduces the taxes that must be paid when profits are distributed to shareholders, in the form of share buybacks or selling stocks, and subsequently induces firms to raise funds for new investment.

Based on the model parameterized by Desai and Goolsbee (2004), a firm faces a cost of capital which equals to

$$C_K = \frac{r}{(1 - \tau_c)[(1 - \tau_d)\rho + (1 - \tau_g)(1 - \rho)]} \quad (1.12)$$

where r is the expected return, τ_c , τ_d , τ_g , and ρ are corporate income tax rate, dividend tax rate, capital gains tax rate, and share of the earnings paid out to shareholders rather than retained, respectively. Fitting the model based on the parameters in my setting, I find that the cost of capital elasticity with respect to $(1 - \tau_g)$ is -0.85.²³ A recent paper by Zwick and Mahon (2017) finds that the investment elasticity with respect to the cost of capital falls between -0.8 and -3.3 based on their estimates from a sample of U.S. firms.²⁴ Multiplying the estimate of -0.85 to the range of the investment elasticity with respect to the cost of capital, I find that the investment elasticity with respect to $(1 - \tau_g)$ falls within (0.7, 2.8).

The estimates in this paper are much larger than the estimates in Yagan (2015) based on dividend taxes. I propose three main reasons to explain the magnitudes of my estimates. First, firms in my analysis sample are either small firms or firms that recently became large. This implies that these firms are more likely to be cash-constrained and to respond to the change in the tax rate more aggressively than bigger firms (Zwick and Mahon 2017). Thus, the relatively higher elasticity estimates could be driven by firm size. Second, the findings in the existing literature do not capture the heterogeneity in investment responses based on the ownership structure or CEO incentives. This paper is one of the few studies to empirically test the importance of this channel and to show that the investment elasticity is much larger for firms with low ownership CEOs.²⁵ This finding suggests that capital gains taxes might be more binding for firms whose CEOs do not have long-term interests on firm growths. By contrast, CEOs who own their firms tend to be more interested in the future growth of their firms, and may be less sensitive to a capital

²³I compute the cost elasticity assuming that $\tau_d = 0.38$ and $\alpha = 0.02$, given that total payouts (dividends and share buybacks) during this sample period were small during the sample period.

²⁴This range comes from the author's interpretation of the coefficient estimates presented in Panel B of Figure 4 in Zwick and Mahon (2017).

²⁵A recent study by Ma (2018) finds that the effects of lower dividend taxes on firms' R&D expenditures were larger for firms with medium ownership CEOs than for firms with high ownership CEOs, consistent with the agency conflict channel.

gains tax cut.²⁶ Third, the elective aspect of capital gains taxes may allow investors to exploit a reduction in tax rates before realizing their gains, which can lead firms to be more responsive to a tax cut.

1.6.2 Model-based Prediction on the Magnitude

An alternative way to interpret the magnitude of the investment response is to consider the capital stock response with respect to lower capital gains tax rates based on a static investment model framework. Under a set of modeling assumptions, outlined in Appendix A, one can derive how the optimal level of capital stock would change when firms' capital gains tax rates decrease from 24 percent to 10 percent. Here, a simplifying assumption is that the steady-state user cost of capital, as in equation (1.12), is a function of the expected rate of return on investment, corporate income tax rate, and capital gains tax rate: $C_K = \frac{r}{(1-\tau_c)(1-\tau_g)}$. When I calibrate the model assuming reasonable values for the output elasticities of capital (0.1) and labor (0.5), and the product demand elasticity (-5), I find that the capital stock elasticity with respect to the net of tax rate is 1.3. I also find that the lower bound on the capital stock elasticity with respect to the net of capital gains tax rate is 1, which implies that when the keep rate increases by one percent, the capital stock would increase at least by one percent.²⁷

Based on the sample of listed firms, I find that the capital stock elasticity with respect to the net of tax rate is 0.56 (see Appendix A). Even though the estimated capital stock elasticity falls below the lower bound of this model-based prediction, this finding is consistent with the fact that the difference-in-differences estimate captures the short-term capital stock response in my empirical setting, given that

²⁶Alternatively, firms whose CEOs own a larger fraction of firms' stock may face an off-setting incentive to increase payouts after a capital gains tax cut (Chetty and Saez 2010).

²⁷The calibrated capital stock elasticity falls within (1, 2.45) under the assumption that $\alpha_K \in (0.05, 0.25)$, $\alpha_L \in (0.45, 0.65)$, and $\epsilon \in (-10, -1)$.

I only observe three years of data after the reform. Furthermore, the static model does not account for dynamic adjustment frictions that may slow down the capital stock response in the short-run.²⁸

The capital stock response is also consistent with the larger investment response. Assuming that the pre-reform levels of the capital stock and investment were at the steady-state, the base level of investment is equal to the base level of the depreciated capital stock: $I^* = \delta K^*$, where δ is the depreciation rate. Then the investment elasticity with respect to the net of tax rate based on the aforementioned model framework is $1/\delta$ times the capital stock elasticity in the short-run: $\epsilon_{I,1-\tau_g} = \frac{1}{\delta}\epsilon_{K,1-\tau_g}$. If we assume that $\delta = 0.2$ in my setting, then the investment elasticity of 2.77 with respect to the net of tax rate roughly matches five times the capital stock elasticity of 0.56 with respect to the net of tax rate. Another reason for the higher investment response could be that investment is used to cover capital adjustment costs. If adjustment costs were high, then the capital stock response would be much lower than the investment response. Therefore, the difference-in-differences estimates of the investment and capital stock elasticities in this paper are consistent with what we would expect from a standard investment model. Incorporating dynamic prices and adjustment costs through a general equilibrium model to quantify the long-run aggregate investment response would be a valuable extension of this paper.

1.6.3 Policy Implications

The findings in this paper have important policy implications. First, taking the most conservative elasticity estimate, increasing the net of capital gains tax rate by one percent would increase investment by 1.9 percent based on the sample of

²⁸Another explanation for the smaller empirical estimate could be that the model assumes that the cost of capital goes down for every firm when capital gains tax rates decrease. In reality, however, certain firms may be less sensitive to lower capital gains tax rates depending on their ownership structure.

both publicly traded and private firms. In terms of aggregate dollars, the average aggregate investment amounts were 5.2 billion dollars among the treated firms after the reform. Then the aggregate increase in investment is given by $\Delta I = I_{actual} \times (1 - e^{-\theta}) = 1.6$ billion dollars within the main analysis sample, which is roughly 3 percent of total investment in the economy after the reform.²⁹ Even though a 3 percent increase in aggregate investment by the treated firms may seem like a small change, this investment increase is huge considering that the share of affected firms was relatively small and the reform was not intended as a stimulus.³⁰ Second, the huge investment response to the tax cut was concentrated among firms whose managers own a lower fraction of firms' stock, so when implementing a tax change, policymakers may benefit from considering firms' ownership structure.

However, policymakers should take the results from this paper with caution. First, the change in the capital gains tax rate from the reform was not meant to give a tax break for firms to stimulate aggregate investment, but rather to simplify the rules on firm size for tax purposes. The reform was unexpected and was implemented three months after it was announced. It might be difficult to discern tax effects from other confounders under an alternative policy change targeted to reduce the tax rate for certain groups of firms if it is well announced in advance so that firms gradually internalize and adapt to this change. Furthermore, this aggregate analysis assumes that aggregate investment of the control firms was unaffected. Accounting for potential general equilibrium effects as well as long-run investment response would be important to understand the aggregate impact of the reform. Moreover, CEOs' stock share may not be the only incentive that can

²⁹This calculation is based on a formula, $I_{actual} = I_{counterfactual} \times e^{\theta}$, where $\theta = 0.356$ is from the difference-in-differences estimation of the tax effects on investment using a sample of both publicly listed and private firms (Table 3).

³⁰The affected firms (publicly traded and private) accounted for about 8 percent of total revenues and 10 percent of total investment in the economy during the pre-reform period (2009-2014). Note that the aggregate amounts are based on the entire sample of my data sets, which include both publicly traded and private firms across all sectors. However, these data sets do not include firms that are too small to be audited (assets below 10 million dollars).

influence firms' reactions to a capital tax cut. For example, various measures of corporate governance, such as board independence and monitoring, may potentially explain the heterogeneity in investment responses to a tax cut. Despite these cautionary remarks, the findings in this paper are significant and provide concrete evidence that capital gains taxes affect firms' investment, market value, and equity issuances.

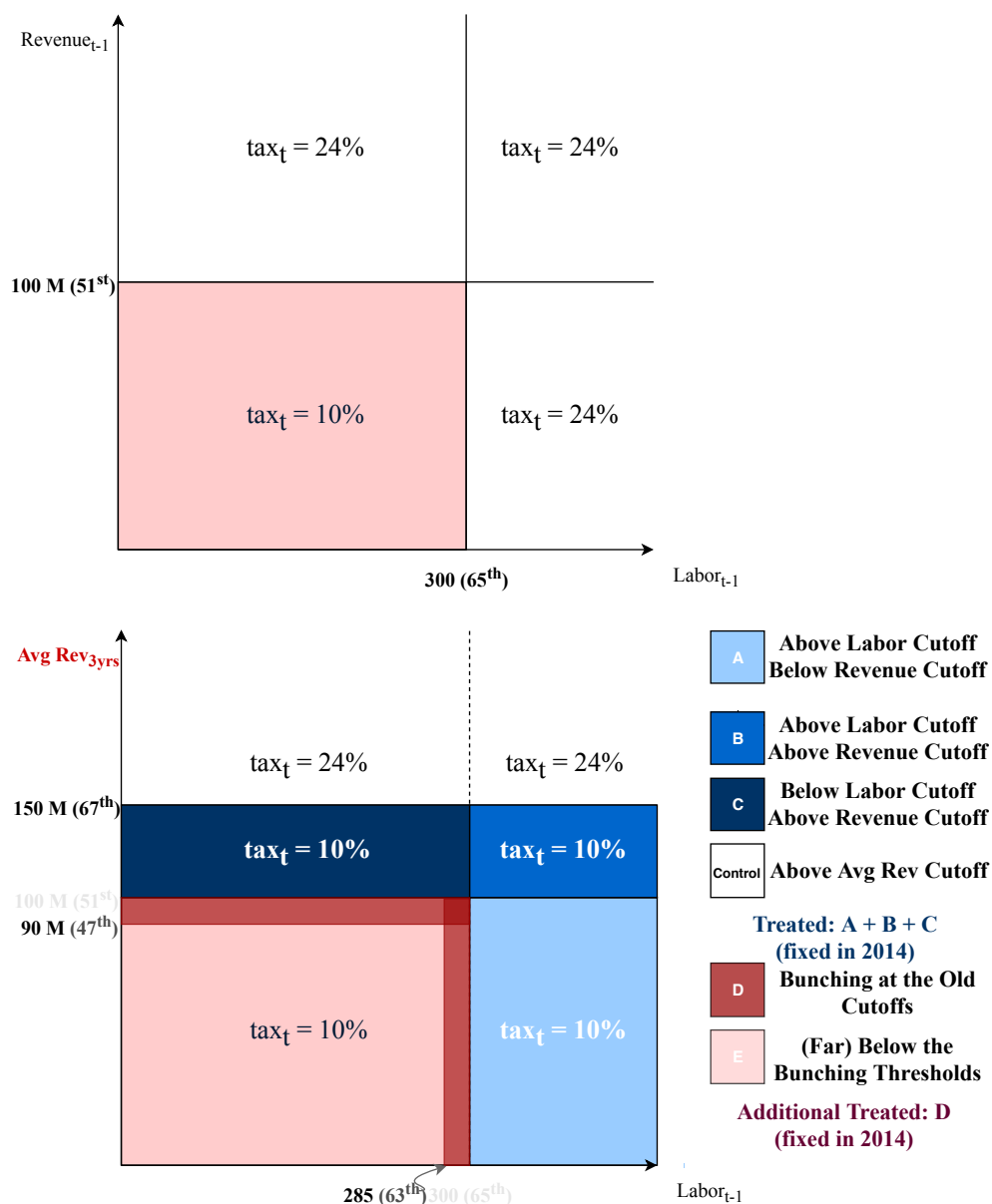
1.7 Conclusion

This paper exploits a unique institutional setting, policy reform, and comprehensive data sets to estimate the effects of capital gains taxes on corporate investment. In Korea, investors face starkly different average capital gains tax rates based on firm size, determined mainly by revenue and labor thresholds. In 2014, the government changed firm size regulations, and due to this unexpected reform, firms initially above the old cutoffs, but below the new threshold, experienced a reduction in tax rates. I compare their corporate outcomes with those of unaffected firms, finding that market value, investment, and newly issued equity increased significantly for the affected firms. Additionally, I find that investment responses are larger for firms that appeared more cash-constrained. These results are robust across various specifications and consistent with a class of the "traditional-view" models predicting that lowering capital tax rates spurs equity-financed investment.

Moreover, I find that firms whose CEOs own a lower fraction of firms' stock issued more equity and increased investment more than firms whose managers owned a larger share. These results suggest that CEO incentives and ownership structure matter for how firms react to changes in capital gains tax rates. Consistent with the agency model that nests both the "Old View" and the "New View", the results from this paper suggest that agency conflict may be an important mech-

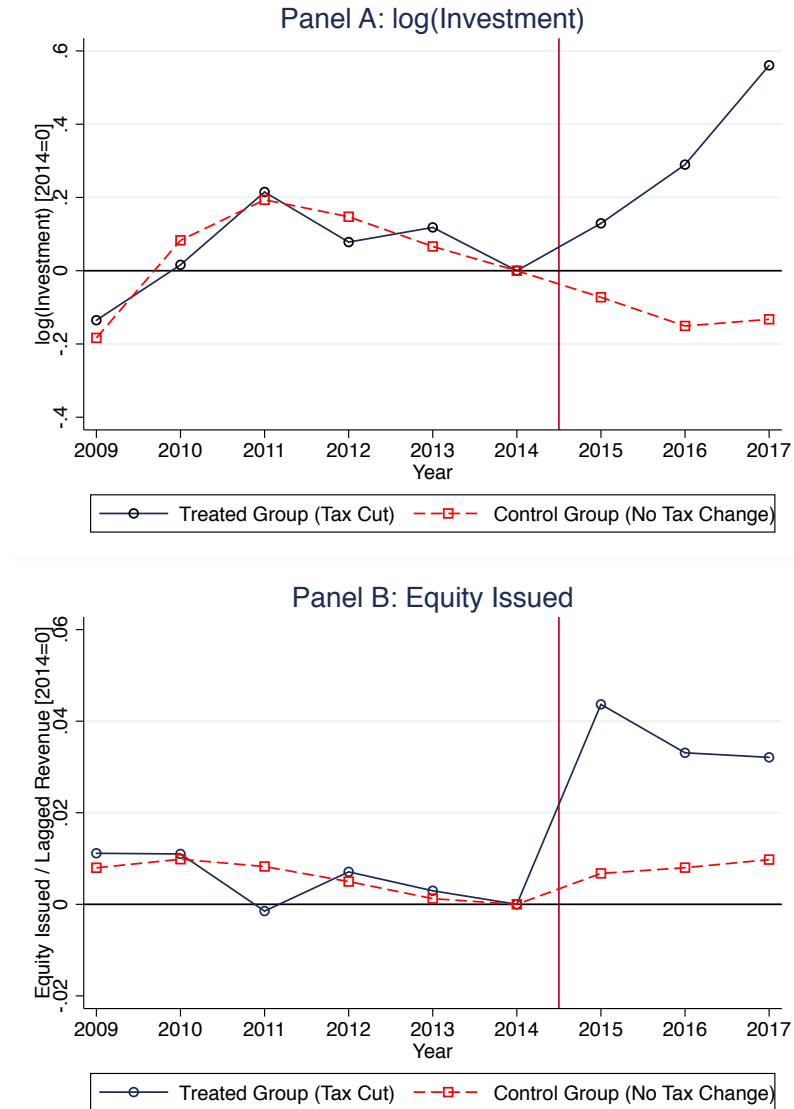
anism that explains heterogenous investment responses to a capital tax cut, and that policymakers may benefit from considering CEO incentives or the underlying ownership structure of firms when designing an optimal capital tax policy. Future empirical studies that test this agency channel more rigorously, through incorporating either different dimensions of managerial incentives or various measures of corporate governance, would be an interesting extension of this paper that may shed further light on the underlying mechanism and efficiency costs of capital taxation.

Figure 1.1: Policy Reform 2014 and Treated vs. Control Groups



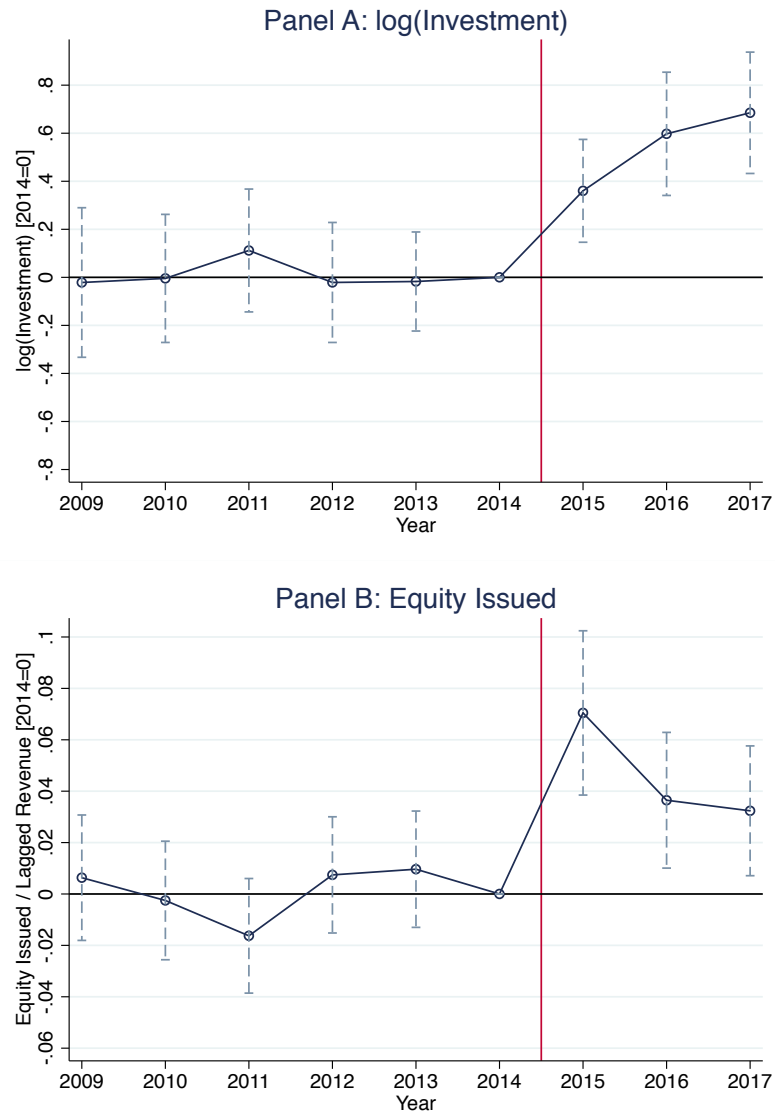
Notes: This figure illustrates how the reform in 2014 assigned firms into treated (tax cut) or control groups. The first figure on the top shows the initial rule on firm size prior to the reform, where firms in the pink area are jointly below labor and revenue threshold at time $t - 1$ and face a tax rate of 10%. The second figure shows how the reform affected firm size and the tax rates. I use firms in the blue areas (that experienced a tax cut by 14 percentage points) as the main treated group, and run a separate analysis using the second type of treated firms (that bunched in red areas) in Appendix A. I define the control group as firms that did not face any change in the tax rate or incentive to invest (in the white areas above the new revenue cutoff). Firms in the pink area were not directly impacted by the reform, but it is difficult to consider them as part of the control group because these firms were growing and may grow even more because the old thresholds no longer bind. The percentiles in brackets are based on a sample of publicly listed firms.

Figure 1.2: Raw Means of Corporate Outcomes of Affected and Unaffected Firms



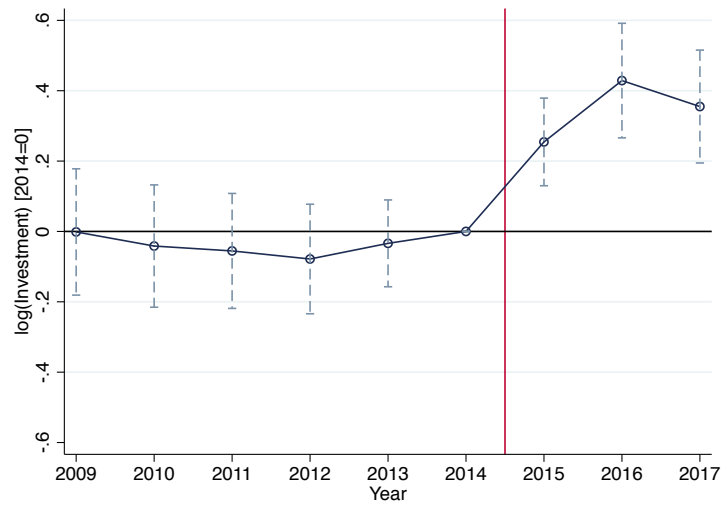
Notes: Panel A in this figure shows raw means of $\ln(\text{investment})$ of treated (solid) and control (dash) firms in each year, from 2009 to 2017. Panel B shows raw means of equity issuance, defined as non-negative annual changes in total paid-in capital, scaled by lagged revenues. Means of each outcome are normalized to be zero in year 2014, when the reform was implemented.

Figure 1.3: Tax Effects on Real Outcomes of Affected and Unaffected Firms



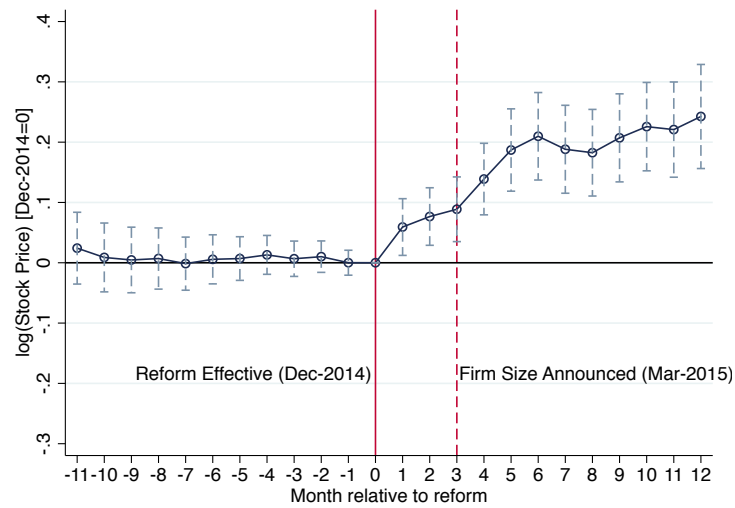
Notes: Panel A in this figure shows the coefficients on $Treated \times Time$ for firms' investment, defined as $\log(\text{expenditures on physical capital assets})$, in equation (1.7). The dashed lines indicate 95% confidence intervals for these coefficient estimates. The solid vertical line indicates the reform year. The same graph with two additional lines (without any controls or with only basic controls) are included in Appendix A. Panel B shows the coefficients on $Treated \times Time$ for firms' equity issuance, defined as non-negative annual changes in total paid-in capital, scaled by lagged revenue, in equation (1.7).

Figure 1.4: Tax Effects on $\ln(\text{Investment})$: Listed and Private Firms



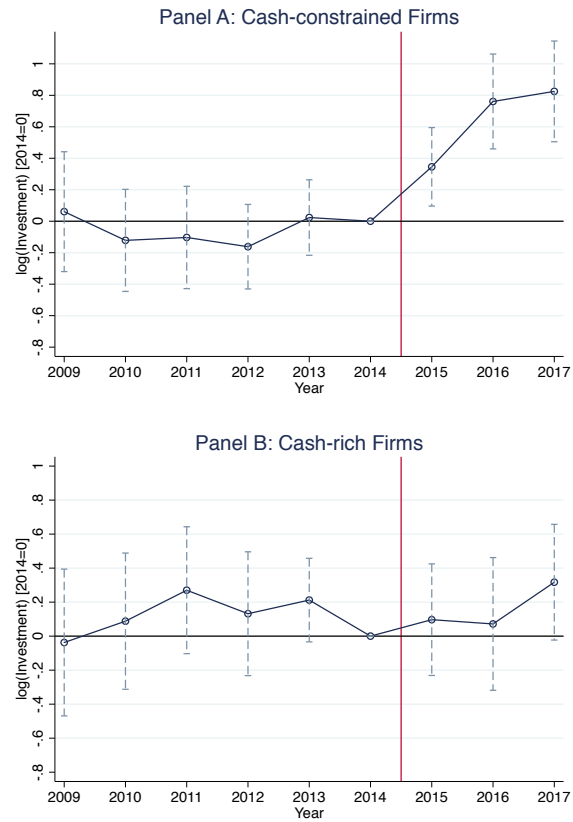
Notes: This figure shows the coefficients on $Treated \times Time$ for firms' investment, defined as $\log(\text{expenditures on physical capital assets})$, in equation (1.7) using both publicly listed and private firms. The dashed lines indicate 95% confidence intervals for these coefficient estimates. The solid vertical line indicates the reform year.

Figure 1.5: Tax Effects on $\ln(\text{Stock Prices})$: Listed Firms



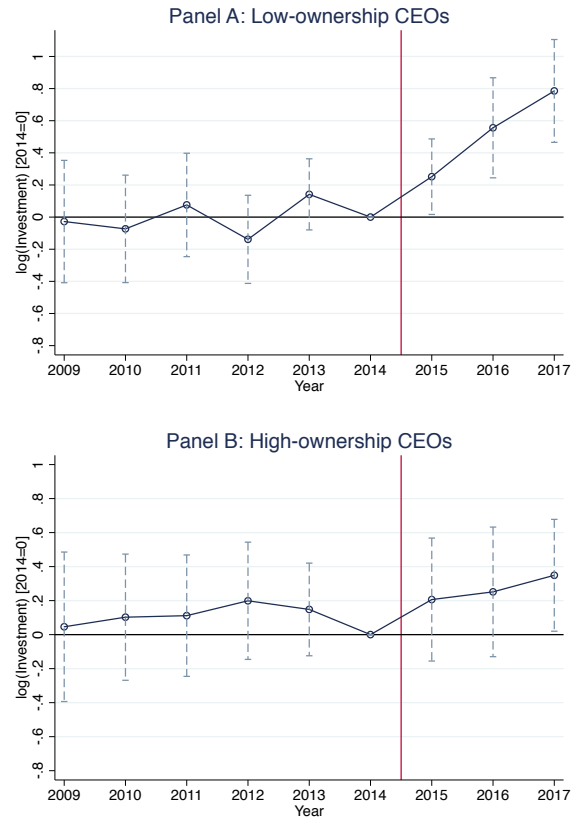
Notes: This figure shows the coefficients on $Treated \times Time$ for each time period (month). The outcome variable is $\ln(\text{Stock Prices})$. The dashed lines indicate the 95% confidence intervals for those coefficient estimates. The solid vertical line indicates the month in which the reform was implemented, and the dash vertical line indicates the month in which the firm size was publicly announced through the annual audit reports. The sample periods are from the beginning of 2014 to the end of 2015. The sample is restricted to publicly listed companies, where I can observe their stock prices at the monthly frequency.

Figure 1.6: Tax Effects on $\ln(\text{Investment})$ by Cash Constraints



Notes: Panel A in this figure shows the coefficients on $Treated \times Time$ for $\ln(\text{Investment})$ of cash-constrained firms. The dashed lines indicate the 95% confidence intervals for those coefficient estimates. The solid vertical line indicates the reform year. The sample is restricted to publicly listed companies. Panel B shows the coefficients on $Treated \times Time$ for $\ln(\text{Investment})$ of cash-rich firms.

Figure 1.7: Tax Effects on $\ln(\text{Investment})$ by CEO Ownership



Notes: Panel A in this figure shows the coefficients on $Treated \times Time$ for $\ln(\text{Investment})$ of low-ownership CEOs. The dashed lines indicate the 95% confidence intervals for those coefficient estimates. The solid vertical line indicates the reform year. The sample is restricted to publicly listed companies. Panel B shows the coefficients on $Treated \times Time$ for $\ln(\text{Investment})$ of high-ownership CEOs.

Table 1: Descriptive Statistics

	Listed and Private Firms		Listed Firms		Private Firms	
	(1) Treated	(2) Control	(3) Treated	(4) Control	(5) Treated	(6) Control
Total Revenue (in million)	74.69 (45.16)	251.8 (176.5)	95.13 (44.34)	284.4 (189.3)	67.10 (43.06)	226.6 (161.5)
Labor (Average Employees used)	243.1 (174.5)	393.4 (295.1)	254.3 (135.5)	467.5 (300.8)	238.9 (186.9)	335.9 (277.3)
Total Asset (in million)	75.02 (64.65)	237.3 (189.4)	123.1 (68.29)	305.2 (201.2)	57.16 (53.15)	184.7 (161.1)
Total Capital (in million)	40.50 (41.94)	123.1 (109.1)	74.52 (46.68)	165.8 (113.0)	27.86 (31.83)	89.99 (93.43)
CAPEX (in million)	4.067 (6.086)	11.22 (13.83)	5.763 (7.647)	13.34 (14.67)	3.436 (5.257)	9.580 (12.92)
CAPEX / lagged PPE	0.219 (0.264)	0.202 (0.234)	0.186 (0.224)	0.180 (0.209)	0.231 (0.277)	0.219 (0.250)
Observations	2635	8037	714	3509	1921	4528

Notes: Sample years include 2009-2017. Labor is the average employees used in a given year. CAPEX is expenditures on physical capital assets, such as plants, property, and equipment (PPE). Treated and control firms are defined in Section 1.4.

Table 2: Results on Investment and Capital Structure (Publicly Listed Firms)

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.511*** (0.110)	0.044*** (0.011)	0.002 (0.001)	0.001 (0.001)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.525	0.035	0.006	0.001
Implied Elasticity wrt (1-tau)	2.77	6.78	1.41	3.89
R-squared	0.68	0.30	0.74	0.28
Observations (firm-years)	4732	4503	4710	4715
Clusters (firms)	541	541	541	541

Notes: This table reports the tax effects on the main corporate outcomes based on specification (1.8). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, as explained in Section 1.4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. Basic controls are quartics in firm age and industry dummies interacted with time dummies. Additional controls are dummies for pre-reform (2014) operating profit quintile interacted with time dummies. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 3: Results on Investment (Publicly Listed and Private Firms)

	Listed and Private Firms	Listed Firms	Private Firms
	(1) ln(CAPEX)	(2) ln(CAPEX)	(3) ln(CAPEX)
Treated x Post	0.356** (0.068)	0.511** (0.110)	0.239** (0.087)
Basic Control	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes
Pre-reform Treated Mean	14.140	14.525	13.992
Implied Elasticity wrt (1-tau)	1.93	2.77	1.30
R-squared	0.72	0.68	0.73
Observations (firm-years)	12496	4732	7764
Clusters (firms)	1477	541	936

Notes: This table reports the tax effects on investment based on specification (1.8). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, as explained in Section 1.4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Basic controls are quartics in firm age and industry dummies interacted with time dummies. Additional controls are dummies for pre-reform (2014) operating profit quintile interacted with time dummies. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample includes both publicly listed companies and private firms. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 4: Results on Stock Prices, Firm Value, and Trading Volumes (Publicly Listed Firm)

	Stock Prices		Price to Book Value		Trading Volumes	
	(1)	(2)	(3)	(4)	(5)	(6)
	log(Price)	log(Price)	Firm Value	Firm Value	log(Volume)	log(Volume)
Treated x Post	0.118*** (0.034)	0.160*** (0.041)	0.112*** (0.041)	0.111** (0.046)	0.212*** (0.078)	0.266*** (0.080)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	No	Yes	No	Yes	No	Yes
Time and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	1.45	1.45	0.29	0.29	14.40	14.40
Implied Elasticity wrt (1-tau)	-0.64	-0.87	-0.61	-0.60	-1.15	-1.44
R-squared	0.96	0.96	0.87	0.87	0.80	0.82
Observations (firm-months)	12405	12405	12381	12381	12384	12384
Clusters (firms)	543	543	543	543	543	543

Notes: This table reports the tax effects on stock prices and firm value based on a variation of specification (1.8). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, as explained in Section 1.4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Price is the closing stock price (converted into US dollars) at the end of each month. Firm value is the price to book value ratio. Trading volume is the total market value of stocks traded in a given period. Basic controls are quartics in firm age and industry dummies interacted with time dummies. Additional controls are dummies for pre-reform (2014) operating profit quintile interacted with time dummies. The main outcomes are winsorized at the ninety-ninth level. Each time period is a month, and the sample period is from the beginning of the 2014 to the end of 2015. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies, where I can observe their stock prices at the monthly frequency. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 5: Results on Investment and Capital Structure by Cash Constraints (Listed Firms)

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.653*** (0.144)	0.066*** (0.019)	0.002 (0.001)	0.001 (0.001)
Treated x Post x Cash-Rich	-0.553*** (0.205)	-0.044** (0.022)	-0.001 (0.002)	-0.001 (0.001)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean (CR=0)	14.371	0.047	0.002	0.001
Implied Elasticity wrt (1-tau) (CR=0)	3.55	7.66	3.50	4.88
Pre-reform Treated Mean (CR=1)	14.7	0.015	0.011	0.0014
Implied Elasticity wrt (1-tau) (CR=1)	0.54	8.90	0.12	0.41
R-squared	0.64	0.29	0.74	0.28
Observations (firm-years)	4675	4454	4625	4658
Clusters (firms)	541	541	541	541

Notes: This table reports the tax effects on investment based on specification (1.10). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, as explained in Section 1.4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). The dummy for CR_i is 1 if the firm is cash-rich firm, as defined in Section 1.5. Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 6: Results on Investment and Capital Structure by CEO Ownership (Listed Firms)

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payout	(4) Share Buyback
Treated x Post	0.532*** (0.131)	0.075*** (0.018)	0.002 (0.001)	0.001 (0.001)
Treated x Post x CEO	-0.405** (0.203)	-0.068*** (0.020)	-0.004 (0.003)	0.000 (0.002)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean (CEO=0)	14.253	0.041	0.003	0.001
Implied Elasticity wrt (1-tau) (CEO=0)	2.89	9.89	3.99	3.69
Pre-reform Treated Mean (CEO=1)	14.9	0.019	0.011	0.002
Implied Elasticity wrt (1-tau) (CEO=1)	0.69	1.85	-0.62	2.19
R-squared	0.64	0.29	0.74	0.28
Observations (firm-years)	4673	4454	4625	4658
Clusters (firms)	541	541	541	541

Notes: This table reports the tax effects on investment based on specification (1.11) The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, as explained in Section 1.4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). The dummy for CEO_i is 1 if the firm's CEO has stock shares above the median, as defined in Section 1.5. Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Chapter 2

Capital Gains Taxation and Investment Dynamics¹

2.1 Introduction

A central question in the study of fiscal policies is the degree to which tax incentives affect aggregate investment to stimulate growth and job creation in the economy. While there is mixed empirical evidence on how much investment responds to lower corporate tax rates (Desai and Goolsbee (2004); House and Shapiro (2008); Yagan (2015); Zwick and Mahon (2017)), a recurring topic that features prominently in policy debates is the extent to which a reduction in capital gains tax rates would stimulate the economy by inducing corporate investment (Rappeport 2017).

Assessing the tax effects on investment is challenging in part because it is difficult to find large and exogenous variation in tax rates across firms. To control for business cycle effects, we need variation in the tax rates across firms, but capital gains tax rates vary at the investor level in most settings. Furthermore, quantifying

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the aggregate effects of capital gains taxes is challenging in a reduced-form analysis as it is difficult to account for general equilibrium and dynamic effects without a structural model.

In this paper, we quantify the effects of reducing capital gains taxes on aggregate investment by estimating a dynamic general equilibrium model. Building upon a standard investment model, we micro-found its main features based on our institutional setting, where capital gains tax rates vary across firms, and a policy reform that reduced the tax rates for firms affected by the new regulations. We discipline our model's key parameters using our difference-in-differences estimate of the capital stock elasticity with respect to capital gains taxes. We then use the model to reproduce the short-run investment response after the reform and to predict the long-run effects of reducing capital gains taxes on aggregate investment.

To recover the reduced-form estimates used in our model, we leverage the setting in Korea, where capital gains tax rates vary by firm size, mainly determined by revenue and labor thresholds prior to the reform in 2014. An investor in a small firm faces a tax rate of 10 percent when selling a stock, while an investor in a big firm faces a tax rate of 24 percent. In 2014, the government unexpectedly changed the regulations on firm size by eliminating the labor threshold and setting a new revenue cutoff based on the average over the past three years. From this change, a large number of firms that were above either of the old cutoffs, but below the new threshold, became reclassified as small firms, while firms above the new cutoff were unaffected by the reform. To identify the tax effects on real outcomes, we compare firms that experienced a tax reduction with unaffected firms in a difference-in-differences framework using proprietary firm-level data.

Comparing firms that experienced a reduction in tax rates from 24 percent to 10 percent with unaffected firms, we find that the affected firms increased investment by 36 percent and capital stock by 9 percent within three years after the reform. The

estimates suggest that affected firms increased investment by roughly 1.6 billion dollars, which is 3 percent of total investment in the economy after the reform. We show that our results are internally consistent by providing the evidence of the parallel pre-trends on the key outcomes and a set of robustness checks and placebo tests.

To quantify the aggregate effects, we develop a dynamic general equilibrium model with heterogeneous firms building on a framework by Gourio and Miao (2010). We extend their model by incorporating three important features to match the key empirical moments and the institutional setting in Korea. First, our model incorporates a discrete change in capital gains tax rates based on firm size to be consistent with the institutional feature in Korea. Second, we introduce the state-of-the-art random fixed costs of capital adjustments as in Khan and Thomas (2008) to capture lumpy investment at the micro-level. Third, we assume a Poisson process for shocks to productivity as in Midrigan (2011) and Bachmann and Bayer (2014), which is a key element for matching the empirical estimate of the capital stock elasticity with respect to capital gains tax rates.

Based on this framework, we structurally estimate the capital adjustment costs and an idiosyncratic productivity process using a simulated method of moments (SMM). We target micro moments of the investment to capital ratio, firms' revenues, and the difference-in-differences estimate of the capital stock elasticity. Although firm density is not targeted, our model generates a set of firms bunching below the thresholds that determine the tax rates, consistent with our observation in the data.

One primary feature that distinguishes our model from the existing models (Gourio and Miao 2010) is that our model targets the difference-in-differences estimate of the capital stock elasticity with respect to capital gains tax rates based on the reform in 2014.² To match this moment, we conducted the same policy reform

²There are studies that discipline structural models based on empirical estimates using natural policy experiments to analyze the effectiveness of other policy instruments. Kaplan and Violante

in our model to calculate the average response of treated firms in the short-run partial equilibrium. The partial equilibrium framework is appropriate for calibration because the reform in 2014 affected a small portion of firms in the economy, whose aggregate investment response comprised only 3 percent of total investment in the economy.

A key finding from our model is that the effect of reducing capital gains taxes depends on the persistence of firm size. In each period, firms expand or shrink in size, creating firm-size dynamics around each threshold. We calibrate a productivity process more persistent than an AR(1) process with Gaussian shocks by allowing productivity shocks to arrive infrequently with a Poisson probability as in Midrigan (2011) and Bachmann and Bayer (2014). The Poisson probability makes the conditional distribution of the next period's productivity have more peakedness around the mean with heavier tails. The "peakedness effect" increases the probability of a treated firm becoming a large firm again, while the "heavy tail effect" does the opposite. Our simulation shows that the former effect dominates the latter. Consequently, our model with Poisson shocks generates much larger capital responses than does the model by Gourio and Miao (2010).

Using these key moments, we conduct a counterfactual analysis of extending the scope of the 2014 reform to the overall economy to assess the aggregate effects of reducing capital gains taxes. In the partial equilibrium setting, the reform had a large impact on the economy, increasing aggregate consumption, output, capital, and labor by 2.4, 2.6, 3.6, and 2.8 percent, respectively, in the long run.

In the general equilibrium framework, which reflects the interest rate and wages responses, we find that the overall effects were much more dampened, with aggregate consumption, output, capital, and labor increasing by only 0.40, 0.60, 1.61, and 0.21 percent, respectively. Therefore, ignoring the general equilibrium effects

(2014) examine the 2001 tax rebate episode in the US; Buera, Kaboski and Shin (2012) evaluate the micro-finance programs in India and Thailand.

of the interest rate and wage would overstate the aggregate responses. The reform also features rich dynamics of aggregate variables along the transitional path. In the short run, aggregate consumption drops by 0.14 percent, labor supply increases by 0.55 percent, and investment increases by 2.57 percent. Hence the consumption equivalent welfare increases by 0.18 percent along the transitional dynamics, compared to 0.3 percent in the steady state.

Moreover, we conduct two related counterfactual policy analyses. One analysis is eliminating the tax differences based on firm size by imposing a uniform capital gains tax rate of 10 percent. This counterfactual analysis relates to a large literature on how distortionary policies may reduce aggregate productivity. Restuccia and Rogerson (2008) show that policies distorting prices faced by individuals and firms could lead to large reductions in aggregate output and productivity. Guner, Ventura and Xu (2008) find size-dependent policies could be costly to the economy. Garicano, Lelarge and Van Reenen (2016) and Gourio and Roys (2014) estimate the costs of a size-dependent policy that regulates firms with 50 employees or more in France. In this paper, we find that eliminating distortions created by the size-dependent capital gains tax system would increase aggregate consumption, output, capital, and labor by 1.92, 2.81, 6.89, and 1.10 percentage points. Additionally, in the second counterfactual analysis where we set the capital gains tax rate to zero, aggregate consumption, output, capital, and labor increased by 2.78, 4.50, 12.39, 1.50 percentage points compared to the pre-2014 steady state.

We show that matching micro moments in the data is crucial for our model and for policymakers. If we completely eliminate size thresholds that determine capital gains tax rates, a model without Poisson shocks that does not match our difference-in-differences estimate of the capital elasticity would under-predict the increases in aggregate capital, labor, consumption, and output by 42 percent, 32 percent, 38 percent, and 37 percent, respectively. Therefore, targeting the difference-in-differences

estimate of the capital stock response is crucial for building a general equilibrium model to quantify the aggregate effects of capital gains taxes. Policymakers designing an effective capital tax system may benefit from the implication of our key finding that model-based predictions might severely understate (or overstate) the true aggregate responses if they do not target the micro moments based on both cross-sectional and time-series variation in tax rates.

This paper’s main contribution is twofold. First, to the best of our knowledge, this paper is the first to incorporate both firm-level and time-series variation in capital gains tax rates into a dynamic general equilibrium model, which has the main advantage of identifying the model’s key parameters based on the institutional setting. Second, this paper’s findings contribute to the long-standing academic and policy debates on how much capital taxation affects aggregate investment, providing supporting evidence for a class of the “traditional-view” models predicting that lowering capital tax rates would increase investment by reducing the marginal cost of investment (Feldstein 1970; Poterba and Summers 1983).

Our paper bridges the gap between a strand of studies that rely solely on reduced-form methods to estimate the tax effects on investment, and another strand of structural papers that consider the aggregate responses without fully capturing firm dynamics at the micro-level. By bridging this gap, our model not only matches the short-term firm responses to a change in capital gains tax rates in the partial equilibrium, as we see in the data, but also consistently predicts the long-run aggregate responses that reflect the general equilibrium price effects and dynamic effects from adjustment frictions.

The remainder of the paper is organized as follows. Section 2.2 describes the institutional background for the capital tax system in Korea. In Section 2.3, we present our reduced-form strategy and data to estimate key moments in our model. We describe our model in Section 2.4, present estimation results in Section

2.5, and conduct welfare analyses in Section 2.6. In Section 2.7, we discuss policy implications of our findings. Section 2.8 concludes.

2.2 Institutional Background

This section provides a brief overview of the institutional background on the capital gains tax system and the policy reform in Korea.³ The main institutional features are that the tax rate varies by firm size and that the government unexpectedly changed the regulations on firm size in 2014, reducing the tax rates for firms that became re-classified as small due to the new regulations. Note that we use a conversion ratio of 1000 Korean Won to 1 U.S. dollar throughout our paper to describe the setting and interpret the findings.

In Korea, capital gains tax rates differ mainly based on firm size. An investor in a large firm faces a capital gains tax rate of 24 percent on average, depending on his ownership rate, while an investor in a small firm faces a flat tax rate of 10 percent regardless of his share.⁴ In 2014, the government changed the regulations on firm size, which generated time-series variation in the tax rates within a given firm affected by the rule change. To identify the effects of capital gains taxes on corporate outcomes, we compare the outcomes of firms affected by this reform with the outcomes of unaffected firms for our identification strategy.

Until 2014, the government enforced the following rules for firm size. For the main sectors (manufacturing, construction, production and information services) used in our analysis, a firm has to jointly satisfy the following criteria to be classified as small: total revenues below 100 million dollars and average employees below 300. In 2014, the government unified the regulations on firm size by eliminating

³Additional details on the institutional background can be found in Moon (2018).

⁴In Korea, investors pay capital gains taxes on their realized gains when they sell their stock, whether publicly or privately, and when firms initiate share purchases. More details on the historical capital gains tax rates in Korea can be found at this website: www.nts.go.kr/eng.

the labor threshold and by setting a new threshold, namely, "average revenue" over the past years. The primary intention of the reform was to simplify the rules on firm size. This reform was discussed by government officials and policymakers in early 2014, its approval was announced in August, 2014, and the reform was implemented by the end of 2014; therefore, this policy change came as a shock to affected firms. Moreover, investors did not know which firms were actually affected by this reform until firm size was publicly announced through annual audit reports in the first quarter (March) of 2015. This is evidenced by stock price responses for affected firms, as compared to unaffected firms (Moon 2018). We describe how we use this reform for identification in Section 2.3.

Although the reform eliminated the labor threshold and changed the revenue threshold into an average over the past three years for all sectors, it increased the average revenue threshold to 150 million dollars only for the manufacturing sector. Even though the average revenue threshold did not increase to 150 million dollars for the construction or the production and information service sectors, many firms within these sectors became reclassified as small firms because these sectors had many firms above the labor cutoff, but below the revenue cutoff, prior to the reform. Furthermore, the manufacturing, construction, and production and information services sectors account for about 80 percent of firms in our sample period, and hence we use these as the main sectors.

An important thing to note is that, when measuring a firm's accounting variables, such as revenue and labor size, for tax purposes, a parent firm's accounting values are measured as follows if the firm has at least one subsidiary firm. If the parent firm has at least 50 percent ownership of the subsidiary firm, then the subsidiary's accounting variables are directly added to the parent firm. However, if the parent firm has less than 50 percent ownership, then the subsidiary's accounting variables are added by multiplying their values by the ownership rate. The sub-

subsidiary's accounting variables also account for the parent firm's accounting values in the same way, except the double counting. Moreover, if the parent firm owns another subsidiary through its subsidiary, then the parent firm's ownership of that firm is calculated by multiplying the two ownership rates together.

2.3 Reduced-Form Evidence

This section describes our empirical strategy and data to identify the effects of capital gains taxes on real corporate outcomes. The capital tax system in Korea provides a unique empirical framework, where the capital gains tax rates differ across firms based on firm size. Until the reform in 2014, firm size was mainly determined by the revenue threshold of 100 million dollars and the average employee threshold of 300. If a firm has an incentive to minimize capital gains taxes, then one would expect to see firms sorting below each threshold.

2.3.1 Estimating Tax Effects on Main Outcomes

To identify the tax effects on corporate outcomes, we compare firms that became reclassified as small and experienced a tax reduction of 14 percentage points after the reform in 2014 with unaffected firms. To define the treated and control groups, we exploit the reform on firm size regulations in 2014, which brought three major changes. First, it eliminated the labor threshold, so firms above the labor cutoff but below the revenue threshold experienced a 14 percentage point reduction in their tax rates. Second, the revenue threshold became the average of revenues over the past three years. Lastly, the average revenue cutoff increased from 100 million to 150 million dollars, so firms initially above the original revenue threshold but below the new average revenue cutoff experienced a 14 percentage point drop in

their tax rates.⁵ We define these firms that got a reduction in capital gains tax rates from 24 percent to 10 percent as the main type of treated firms for the main results.

Furthermore, due to this reform, firms below and close to the labor and original revenue cutoffs may face an incentive to increase investment, since there was evidence of bunching at both thresholds. If labor and capital were complementary, then eliminating the labor constraint should provide a similar tax incentive to increase investment as a reduction in the tax rate. Hence, we define these firms that were close to the labor cutoff, but 5 percent below it, as the second type of treated firms.⁶ Similarly, firms that were close to the old revenue cutoff, but 10 percent below it, fall into the second type of treated firms because they were bunching precisely to avoid higher tax rates; so, removing this cutoff may provide an incentive to increase investment.

On the other hand, firms whose size was unaffected by the reform serve as the control group, given that there was no change in their incentive to invest.⁷ Therefore, our main analysis sample consists of the first type of treated firms that experienced a reduction in capital gains tax rates of 14 percentage points, while the control firms were unaffected by the reform because they were above the new threshold and still remained large firms after the reform. We run a separate analysis for the second type of treated (bunching) firms in Appendix B, along with

⁵The new revenue threshold did not increase to 150 million dollars for certain industries within the manufacturing sector and other sectors. Therefore, firms in these excluded industries that were above the initial revenue cutoff, but below the new revenue threshold, are defined as part of the control group. More details on how the reform affected different industries can be found in Moon (2018).

⁶We chose firms 5 percent below the labor cutoff and 10 percent below the revenue cutoff as part of the additional treated group. The reason is that the growth rate of labor size for firms below the labor cutoff was about 5 percent on average, and the growth rate of revenues for firms below the revenue cutoff was about 10 percent on average prior to the reform. Our results are qualitatively similar if we use a larger or smaller sample below each cutoff to define this additional treated group.

⁷Firms that were above, but close to, the new cutoff might have an incentive to decrease investment to go below the threshold. Therefore, we drop 5 percent of firms above the new average revenue cutoff to mitigate this potential issue.

the analysis where we combine both types of treated firms. Figure 2.1 illustrates the reform, and the two types of treated groups and the control group.

To validate our empirical design and visually show the reform effects on firms' real outcomes, we estimate the following model:

$$y_{it} = \sum_{\tau=2009}^{2017} \theta_{\tau} \mathbb{1}[t = \tau] \times Treated_i + \alpha_i + \alpha_t + X_{it}\beta + \epsilon_{it} \quad (2.1)$$

where α_i and α_t are firm and year fixed effects, $Treated_i$ is a dummy equal to 1 if the firm experienced a reduction in capital gains tax rate from 24 percent to 10 percent, and X_{it} is a vector of firm characteristics, which consists of (1) basic controls, such as quartics in firm age and industry dummies interacted with year dummies, and (2) additional controls, such as dummies for each pre-reform (2014) operating profit quintile interacted with dummies for each year.⁸ We include quartics in age to control for baseline financial constraints of firms among treated and control groups. Furthermore, industry composition is different between treated and control firms, so we include industry dummies interacted with year dummies to flexibly control for any time-varying industry-specific shocks. Additionally, to absorb any non-tax trends driven by baseline differences in productivity across groups, we include dummies for pre-reform (2014) operating profits (revenues minus operating costs) quintiles interacted with dummies for each year. We cluster standard errors at the firm-level. Each coefficient θ_{τ} measures the change in the outcome variable y_{it} for

⁸We fix the dummy for $Treated_i$ at the time of the reform. In theory, treated firms in our sample may cross the new threshold within three years after the reform and face a higher capital gains tax rate again, which could attenuate our estimates since they may not increase investment as much as they would have had they remained small throughout the post-reform period. Furthermore, control firms in our sample may go below the new cutoff and face a lower capital gains tax rate, which could also attenuate our estimates, since they may increase investment after a tax cut. If either of these cases were prevalent, then our difference-in-differences estimates would give us a lower bound on the investment elasticity by holding the definition of $Treated_i$ fixed throughout the sample period.

affected firms relative to unaffected firms in the τ -th year before or after the reform became effective in 2014. Note that θ_{2014} is normalized to be zero.

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

$$y_{it} = \alpha + \theta Treated_i \times Post_t + \alpha_i + \alpha_t + X_{it}\beta + \epsilon_{it} \quad (2.2)$$

where $Post_t$ is a dummy equal to 1 if it is after the reform year of 2014, and all the other variables are as defined in equation (2.1). We report the estimates from this equation (2.1), as well as the ones from the equation (2.2) in Section 2.3.

The main identifying assumptions behind our difference-in-differences design is that the affected and unaffected firms' outcomes would have trended similarly in the absence of the policy change. The key threat to this design is that time-varying shocks may coincide with the reform. We present three reasons why this threat is minimal. First, we show that the parallel trend assumption is satisfied prior to the reform. Second, stock price responses show that the reform was unexpected (Moon 2018), and there was no evidence of sorting at the new cutoff for the first three years after the reform. Lastly, we conduct placebo tests defining a reform date with a year prior to the actual reform date and defining treated groups with random cutoff values. We fail to reject the null hypothesis that the effects are not statistically different from zero in each of these tests.

2.3.2 Data and Analysis Sample

For empirical analysis, we use firm-level data on publicly listed and private firms in Korea from 2009 to 2017, where we observe detailed accounting information about the firms. We acquired this data set from a data company called Korea Listed Company Association (KLCA). We focus on the following sectors: (1) Manufacturing,

(2) Construction, and (3) Production and Information Services. We focus on this time period because the rules for determining firm size remained the same, except in 2014. In our sample period, firms in these sectors account for about 90 percent of all publicly listed companies and 80 percent of all private firms. Furthermore, firms in these sectors account for about 80 percent of total revenues in the economy. Moreover, for private firms, expenditures on physical capital assets are easier to measure and observe for these sectors compared with other sectors, such as retail. We run a separate analysis including firms in other sectors and find qualitatively similar results (see Appendix B).

We also use an accounting data set for private firms, which we acquired from another data company called Korea Information Service (KIS). The main difference between this and the other data set is the coverage rate: because private firms report this information only when they have assets worth at least 10 million dollars and are audited by the government, we have missing information on accounting variables for certain firms and for certain years. Another difference is that for private firms, many variables related to firms' capital structure, such as equity issuance or payouts, are missing, so we use private firm data primarily to analyze the tax effects on investment, employment, and total revenues. Finally, we use data on firms' ownership rates of their subsidiaries to compute accounting values for firm size.

2.3.3 Variable Definitions

The main data set contains accounting variables necessary for empirical analysis: assets, revenues, average employees, physical capital (tangible) assets, expenditures on physical capital assets, profits, and total capitals.

The key outcome variables are physical capital assets and investment. We define physical capital assets as the total book value of tangible assets (plants,

properties, and equipment) as they appear in firms' balance sheets. We define investment as the log of the firm's capital expenditures on physical capital assets. We winsorize main outcome variables at the 99 percent level, and do robustness checks by winsorizing main outcomes at the 95 percent level in Appendix B.

2.3.4 Descriptive Statistics

We summarized the main variables, such as revenue, asset, labor size, and capital expenditure in Table 1. There are economically and statistically significant differences in these variables between treated and control firms. An important thing to note is that treated firms' revenues are below 150 million dollars on average, while the control firms' revenues are above 150 million dollars on average. Even though expenditures on physical capital assets are lower for treated firms than for control firms, the difference in their expenditures scaled by lagged physical capital assets is not statistically different from zero.

2.3.5 Results

This subsection shows the results from the estimation of the difference-in-differences models in Section 2.3 and presents additional tests supporting the interpretations of the results.

Panel A in Figure 2.2 plots the coefficients θ_τ , where $\tau \in (2009, \dots, 2017)$, for $\log(\text{investment})$ as in equation (2.1). As the graph shows, the parallel trend on investment between the affected and unaffected firms is satisfied, as the coefficient estimates are close to zero prior to the reform. Moreover, positive and statistically significant coefficients after the year 2014 indicate that lower tax rates induced the affected firms to increase investment.

Panel B in Figure 2.2 plots the coefficients θ_τ , where $\tau \in (2009, \dots, 2017)$, for $\log(\text{tangible assets})$ as in the equation (2.1). As the graph shows, the parallel trend on investment between the affected and unaffected firms is satisfied, as the coefficient estimates are close to zero prior to the reform. Moreover, positive and statistically significant coefficients after the year 2014 indicate that lower tax rates induced the affected firms to increase the size of tangible assets.

Table 2 presents the difference-in-differences estimation results on investment, tangible assets, net investment, and investment rate, using the sample of both listed and private firms. We winsorize (top-code) the main outcomes at the ninety-ninth percentile. Column (1) shows the coefficient is 0.356 for $\log(\text{investment})$, with the 95% confidence interval of (0.223, 0.489), implying that firms that experienced a reduction in capital gains tax rates from 24 percent to 10 percent increased investment by 36 percent, compared to unaffected firms. Column (2) shows the coefficient is 0.087 for $\log(\text{tangible assets})$, with the 95% confidence interval of (0.01, 0.167), implying that firms that experienced a drop in capital gains tax rates from 24 percent to 10 percent increased tangible assets by 9 percent, compared to unaffected firms.

We compute the implied capital stock elasticity with respect to the net of tax rates in the following way:

$$\epsilon_{y,1-\tau} = \frac{\% \Delta y}{\% \Delta(\text{net of tax rate})} = \frac{\Delta y}{y_0} * \frac{(1 - \tau_0)}{\Delta \tau} \quad (2.3)$$

The estimated elasticity is 0.47, which implies that a one percent increase in the net of tax rate would increase physical capital stocks by a half percent. Our results are consistent with a class of the “traditional-view” models (Feldstein 1970; Poterba and Summers 1983) that lowering capital tax rates would induce investment by increasing the marginal returns on investment.

2.3.6 Robustness and Internal Validity

We conduct several robustness checks to strengthen the internal validity of our results. First, we repeat the main analysis in equation (2.2) without any basic or additional controls and with only basic controls and find qualitatively similar results. Second, we repeat the analysis using different levels of winsorizing and find that the results are quantitatively similar when winsorizing at the 95 percent level. Third, we repeat the main analysis using a balanced panel and find results that are qualitatively similar. Fourth, we repeat the main analysis by including firms in other sectors and find results that are qualitatively similar. Results from these robustness tests are reported in Appendix B.

A potential threat to the internal validity of our empirical strategy is that contemporary changes to other tax policies might affect the results. To account for this potential bias, we conduct a placebo test defining the reform year as the year 2012, instead of the year 2014, and fail to reject the null hypothesis that the effects on the main outcomes are not statistically different from zero. We also conduct another placebo test defining treated firms with random cutoff values and fail to reject the null hypothesis that the effects are not statistically different from zero. Results from these placebo tests are included in Appendix B.

2.4 Model

In this section, we build a dynamics general equilibrium model with heterogeneous firms based on a framework by Gourio and Miao (2010). We extend their model by incorporating (1) our institutional feature that firms face discrete average capital gains tax rates based on their firm size, (2) lumpy investment, and (3) a productivity process with Poisson shocks as in Midrigan (2011).

2.4.1 Households

Time is discrete, and a representative household has an additive utility function in consumption and labor supply:

$$\sum_{t=0}^{\infty} \beta^t \left(\frac{C_t^{1-\sigma}}{1-\sigma} - \omega \frac{L_t^{1+\nu}}{1+\nu} \right) \quad (2.4)$$

where β is the discount rate, C_t denotes consumption, L_t denotes labor supply, σ is the risk aversion, ω is the disutility from labor, and ν is the inverse of Frisch labor supply elasticity.

The household (1) purchases share θ_{jt} at price P_{jt} , (2) receives share repurchases s_{jt} and capital gains $P_{jt} - P_{jt-1}$ from a fixed continuum of firms $j \in [0, 1]$, (3) purchases a risk-free bond B_t with return r_t , and (4) supplies labor at wage rate w_t .⁹ The household also needs to pay income taxes τ_i on labor income and bond returns, and capital gains tax τ_{jt}^g and receives government lump-sum transfer T_t . The household's budget constraint is:

$$\begin{aligned} & C_t + \int P_{jt} \theta_{jt+1} dj + B_{t+1} \\ &= \int \left[P_{jt-1} + (1 - \tau_{jt}^g)(s_{jt} + P_{jt} - P_{jt-1}) \right] \theta_{jt} dj + (1 + (1 - \tau^i)r_t)B_t + (1 - \tau^i)w_t L_t + T_t \end{aligned} \quad (2.5)$$

Note that $s_t < 0$ means new equity issuances by firms. Also, the capital gains tax rate depends on firm size owned by the household. The details are specified below.

The household's intra-temporal condition in consumption and labor is:

⁹In Korea, the top marginal dividend tax rate is 38%, which is higher than the top capital gains tax rate of 24%, but firms still pay dividends along with share repurchases. Since our main focus is the effects of capital gains taxes on investment dynamics, we do not include dividend payments in our model. Incorporating dividends into our model will not qualitatively change our model predictions or results.

$$(1 - \tau^i)w_t = \omega C_t^\sigma L_t^\nu$$

The risk free bond holding condition is:

$$1 = \beta E_t \left[\left(\frac{C_{t+1}}{C_t} \right)^{-\sigma} (1 + (1 - \tau^i)r_t) \right]$$

The firm share θ_t holding condition is:

$$P_t = \beta E_t \left[\left(\frac{C_{t+1}}{C_t} \right)^{-\sigma} (P_{t+1} + (1 - \tau_t^s)(s_t + P_t - P_{t+1})) \right]$$

In a stationary equilibrium without aggregate shocks, aggregate consumption stays constant $C_{t+1} = C_t$. Hence, the combination of the two conditions above yields the following required return on the firm share

$$(1 - \tau^i)r = \frac{1}{P_t} (1 - \tau_{jt+1}^s) E_t [s_{jt+1} + P_{jt+1} - P_{jt}]. \quad (2.6)$$

In equilibrium, the household holds all shares of the firms $\theta_{jt} = 1$ and zero bonds $B_t = 0$.

2.4.2 Firms

Technology and Capital Gains Taxes

There is a continuum of firms $j \in [0, 1]$ in the economy. A firm j produces output y_{jt} with Cobb-Douglas technology:

$$y_{jt} = z_{jt} k_{jt}^{\alpha_k} l_{jt}^{\alpha_l},$$

where y_{jt} , z_{jt} , k_{jt} and l_{jt} denote output, productivity, capital, and labor, respectively. α_k and α_l denote capital and labor elasticity of production. We assume that the

productivity follows an AR(1) process, and innovations to productivity arrive infrequently as in Midrigan (2011) and Bachmann and Bayer (2014) with a Poisson probability p_z :

$$\log z_{jt} = \begin{cases} \rho_z \log z_{jt-1} + \varepsilon_{jt}, & \varepsilon_{jt} \sim N(0, \sigma_z) \quad \text{with probability } p_z \\ \log z_{jt-1} & \text{with probability } 1 - p_z \end{cases} \quad (2.7)$$

A firm can invest i_{jt} in capital for the next period's capital:

$$k_{jt+1} = (1 - \delta)k_{jt} + i_{jt}, \quad (2.8)$$

where δ is the physical capital depreciation rate. In the beginning of the period, it draws ψ_0 from a distribution $G(\psi_0)$ and has to pay a fixed cost proportional to the capital stock $\psi_0 k_{jt}$ for non-zero investment $i_{jt} \neq 0$. Also it has to pay a quadratic adjustment cost of capital $\frac{\psi_1}{2} \left(\frac{i_{jt}}{k_{jt}} \right)^2 k_{jt}$. The firm faces a linear corporate tax rate τ_c on its profit and receives a fraction $\hat{\delta}$ of its capital stock for depreciation allowances. In sum, the firm's budget constraint is:

$$s_t + i_{jt} + \psi_0 \mathbb{1}_{i_{jt} \neq 0} + \frac{\psi_1}{2} \left(\frac{i_{jt}}{k_{jt}} \right)^2 k_{jt} = (1 - \tau^c)(y_{jt} - w_t l_{jt}) + \tau^c \hat{\delta} k_{jt}. \quad (2.9)$$

Size-dependent Capital Gains Tax Rates

The capital gains tax rate faced by the household is firm-size specific. Holding a share in a small firm is associated with a low average tax rate τ_l^g , and in a large firm is associated with a high average rate τ_h^g . A firm is categorized as small if it jointly satisfies two criteria: (1) total revenue below $\bar{y}$ and (2) number of employees below $\bar{l}_E$.¹⁰ The tax schedule can be summarized as:

¹⁰Since we do not observe firm-specific prices, we use revenue and output interchangeably in this paper.

$$\tau_{jt+1}^g = \begin{cases} \tau_h^g & \text{if } l_{jt} > \bar{l}(z_{jt}, k_{jt}) \equiv \min\{\bar{l}_R(z_{jt}, k_{jt}), \bar{l}_E\} \\ \tau_l^g & \text{otherwise,} \end{cases} \quad (2.10)$$

where $\bar{l}_R(z_{jt}, k_{jt}) \equiv \left(\frac{\bar{y}}{z_{jt} k_{jt}^{\alpha_k}} \right)^{\frac{1}{\alpha_l}}$.

Optimization Problem

Let $V(z, k, \tau_g, \psi_0)$ be the value of a firm entering the period with productivity z , capital stock k , capital gains tax rate τ_g , and a draw of fixed cost ψ_0 and define it as the sum of net share repurchases and equity value:

$$V(z, k, \tau_g, \psi_0) = s + P$$

Using equation (2.6), the firm's dynamic problem can be written as the following:

$$V(z, k, \tau^g, \psi_0) = \max_{l, i, s} s + \frac{1}{1 + \frac{(1-\tau^i)r}{1-\tau^{g'}}} E_t [V(z', k', \tau^{g'}, \psi'_0)], \quad (2.11)$$

subject to (2.9), and (2.10). From the equation above, we see that the capital gains tax rate does not distort a firm's intra-temporal decision; hence, we drop τ^g from the state variables: $V(z, k, \tau^g, \psi_0) = V(z, k, \psi_0)$.

Firm's problem (2.11) can be rewritten as the following:

$$V(z, k, \psi_0) = \max\{V_N(z, k), V_A(z, k) - \psi_0 k\},$$

in which the firm decides to pay the fixed cost or not. $V_N(z, k)$ denotes the value of a firm not paying the fixed cost for capital adjustment:

$$V_N(z, k) = \max_{l \in \{\bar{l}(z, k), l^*(z, k)\}} (1 - \tau^c) (zk^{\alpha_k} l^{\alpha_l} - wl) + \tau^c \hat{\delta} k + \frac{1}{1 + \frac{(1-\tau^i)r}{1-\tau^{\delta'}(l)}} E_t \left[V(z', (1-\delta)k, \psi'_0) \right], \quad (2.12)$$

where $l^*(z, k)$ is the firm's unconstrained labor choice:

$$\begin{aligned} l^*(z, k) &\equiv \arg \max_l zk^{\alpha_k} l^{\alpha_l} - wl \\ &= \left(\frac{\alpha_l}{w} zk^{\alpha_k} \right)^{\frac{1}{1-\alpha_l}}. \end{aligned}$$

A firm always chooses $l^*(z, k)$ that maximizes the current profit flow if it is not above the size threshold $\bar{l}(z, k)$. Otherwise, it needs to decide between $l^*(z, k)$ and constrained labor $\bar{l}(z, k)$ that results in the low capital gains tax rate.

If the firm chooses to pay the fixed cost, its value function becomes:

$$\begin{aligned} V_A(z, k) &= \max_{i, l \in \{\bar{l}(z, k), l^*(z, k)\}} (1 - \tau^c) (zk^{\alpha_k} l^{\alpha_l} - wl) + \tau^c \hat{\delta} k - i - \frac{\psi_1}{2} \left(\frac{i}{k} \right)^2 k \\ &\quad + \frac{1}{1 + \frac{(1-\tau^i)r}{1-\tau^{\delta'}(l)}} E_t \left[V(z', (1-\delta)k + i, \psi'_0) \right]. \end{aligned} \quad (2.13)$$

After paying the fixed cost, the firm can choose any value of investment subject to quadratic costs to maximize its discounted present value. It also chooses labor between $\bar{l}(z, k)$ and $l^*(z, k)$.

The firm would choose to make non-zero investment if and only if the benefit of making adjustments is higher than paying the fixed cost. There is a unique threshold $\hat{\psi}_0(z, k)$ that satisfies:

$$\hat{\psi}_0(z, k) = \frac{V_A(z, k) - V_N(z, k)}{k}.$$

If the fixed cost draw ψ_0 is smaller than $\hat{\psi}_0$, then the firm is willing to pay for capital adjustments. Note that this threshold is increasing in the gap between the current capital stock and desired capital level. If the current capital is close to the desired level, a firm would not change investment. This feature generates lumpy investment behavior in the model.

2.4.3 Comparative Statics

To illustrate how the capital gains taxes affect investment in the model, we first make the following simplifying assumptions:

1. Physical and depreciation-allowance capital depreciate fully in one period $\delta = \hat{\delta} = 1$.
2. Idiosyncratic productivity takes two states $\{\bar{z}, \underline{z}\}$, and transition probability is $\Pr(z = \bar{z}|z = \bar{z}) = \Pr(z = \underline{z}|z = \underline{z}) = \rho$.
3. We assume that if $z = \bar{z}$, the firm is categorized as a large firm and faces a high capital gains tax rate, and vice versa.
4. Firm always chooses flow profit maximization labor $l^*(z, k)$. Hence the firm's flow profit $\pi(z, k)$ is

$$\pi(z, k) \equiv (1 - \alpha_l) \left(\frac{\alpha_l}{w} \right)^{\frac{\alpha_l}{1-\alpha_l}} (zk^{\alpha_k})^{\frac{1}{1-\alpha_l}} \quad (2.14)$$

5. Firm chooses investment before observing the current productivity. This is a simple way to capture the capital adjustment cost.

For a firm with productivity $z = \bar{z}$ and capital stock k in the last period, its maximization problem is

$$V(k, \bar{z}) = \max_i \left\{ \rho \left\{ \pi(k, \bar{z}) - i + \left(1 + \frac{(1 - \tau_i)r}{1 - \tau_h^g} \right)^{-1} E[\pi(i, z)|\bar{z}] \right\} \right. \\ \left. + (1 - \rho) \left\{ \pi(k, \underline{z}) - i + \left(1 + \frac{(1 - \tau_i)r}{1 - \tau_l^g} \right)^{-1} E[\pi(i, z)|\underline{z}] \right\} \right\}. \quad (2.15)$$

Then the investment elasticity with respect to the net of capital gains tax rate $(1 - \tau_h^g)$ is

$$\epsilon_{i, 1 - \tau^g} \equiv \frac{\partial i}{\partial (1 - \tau_h^g)} \frac{1 - \tau_h^g}{i} \\ = \underbrace{\left(1 - \frac{\alpha_k}{1 - \alpha_l} \right)^{-1}}_{\text{profit curvature effect}} \underbrace{\frac{(1 - \tau_i)r}{(1 - \tau_i)r + 1 - \tau_h^g}}_{\text{user cost of capital effect}} \underbrace{\frac{\frac{\rho}{R_h} E[z^{1/(1 - \alpha_l)}|\bar{z}]}{\frac{\rho}{R_h} E[z^{1/(1 - \alpha_l)}|\bar{z}] + \frac{1 - \rho}{R_l} E[z^{1/(1 - \alpha_l)}|\underline{z}]}}_{\text{size threshold effect}}, \quad (2.16)$$

where $R_h \equiv 1 + \frac{(1 - \tau_i)r}{1 - \tau_h^g}$ and $R_l \equiv 1 + \frac{(1 - \tau_i)r}{1 - \tau_l^g}$.

The equation 2.16 shows that the investment elasticity with respect to the net of tax rate is always positive and can be further decomposed into three parts. The first part is the *profit curvature effect*. A more concave profit function of capital yields a smaller response of investment, which could be due to decreasing returns-to-scale technology or a downward-sloping demand curve.¹¹ The second term is *user cost of capital effect*. As the required return $(1 - \tau_i)r$ increases, a firm's investment policy would also be more responsive to a change in capital gains taxes. The last one is *size threshold effect*, which is increasing in ρ . Hence, if a large firm today is more likely to remain large in the next period, it would increase investment more from a tax cut. It turns out that the last effect is significant in our dynamic heterogeneous firm model and sharply disciplines parameters related to

¹¹Note that a model with equity issuance costs would generate an inactive region of firms not responding to changes in capital gains tax rates. Studying the interaction of financial constraints and capital gains taxes would be an interesting addition to this paper.

the productivity process. Not controlling for this effect would yield different results and have different implications for policy analysis.

2.4.4 Stationary Competitive Equilibrium

A stationary competitive equilibrium consists of (i) invariant joint distribution of idiosyncratic productivity and capital $F(z, k)$, (ii) firm policy functions $l(z, k)$ and $i(z, k)$, and (iii) household wage w and consumption C such that

1. $l(z, k)$ and $i(z, k)$ solve firm's maximization problem;

2. Labor market clears

$$L(w, C) = \int l(z, k) dF(z, k) \quad (2.17)$$

3. Aggregate Output

$$Y = \int y(z, k) dF(z, k); \quad (2.18)$$

4. Aggregate Investment

$$I = \int i(z, k) dF(z, k); \quad (2.19)$$

5. Aggregate Adjustment Cost

$$\Psi = \int \psi_0 \mathbb{1}_{i(z, k) \neq 0} k dG(\psi_0) dF(z, k) + \int \frac{\psi_1}{2} \left(\frac{i(z, k)}{k} \right)^2 k dF(z, k); \quad (2.20)$$

6. Government Budget Constraint

$$T = \tau^i w \int l(z, k) dF(z, k) + \tau^c \int (y(z, k) - wl(z, k) - \delta k) dF(z, k) + \int \tau^g(z, k) s(z, k) dF(z, k); \quad (2.21)$$

7. Aggregate Consumption

$$C = Y - I - \Psi; \quad (2.22)$$

2.5 Calibration and Simulation

To quantify the effects of reducing capital gains taxes, we calibrate parameters in two steps. First, we externally calibrate a subset of parameters by adopting commonly used values in the literature. Second, we calibrate the rest of the parameters by matching micro moments from our reduced-form analysis, in addition to important aggregate moments from our firm-level data from 2009 to 2017.

2.5.1 Externally Calibrated

The model period is one year. We choose the risk-free interest rate to be 4 percent, which implies from the household's inter-temporal condition that the discount rate is equal to 0.97. We set the constant relative risk aversion σ to 1, meaning that the household has log utility in consumption. We also set the Frisch elasticity of labor supply to be 1 as suggested by Chang et al. (2018) for a representative household model. Labor disutility parameter ω is chosen such that the aggregate labor supply is 1/3 in the steady state. In order to evaluate the firm-size reform in 2014, we set the income tax rate $\tau^i = 25$ percent, corporate tax rate $\tau^c = 34$ percent, and low (high) capital gains tax rates as 10 percent (24 percent), consistent with the tax rates in Korea. For capital depreciations, we set the physical rate and allowance rate to be the same value of 10 percent, implying that the aggregate investment rate is 10 percent. Lastly, we set the capital production elasticity α_k and labor production elasticity α_l to be 0.28 and 0.57, such that the capital-labor ratio is 0.5 and total returns-to-scale is 0.85.

2.5.2 Internally Calibrated

The rest of the parameters are jointly calibrated using a simulated method of moments to minimize the distance between empirical moments and simulated

moments. We choose a uniform distribution $[0, \bar{\psi}_0]$ for the fixed-cost distribution. Hence, the internally calibrated parameters are (1) the upper bound of fixed-cost distribution $\bar{\psi}_0$, (2) the quadratic adjustment cost ψ_1 , (3) the persistence of productivity ρ_z , (4) the standard deviation of productivity shock σ_z , (5) a Poisson probability of shock arrival p_z , (6) the revenue threshold $\bar{y}$, and (7) the employee threshold $\bar{l}_E$.

Table 4 reports the data and simulated moments. We select the following seven moments as informative about the parameters (1) to (5): a fraction of the absolute investment to capital ratio less than 5 percent (inaction rate) and larger than 20 percent (spike rate); standard deviation of the investment rate; standard deviation of $\log(\text{output})$; output autocorrelation of one, three, and five years; and the reduced-form estimates of the changes in capital stocks for affected firms relative to unaffected firms after the reform. For the size thresholds $\bar{y}$ and $\bar{l}_E$, we match the percentiles of thresholds to compare our data with the model for policy evaluations.

All parameters are estimated jointly; hence, it is hard to give a one-to-one mapping between parameters and moments. However, intuitively, the investment rate moments discipline the adjustment costs, revenue moments discipline the AR(1) productivity process, and the difference-in-differences estimate of the capital elasticity pins down the Poisson probability through the size-threshold effect. We use short and long horizons of output autocorrelations to distinguish between two seemingly similar productivity processes.

Table 5 reports the estimated parameters. We find low estimates of fixed adjustment costs, equivalent to 0.25 percent of capital stock for adjustments, while the estimate of Cooper and Haltiwanger (2006) is 3.9 percent. The Poisson probability of new productivity shock is $p_z = 0.33$. This estimate is comparable to 0.41 in

Bachmann and Bayer (2014) and much higher than 0.03 in Midrigan (2011).¹² The estimated quadratic cost is 0.15, comparable to 0.05 in Cooper and Haltiwanger (2006). Finally, the persistence of an AR(1) productivity process is 0.87 and the standard deviation of shocks is 0.10, comparable to the findings in the literature.

Lastly, although not targeted, the baseline model yields a high kurtosis for investment rate (7.5) and output growth rate (9.6), comparable to 7.4 and 10.3 in the data, respectively. This is because the productivity process with the Poisson shocks itself has a high kurtosis. In contrast, the Gaussian model yields only kurtosis of 3.7 for the investment rate and 4.7 for the output growth rate, close to the kurtosis of a normal distribution.

2.5.3 Inspecting the Mechanism of Poisson Shocks

To explain the role of the Poisson shock in our model, we use the following illustration. First, we recalibrate a model without the Poisson shock and report the model fits in Table 4 along with the baseline case. We refer to it as the Gaussian ($p_z = 1$) case. We find that the model could match all the micro moments well except for the difference-in-differences estimate of the capital stock response. The Gaussian case yields a much smaller response of the treated firms, 0.03, compared to 0.09 that we estimated in the data.

The smaller response in the Gaussian case is due to the size threshold effect in the model. We illustrate this point using a heuristic example in Figure 2.3. The two panels show the distribution of firm size in the next period, conditional on today's firm size being 0. The left and right panels show cases when the shock to productivity is Gaussian ($p_z = 1$) and Poisson ($p_z < 1$), respectively. $\bar{l}$ is the size threshold for the capital gains tax rates. A firm faces the low tax rate when

¹²Bachmann and Bayer (2014) assume a mixture of Gaussian productivity shocks in a real business cycle model with heterogeneous firms and lumpy investment decisions. Midrigan (2011) identifies the Poisson probability with cross-sectional firm price moments.

downsizing below $\bar{l}$ and the high tax rate otherwise. We see that the distribution of the Gaussian shock is more dispersed while the Poisson shock has more mass concentrated in the mean (more "peakedness"). Therefore there is a larger shaded area below $\bar{l}$ in the left panel, meaning a low probability of staying large. This corresponds to the low persistence parameter ρ in equation (2.16). Hence, the Poisson model has a stronger size-threshold effect and is able to match the large difference-in-differences estimate from our reduced-form analysis.

For a more quantitative explanation, we consider the following two economies: Gaussian economy and Poisson economy. In a Gaussian economy, a firm receives the following Gaussian shock and its next period size evolves as the following:

$$l' = l + \varepsilon_g, \quad \varepsilon_{jt} \sim N(0, \sigma_g) \quad (2.23)$$

Alternatively, in a Poisson economy, a firm receives a Gaussian shock with probability p_z :

$$l' = \begin{cases} l + \varepsilon_p, & \varepsilon_{jt} \sim N(0, \sigma_p) \quad \text{with probability } p_z \\ l & \text{with probability } 1 - p_z, \end{cases} \quad (2.24)$$

We normalize the current firm size to $l = 0$ and restrict the variance of the two economies to be the same, which is similar to matching the same standard deviation of output in our calibration. Then we have

$$\sigma_p = \frac{\sigma_g}{\sqrt{p_z}}. \quad (2.25)$$

Note that the kurtosis of the Gaussian shock is 3, while the Poisson shock is $\frac{3}{p_z}$. In Figure 2.4, we plot the probability ratio $\Pr(\varepsilon_p < \bar{l})/\Pr(\varepsilon_g < \bar{l})$ for different values of p_z . We also plot for different values of labor threshold $\bar{l}$. We see that the probability ratio is a monotonically increasing function of p_z for the range of $\bar{l}$. Furthermore, for

a given value of p_z , the probability ratio increases with $\bar{l}$. This is because the Poisson shock has “fatter tails” than the Gaussian shock. As the threshold $\bar{l}$ gets far away from the mean, the effect of “fatter tails” starts dominating that of “peakedness.” In general, either “fatter tails” or “peakedness” could dominate. Nevertheless, the latter effect dominates in our simulation with an estimated Poisson probability $p_z = 0.33$. In the Poisson economy, the probability of a firm above the threshold today going below the threshold after 1, 3, and 5 years are 7.72%, 20.90%, and 31.75%, respectively. The corresponding probabilities in the Gaussian economy are 9.91%, 24.29%, and 35.27%, respectively. In terms of the ratio of the probability, the differences between the two economies are 28.37%, 16.22%, and 7.69% over 1-, 3-, and 5-year horizons. Therefore the size threshold effect on the capital response of treated firms is much larger in the Poisson economy, which is more consistent with our data, than in the Gaussian economy.

2.6 Transitional Dynamics and Welfare Implications

Now we present the results of transitional dynamics under alternative policies on firm size thresholds that determine the capital gains tax rates. The initial steady state is in 2014, as it was in Korea, where firms with fewer than 300 employees and less than 100 million dollars in revenues face the capital gains tax rate of 10 percent, and 24 percent otherwise. We focus on three different policy experiments of permanent tax reforms, which occur at period 2, and are unexpected for the household and the firms in the economy.

2.6.1 Firm Size Reform in 2014

We start with the case of the firm size reform. In 2014, the government removed the labor threshold and increased the revenue threshold from 100 million dollars

to 150 million dollars. Since the actual reform affected only a small fraction of firms in the economy, we target the difference-in-differences estimate of the capital stock response in a partial equilibrium for our baseline calibration. For the first experiment, we extend the scope of the reform to the whole economy and study the aggregate effects.

Figure 2.5 presents the transitional dynamics of wage, interest rate, consumption, capital, investment, output, labor, TFP, equity issuance, and share repurchase. In general equilibrium, aggregate investment jumps by 2.6 percent in the short run. An increase in the interest rate induces the representative household to save more by decreasing consumption and increasing labor supply for capital investment. Hence, the effect of the capital gains tax cut on investment is larger in the short run. Since capital is the accumulated investment net of depreciation, it increases gradually to the steady state. Finally, firms finance investment by cutting back on share repurchases and issuing new equity in the first few periods.

Table 8 summarizes the long-run percent changes of the aggregate variables. In the steady state, aggregate capital, labor, output, consumption, and wage increase by 1.61%, 0.21%, 0.60%, 0.40%, and 0.61% respectively, while TFP does not change much. The increase in the output comes mostly from the newly accumulated capital by the firms. The total labor supply goes up by much less than suggested by the increase in the wage, because of the wealth effect through consumption. Also, since only a small fraction of firms were affected by the 2014 reform, there is not much resource reallocation among firms and TFP does not change much.

The analysis above considers the case of full general equilibrium, where both wage and interest rate are flexible. Here, we also consider the case of small open economy, a suitable case for Korea. In this exercise, we keep the interest rate fixed and the consumption is determined by the inter-temporal budget constraint. Figure 2.5 shows the results labeled by SOE (Small Open Economy). Since the

interest rate is constant, the consumption jumps instantly to and stays flat at the new level. Due to the wealth effect, the short-run labor supply does not increase as much as in the general equilibrium case. Also, we see that the short-run investment is more volatile in the case of SOE, since firms have stronger incentives to increase their investment with a fixed interest rate. The aggregate investment increases by 7.19 percent right after the tax cut, compared to 2.6 in the general equilibrium case. Table 9 summarizes the long-run percent changes of the aggregate variables. In the long run, the capital, output, and labor are slightly higher than their counterparts in the general equilibrium. This is due to the over-accumulation of capital from investment spikes right after the reform.

Partial Equilibrium vs. General Equilibrium

The analysis of the transitional dynamics above accounts for general equilibrium (GE) effects through the interest rate and wage. The partial equilibrium (PE) analysis reveals that ignoring aggregate prices would yield results with large differences.

For this exercise, we keep both the interest rate and wage fixed at the initial level. In the short run, Tables 6 and 7 show that aggregate investment and capital increase by 14.37 and 1.44 percent, respectively, in the partial equilibrium. These responses are much higher than the ones from both general equilibrium and small open economy cases. Although the interest rate is kept fixed for both PE and SOE cases, the initial increase in the wage dampens firms' investment responses. In the new steady-state, the PE analysis also overstates the increase in consumption, capital, output, and labor by 2.00 (2.09), 1.94 (1.88), 1.99 (1.93), and 2.56 (2.49) percentage points than the GE (SOE) case, respectively. Firms in the GE and SOE cases face more expensive labor inputs, so they do not invest as much as the firms in the PE case. Overall, we see that the results of alternative cases of partial equilibrium look dramatically different from the general equilibrium.

Welfare Changes

We now calculate the welfare changes of the firm-size reform in 2014. First, we define the consumption-equivalent welfare changes Δ_{SS} between two steady states (C^*, L^*) and (C^{**}, L^{**}) as the following:

$$\frac{((1 + \Delta_{SS})C^*)^{1-\sigma}}{1 - \sigma} - \omega \frac{L^{*1+\nu}}{1 + \nu} = \frac{C^{**1-\sigma}}{1 - \sigma} - \omega \frac{L^{**1+\nu}}{1 + \nu}. \quad (2.26)$$

However, the measure above does not account for the potential welfare loss along the transitional dynamics. In the 2014 reform, the representative household has to decrease consumption and increase labor supply in the short-run to accumulate higher capital stock in the long run, meaning that Δ_{SS} might overstate the actual welfare gain. Therefore, we also define the consumption-equivalent welfare changes along a transitional path $(C_t, L_t)_{t=1,2,\dots,\infty}$ as the following:

$$\frac{1}{1 - \beta} \left[\frac{((1 + \Delta_{TD})C^*)^{1-\sigma}}{1 - \sigma} - \omega \frac{L^{*1+\nu}}{1 + \nu} \right] = \sum_{t=1}^{\infty} \beta^{t-1} \left(\frac{C_t^{1-\sigma}}{1 - \sigma} - \omega \frac{L_t^{1+\nu}}{1 + \nu} \right) \quad (2.27)$$

The results are in Table 8 and Table 9, indicating that the steady state welfare gain is 0.30% in GE and 0.16% in SOE. In the last row of Table 8, we see that the transitional welfare gain in GE is 0.18% and is around 60% of the steady-state gain (0.30%). But, in Table 9, we see that the transitional welfare gain in SOE is similar to the steady-state gain due to the perfect consumption smoothing with the world interest rate and free capital flow.

How large are these welfare changes? To put our estimates of welfare gains into perspective, we compare our results with other findings. Lucas (1987) finds that welfare gains from eliminating business cycles from a representative agent economy with log utility is 0.008 percent. A recent study by Krusell et al. (2009) estimates a heterogeneous agent model and finds that the business cycle welfare costs range from 0.1% to 1%. Although the 2014 reform affected only a subset of

firms in the economy, its welfare implication is comparable to eliminating business cycle risks in the economy.

2.6.2 Uniform and Zero Tax Rate

We conduct two relevant counterfactual analyses. The first case is, if the government removed both revenue and labor thresholds completely in 2014, what would have been the alternative outcome of the economy? The steady-state results are in the second column of Table 8. Aggregate capital, labor, output, consumption, and welfare would have increased by 6.89%, 1.10%, 2.81%, 1.92%, and 1.34%, respectively. TFP increases by 0.26% since firm-size distortions from different capital gains tax rates disappear, accounting for 9% of the increase in output. The transitional dynamics are presented in Figure 2.7. The dynamics exhibit a pattern similar to the post-2014 reform: in the short run, consumption drops and rises gradually to the new steady state. Investment and labor jump and gradually fall to the new levels. The transitional welfare gain of this reform is 0.84%. Again, as in Figure 2.8, the prediction would be rather different if general equilibrium effects were not considered.

In the last policy experiment, we set the capital gains tax rate to zero in the economy. Capital, labor, output, consumption, and welfare increase by 12.39%, 1.50%, 4.50%, 2.78%, and 1.99%, respectively. But TFP gain does not change much from the second policy experiment, since there is no redistribution effect from the uniform tax case to the zero tax case.

2.7 Relevance of Micro-Moments for Policy Implications

In this section, we explore whether successfully capturing the micro moments in the data is important for evaluating the aggregate effects of reducing capital gains taxes. In the baseline model, Poisson shocks, high productivity persistence, and capital adjustment costs are important for the model to match the investment rate moments, output moments, and the difference-in-differences estimate of the capital stock elasticity in the partial equilibrium. Here, we explore how quantitatively important they are for policymakers aiming to calculate the aggregate impacts of changing size thresholds that determine capital gains tax rates in the general equilibrium framework.

To illustrate the importance of matching micro moments, we compare three different cases. The first case is the baseline model with Poisson shocks. The second case is the gaussian model as calibrated in section 2.5. The gaussian model is able to match the investment and output moments, but not the capital stock response of the treated firms. The last one is the limited information case, in which we consider a hypothetical scenario that policymakers have observations only before the 2014 reform. Since there is no natural policy experiment in the limited information case, policymakers would not observe the capital stock response of firms affected by the reform, thereby underweighting the output persistence moments for calibration criteria to capture the size threshold effect. In terms of modeling, policymakers would assume a simple AR(1) process with Gaussian shocks for firm productivity. For calibration, they would choose to match only the following moments: inaction rate, spike rate, standard deviation of investment rate, and standard deviation of output.

The results for the targeted moments are listed in the last column of Table 4, and the calibrated parameters are listed in Table 5.¹³ We see that the limited information case implies very low autocorrelations of output in the long run. The 3-year and 5-year autocorrelations are only 0.71 and 0.54, compared with 0.93 and 0.88 in the data. Also, the capital response of treated firms is only one-tenth of the empirical estimate. Matching the high persistence of output is important, but not enough to generate the large capital response that we see in the data. The Gaussian case previously discussed, though successfully matching long-run autocorrelation of output, yields less than one-third of the capital stock response. Only the baseline case that features both Poisson shocks and high persistence of output could match the high elasticity. This comparison further demonstrates that the “peakedness effect” of Poisson shocks is essential to reduce the size-threshold effect.

But how quantitatively important is matching micro-moments for aggregate implications? In Figure 2.11, we plot the transitional dynamics of the aggregate variables for baseline, gaussian, and limited information cases in general equilibrium. We see that the gaussian model would under-predict the total capital, labor, consumption, and output by 9%, 8%, 10%, and 9% in the new steady state. We also compare the baseline with the limited information case. We see that not accounting for the persistence of output would under-predict the total capital, labor, consumption, and output by 42%, 32%, 38%, and 37%, respectively, in the steady state.

The reason that the limited information case predicts much smaller increases in aggregate variables than do the baseline and the gaussian cases is due to the low persistence of the productivity and its implication for firm-level capital accumulation. Table 10 shows the output, labor, and capital share of the firms above both

¹³Clementi and Palazzo (2016) show that persistence of idiosyncratic productivity is weakly identified by using investment moments alone, so we exogenously choose $\rho_z = 0.8$, a value commonly used in the lumpy investment literature.

revenue and labor thresholds in the pre-reform period. Although three models imply the same output and labor share, but the capital shares are 0.44 and 0.43 for the baseline and gaussian cases, respectively, much higher than the capital share of 0.26 in the limited information case. Since the productivity is less persistent, combined with costly capital adjustments, large firms in the limited information case tend to accumulate less capital. Although three cases are calibrated to match the same standard deviation of log output, the standard deviation of capital in the baseline and gaussian cases are 50% larger than the limited information case. Since the aggregate effect of the uniform tax rate depends on the share of capital distorted by the high capital gains tax rate, the limited information case shows smaller responses to the tax cut compared to the responses in the other cases.

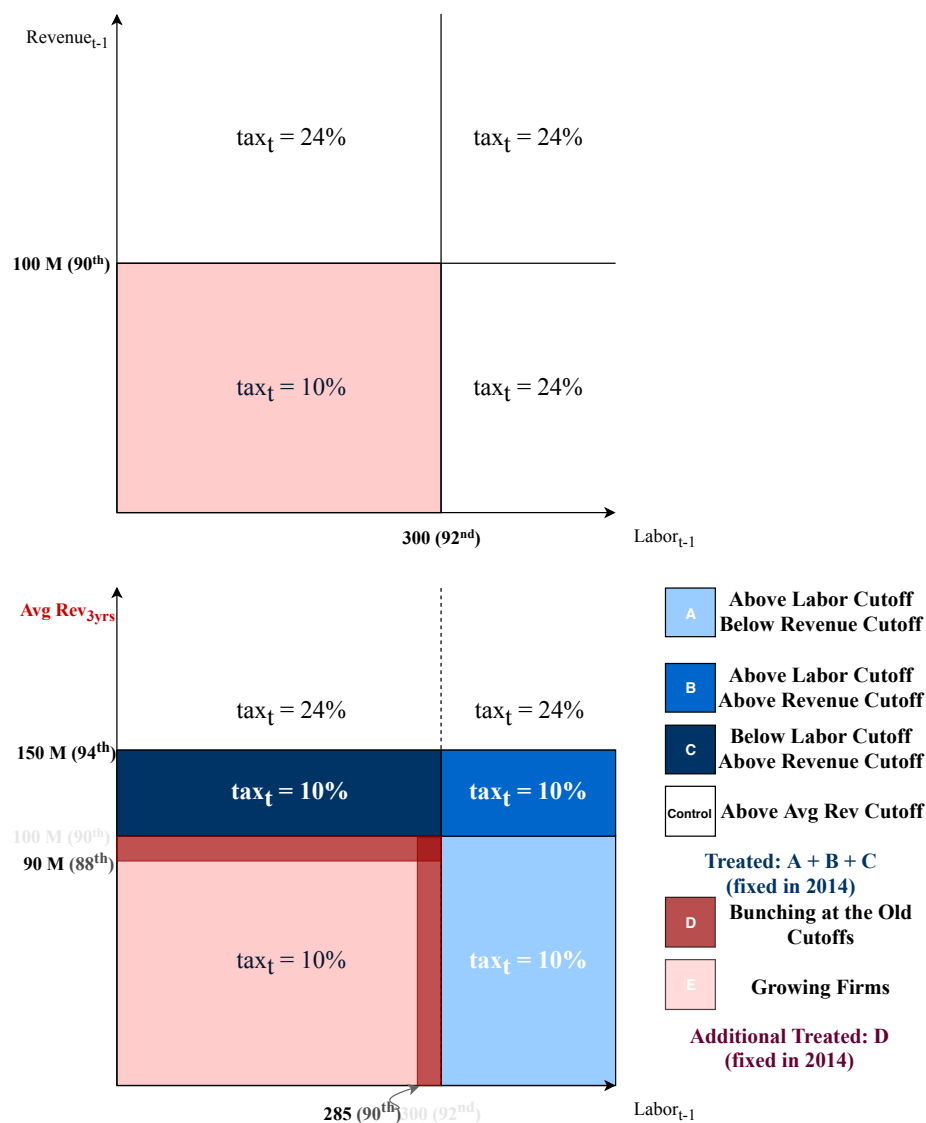
Next, we compare three cases under small open economy. Figure 2.12 plots the transitional dynamics of aggregate variables for the baseline, gaussian, and limited information cases. In small open economy, the interest rate is fixed at the initial level, and the short-run dynamics display further differences among the cases considered. Now the baseline model predicts a much higher short-run increase in aggregate investment than do the gaussian and limited information models. Right after the policy implementation, aggregate investment in the baseline case increases by 31%, while aggregate investment in the gaussian and limited information cases increases by 22% and 11%, respectively. This is because only the baseline case is able to match the difference-in-differences estimate of treated firms' capital elasticity by capturing the size-thresholds effect. Similarly, capital, output, and labor all increase more in the baseline case after the tax cut. Therefore, our simulation shows that capturing micro-level moments are essential for accurately predicting both short-run and long-run investment dynamics in the policy analysis.

2.8 Conclusion

This paper quantifies the long-run aggregate effects of reducing capital gains taxes. We build a dynamic general equilibrium model with heterogeneous firms facing discrete capital gains tax rates based on firm size. We calibrate our model by targeting key micro moments and the difference-in-differences estimate of the capital elasticity based on the institutional setting in Korea. We find that the reform that reduced the capital gains tax rates from 24 percent to 10 percent for firms affected by the new regulations increased aggregate investment by 1.61 percent in the steady state, with the short run effects overstating the effects by 1 percentage points. Moreover, a counterfactual analysis where we set the uniformly low tax rate of 10 percent shows that aggregate investment rose by 6.89 percent in the long run. Furthermore, we find that general equilibrium effects through prices are substantial in our simulation. Our findings suggest that reducing capital gains tax rates would substantially increase investment in the short-run, and accounting for dynamic and general equilibrium responses is important for understanding the aggregate effects of capital gains taxes.

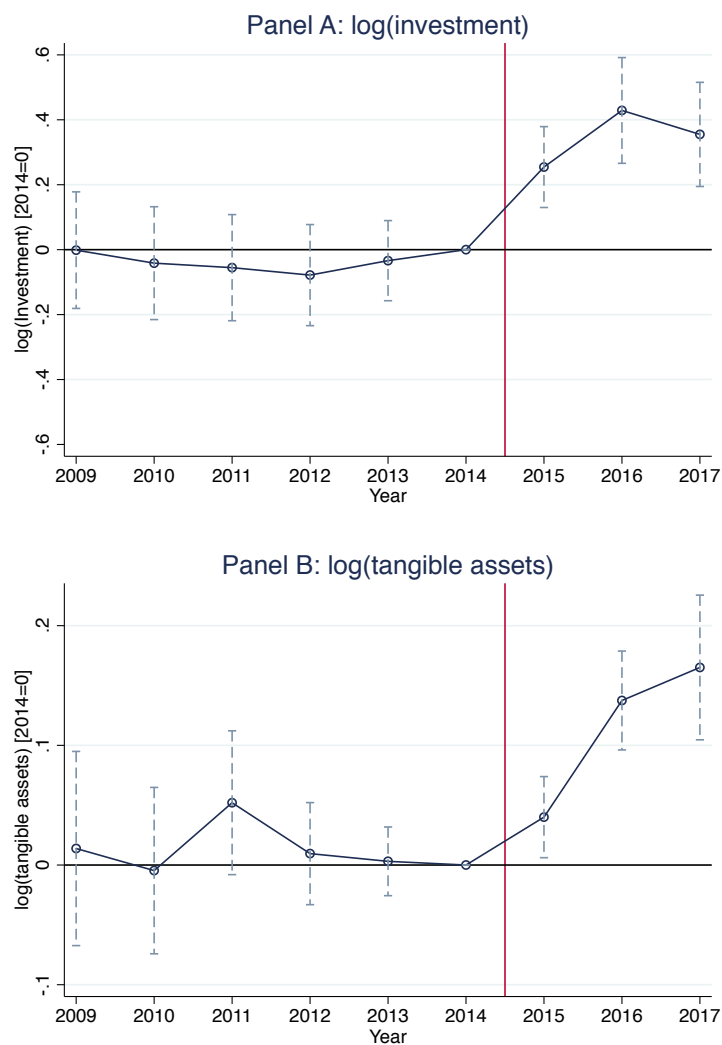
Our paper bridges the gap between reduced-form studies that evaluate tax policies with limited aggregate implications and structural papers that analyze the aggregate responses of tax reforms without fully capturing micro moments derived from clean identification strategies. By bridging this gap, this paper not only captures the short-run effects of reducing capital gains tax rates on investment in the partial equilibrium, but also consistently predicts the long-run aggregate responses that reflect both the general equilibrium price effects and dynamic adjustment frictions. Exploring potential mechanisms behind the aggregate responses, such as financial constraints, and incorporating other relevant features in our model, such as debt-financing, would be a valuable extension of our paper that may shed further light on the efficiency cost of capital taxation.

Figure 2.1: Policy Reform 2014 and Treated vs. Control Groups (Listed and Private Firms)



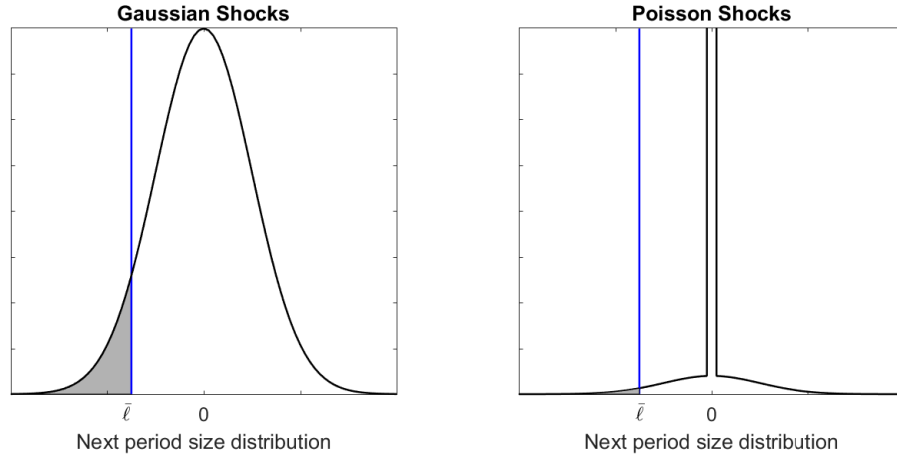
Notes: This figure illustrates how the reform in 2014 assigned firms into treated (tax cut) or control groups. The first figure on the top shows the initial rule on firm size prior to the reform, where firms in the pink area are jointly below labor and revenue threshold at time $t - 1$ and face a tax rate of 10%. The second figure shows how the reform affected firm size and the tax rates. I use firms in the blue areas (that experienced a tax cut by 14 percentage points) as the main treated group, and run a separate analysis using the second type of treated firms (that bunched in red areas) in Appendix B. I define the control group as firms that did not face any change in the tax rate or incentive to invest (in the white areas above the new revenue cutoff). Firms in the pink area were not directly impacted by the reform, but it is difficult to consider them as part of the control group because these firms were growing and may grow even more because the old thresholds are no longer binding.

Figure 2.2: Tax Effects on $\log(\text{investment})$ and $\log(\text{tangible assets})$



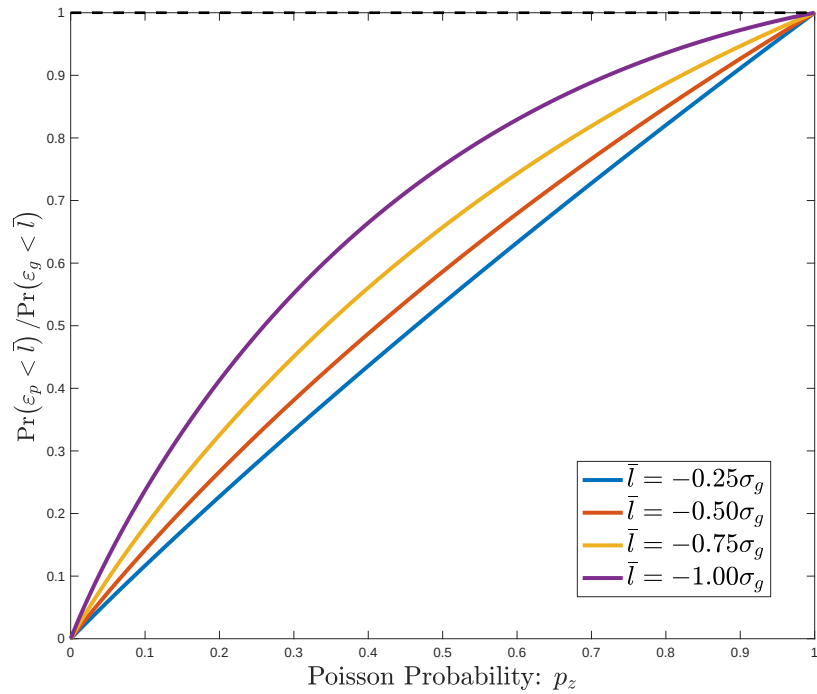
Notes: Panel A of this figure shows the coefficients on $Treated \times Time$ for firms' investment, defined as $\log(\text{expenditures on physical capital assets})$, in equation (2.1) using both publicly listed and private firms. The dashed lines indicate 95% confidence intervals for these coefficient estimates. The solid vertical line indicates the reform year. Panel B of this figure shows the coefficients on $Treated \times Time$ for firms' capital stock, defined as $\log(\text{tangible assets})$.

Figure 2.3: Size Threshold Effect



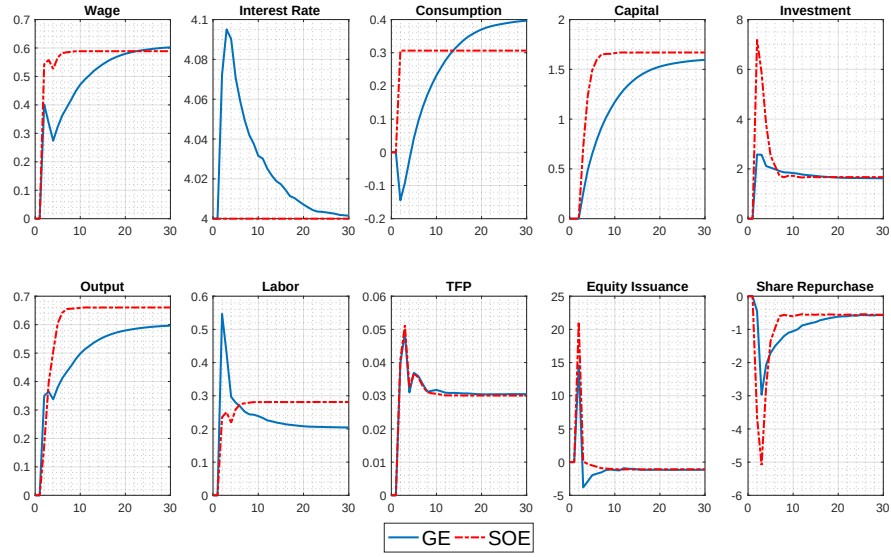
Notes: This figure illustrates size threshold effect. The left panel is the case of Gaussian shocks, and the right panel is the case of Poisson shocks.

Figure 2.4: Gaussian Shock vs Poisson Shock



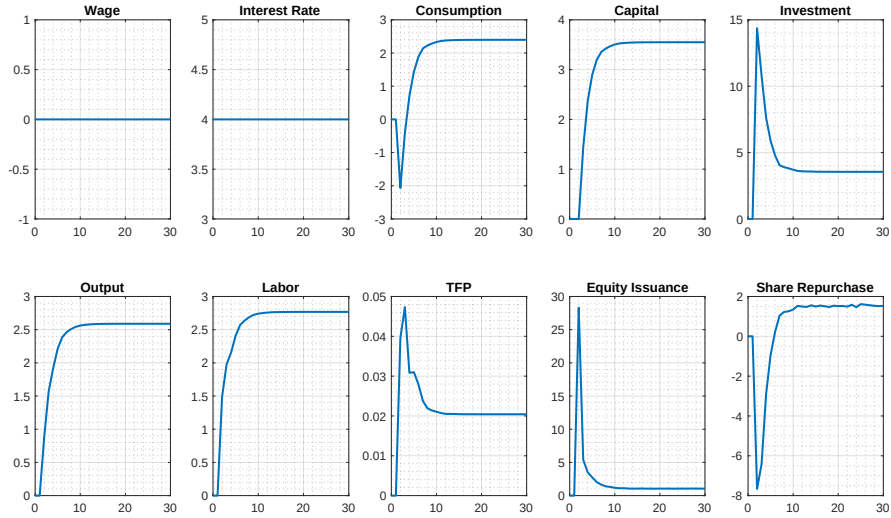
Notes: This figure illustrates the probability ratio of firm size below threshold $\bar{l}$. $\bar{l} \in \{-0.25\sigma_g, -0.50\sigma_g, -0.75\sigma_g, -1.00\sigma_g\}$ corresponds to lines from bottom to above.

Figure 2.5: Transitional Dynamics of Firm-size Reform in 2014



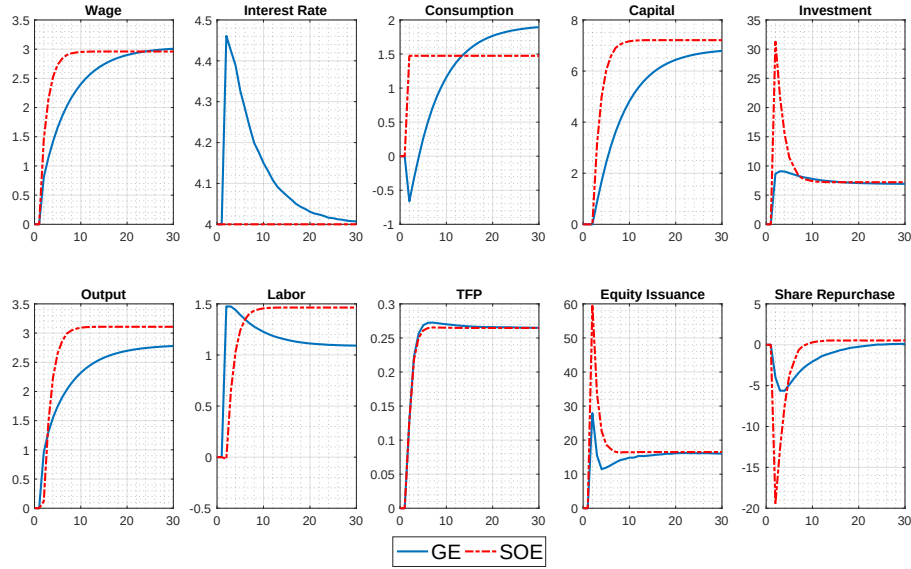
Notes: The economy is at the initial steady state before period 2. The figure plots the transitional dynamics of aggregate variables in response to the firm-size reform of eliminating labor threshold and increasing revenue threshold from 100 million to 150 million dollars. GE is the case of general equilibrium. SOE is the case of Small Open Economy.

Figure 2.6: Transitional Dynamics of Firm-size Reform in 2014: Partial Equilibrium



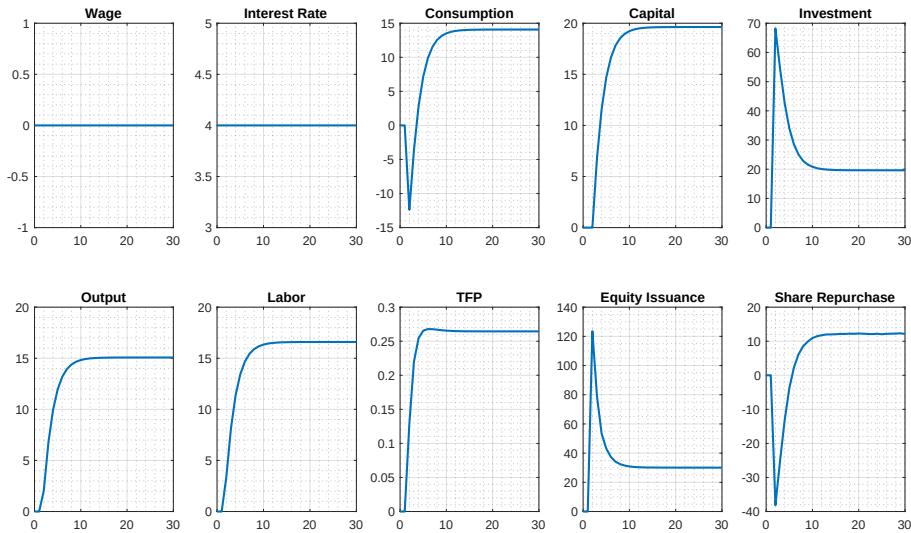
Notes: The economy is at the initial steady state before period 2. The figure plots the transitional dynamics of aggregate variables in response to the firm-size reform of eliminating labor threshold and increasing revenue threshold from 100 million to 150 million dollars. Both wage and interest rate are kept at initial level for Partial Equilibrium analysis.

Figure 2.7: Transitional Dynamics of Uniform Tax Rate



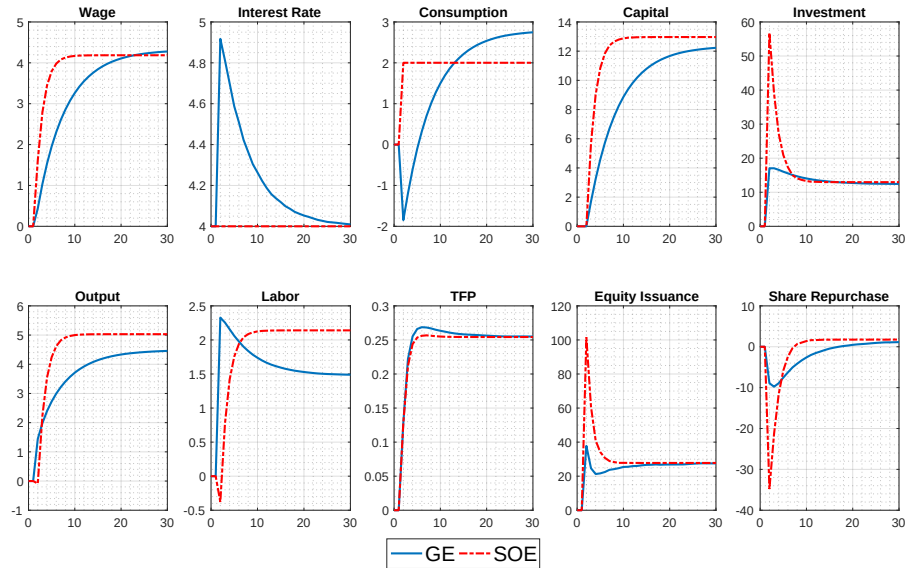
Notes: The economy is at the initial steady state before period 2. The figure plots the transitional dynamics of aggregate variables in response to setting a uniform capital gains tax rate of 10%. GE is the case of general equilibrium. SOE is the case of Small Open Economy.

Figure 2.8: Transitional Dynamics of Uniform Tax Rate: Partial Equilibrium



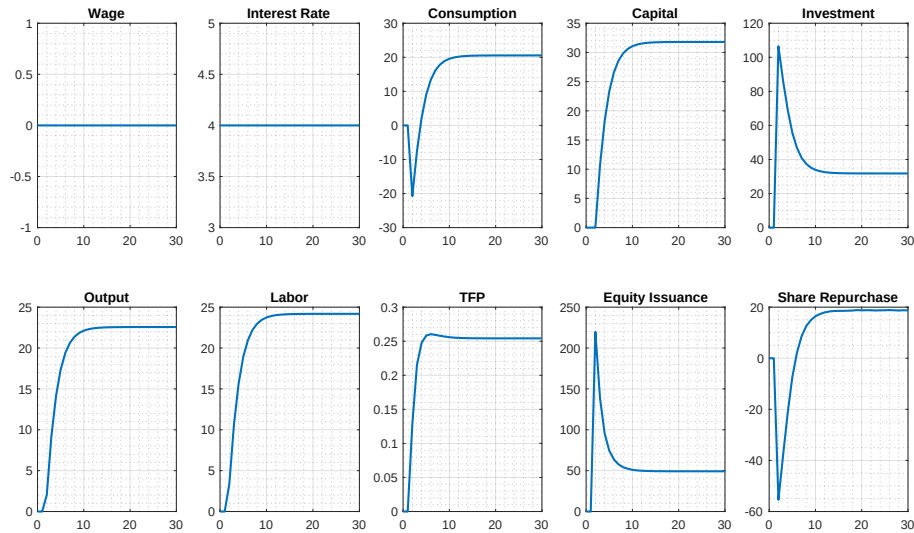
Notes: The economy is at the initial steady state before period 2. The figure plots the transitional dynamics of aggregate variables in response to setting a uniform capital gains tax rate of 10%. Both wage and interest rate are kept at initial level for Partial Equilibrium analysis.

Figure 2.9: Transitional Dynamics of Zero Tax Rate



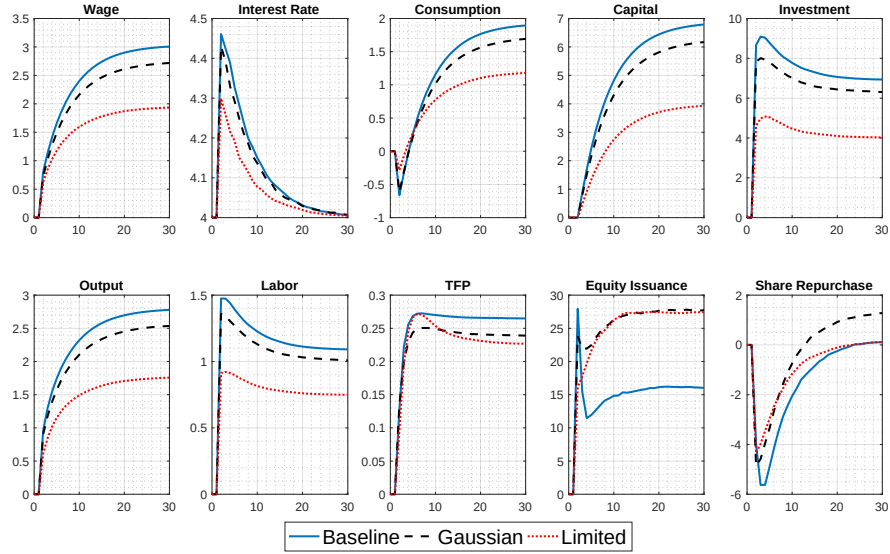
Notes: The economy is at the initial steady state before period 2. The figure plots the transitional dynamics of aggregate variables in response to setting the capital gains tax rate to zero. GE is the case of general equilibrium. SOE is the case of Small Open Economy.

Figure 2.10: Transitional Dynamics of Zero Tax Rate: Partial Equilibrium



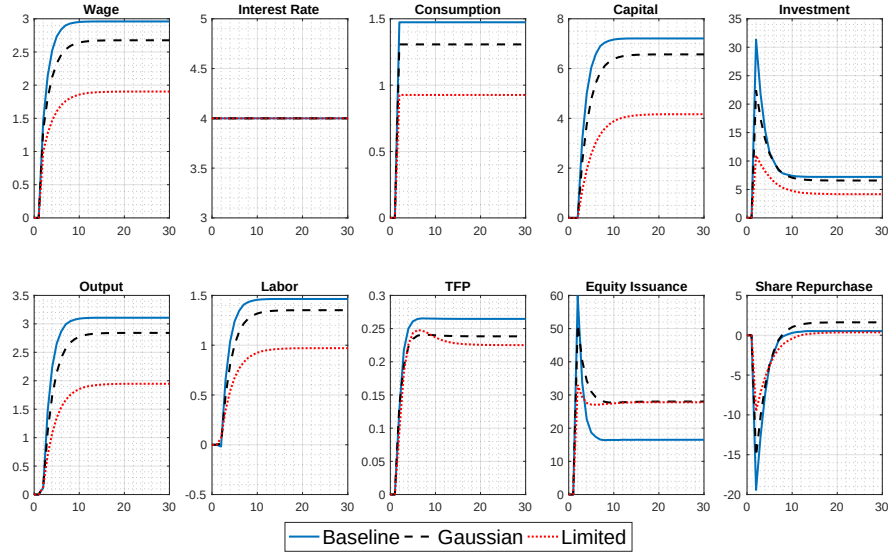
Notes: The economy is at the initial steady state before period 2. The figure plots the transitional dynamics of aggregate variables in response to setting the capital gains tax rate to zero. Both wage and interest rate are kept at initial level for Partial Equilibrium analysis.

Figure 2.11: Comparison of Baseline, Gaussian, Limited Information Cases (GE)



Notes: The economy is at the initial steady state before period 2. The figure plots the transitional dynamics of aggregate variables in response to setting a uniform capital gains tax rate of 10% in general equilibrium, for baseline, gaussian, and limited information cases.

Figure 2.12: Comparison of Baseline, Gaussian, Limited Information Cases (SOE)



Notes: The economy is at the initial steady state before period 2. The figure plots the transitional dynamics of aggregate variables in response to setting a uniform capital gains tax rate of 10% in small open economy, for baseline, gaussian, and limited information cases.

Table 1: Descriptive Statistics

	Listed and Private Firms		Listed Firms		Private Firms	
	(1) Treated	(2) Control	(3) Treated	(4) Control	(5) Treated	(6) Control
Total Revenue (in million)	74.69 (45.16)	251.8 (176.5)	95.13 (44.34)	284.4 (189.3)	67.10 (43.06)	226.6 (161.5)
Labor (Average Employees used)	243.1 (174.5)	393.4 (295.1)	254.3 (135.5)	467.5 (300.8)	238.9 (186.9)	335.9 (277.3)
Total Asset (in million)	75.02 (64.65)	237.3 (189.4)	123.1 (68.29)	305.2 (201.2)	57.16 (53.15)	184.7 (161.1)
Total Capital (in million)	40.50 (41.94)	123.1 (109.1)	74.52 (46.68)	165.8 (113.0)	27.86 (31.83)	89.99 (93.43)
CAPEX (in million)	4.067 (6.086)	11.22 (13.83)	5.763 (7.647)	13.34 (14.67)	3.436 (5.257)	9.580 (12.92)
CAPEX / lagged PPE	0.219 (0.264)	0.202 (0.234)	0.186 (0.224)	0.180 (0.209)	0.231 (0.277)	0.219 (0.250)
Observations	2635	8037	714	3509	1921	4528

Notes: Sample years include 2009-2017. Labor is the average employees used in a given year. CAPEX is expenditures on physical capital assets, such as plants, property, and equipment (PPE). Treated and control firms are defined in Section 2.3.

Table 2: Results on Investment and Capital Stock

	Investment	Capital Stock	Net Investment	Investment Rate
	(1) ln(I)	(2) ln(K)	(3) dK/K	(4) I/K
Treated x Post	0.356*** (0.068)	0.087** (0.041)	0.040*** (0.013)	0.040*** (0.012)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.140	16.323	0.084	0.150
Implied Elasticity wrt (1-tau)	1.93	0.47	2.54	1.46
R-squared	0.72	0.93	0.20	0.42
Observations (firm-years)	12496	12693	12700	4732
Clusters (firms)	1477	1477	1477	1477

Notes: This table reports the tax effects on investment based on specification (2.2). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 2.3. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as the expenditures on physical capital assets. Capital Stock is the total book value of tangible assets, such as plants, properties, and equipments. Net Investment is the annual changes in tangible assets. Investment rate is the investment to capital stock ratio. Basic controls are quartics in firm age and industry specific time trends. Additional controls are dummies for pre-reform (2014) operating profit quintile interacted with time dummies. The main outcomes are winsorized at the 99% level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample includes both publicly listed and private firms. All specifications include time and firm fixed effects (FEs). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 3: Fixed Parameter Values

Parameter	Symbol	Value
Discount Rate	β	0.97
Risk Aversion	σ	1
Labor Disutility	ω	6.616
Frisch Inverse Elasticity	ν	1
Risk-free Interest Rate	r	0.04
Income Tax Rate	τ^i	0.250
Corporate Tax Rate	τ^c	0.340
High (Low) Capital Gains Tax Rate	$\tau_h^s (\tau_l^s)$	0.24 (0.10)
Depreciation Rate	δ	0.10
Depreciation Allowance Rate	$\hat{\delta}$	0.10
Capital Elasticity	α_k	0.28
Labor Elasticity	α_l	0.57

Notes: This table shows externally calibrated parameters.

Table 4: Targeted Moments

Moment	Data	Baseline	Gaussian	Limited
Inaction Rate	0.32	0.28	0.34	0.34
Spike Rate	0.25	0.23	0.27	0.27
Std. of Investment Rate	0.16	0.16	0.15	0.17
Std. of log Output	1.23	1.15	1.19	1.19
1-year Autocorrelation of Output	0.97	0.98	0.98	<u>0.90</u>
3-year Autocorrelation of Output	0.93	0.93	0.93	<u>0.71</u>
5-year Autocorrelation of Output	0.88	0.87	0.86	<u>0.54</u>
Treated Group Capital Response	0.09	0.09	0.03	<u>0.01</u>

Notes: This table shows targeted moments in our model. Sample years include 2009-2017. Inaction rate is fraction of observations with absolute investment rate less than 5%. Spike rate is fraction of observations with absolute investment rate greater than 20%. Output is firm revenue. Treated group capital response is 3 year average of the difference-in-differences estimate of the capital stock response. Baseline case is an economy with Poisson shocks to firm productivity process. Gaussian case is an economy with Gaussian shocks to firm productivity process. Limited case is when policymakers have only observations before 2014 reform. Underlined moments are not included for parameter calibration.

Table 5: Estimated Parameter Values

Parameter	Symbol	Baseline	Gaussian	Limited
Upper Bound of Fixed Cost	ψ_0	0.0025	0.0018	0.0030
Quaratic Adjustment Cost	ψ_1	0.15	0.29	0.65
Persistence of Productivity	ρ_z	0.87	0.95	0.80
Std. of Productivity	σ_z	0.10	0.07	0.22
Poisson Probability	p_z	0.33	1	1
Revenue Threshold	$\tilde{y}$	90%ile	90%ile	90%ile
Labor Threshold	$\tilde{l}_E$	90%ile	90%ile	90%ile

Notes: This table shows parameters chosen to match empirical moments in Table 4.

Table 6: Post 2014 Aggregate Investment Dynamics

	Short Run	Long Run
Partial Equilibrium	14.37	3.55
General Equilibrium	2.57	1.61
Small Open Economy	7.19	1.67

Notes: This table shows percent changes of post 2014 aggregate investment compared to the pre-2014 steady-state.

Table 7: Post 2014 Aggregate Capital Dynamics

	Short Run	Long Run
Partial Equilibrium	1.44	3.55
General Equilibrium	0.26	1.61
Small Open Economy	0.72	1.67

Notes: This table shows percent changes of post 2014 aggregate capital compared to the pre-2014 steady-state.

Table 8: Aggregate Effects of Size-dependent Capital Gains Taxes: General Equilibrium

	Post 2014 $\tau^g = 0.10$ $\tau^g = 0$		
Capital	1.61	6.89	12.39
Labor	0.21	1.10	1.50
Output	0.60	2.81	4.50
Consumption	0.40	1.92	2.78
TFP	0.03	0.26	0.25
Equity Issuance	-1.17	16.33	27.31
Share Repurchase	-0.50	0.23	1.22
Wage	0.61	3.03	4.32
Welfare (Steady)	0.30	1.34	1.99
Welfare (Transitional)	0.18	0.83	1.11

Notes: This table shows percent changes of aggregate variables in the long-run compared to the pre-2014 steady state in general equilibrium. First column refers to the case of eliminating labor threshold $\bar{l}_E$ and increasing revenue threshold $\bar{y}$ from 100 million dollars to 150 million dollars. The second column refers to the case of setting a uniform tax of 10 percent. Third column refers to the case of eliminating capital gains taxes altogether.

Table 9: Aggregate Effects of Size-dependent Capital Gains Taxes: Small Open Economy

	Post 2014 $\tau^g = 0.10$ $\tau^g = 0$		
Capital	1.67	7.21	12.97
Labor	0.28	1.46	2.14
Output	0.66	3.11	5.03
Consumption	0.31	1.47	2.00
TFP	0.03	0.26	0.25
Equity Issuance	-1.08	16.50	27.78
Share Repurchase	-0.55	0.52	1.75
Wage	0.59	2.96	4.19
Welfare (Steady)	0.16	0.71	0.88
Welfare (Transitional)	0.16	0.74	0.93

Notes: This table shows percent changes of aggregate variables in the long-run compared to the pre-2014 steady state in general equilibrium. First column refers to the case of eliminating labor threshold $\bar{l}_E$ and increasing revenue threshold $\bar{y}$ from 100 million dollars to 150 million dollars. The second column refers to the case of setting a uniform tax of 10 percent. Third column refers to the case of eliminating capital gains taxes altogether.

Table 10: Comparison of Capital Distribution

	Data	Baseline	Gaussian	Limited
Std. of log Output	1.23	1.15	1.19	1.19
Std. of log Labor	1.06	1.14	1.19	1.19
Std. of log Capital	1.29	1.08	1.09	0.72
Output Share	0.65	0.47	0.48	0.48
Labor Share	0.53	0.48	0.49	0.48
Capital Share	0.57	0.44	0.43	0.26

Notes: This table shows size moments for the cases of baseline, gaussian, and limited information. The first to third row show the standard deviation of log output, labor, and capital for all firms. The last three rows show the output, labor, and capital share of firms above Pre-2014 revenue and labor thresholds.

Chapter 3

Political Connections and Resource Allocation in

Private Markets: A Social Network Channel¹

3.1 Introduction

A large and growing literature documents that firms benefit from connections to politicians through better access to government resources.² A smaller number of papers document that the private benefits accruing to connected firms come at a cost to society by misallocating resources³. Most studies implicitly or explicitly assume that distortive forces of political connections are limited to the allocation of

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²For example, Roberts (1990), Fisman (2001), Johnson and Mitton (2003), Jayachandran (2006), Faccio (2006), Ferguson and Voth (2008), Bunkanawanicha and Wiwattanakantang (2009), Faccio and Parsley (2009), Goldman, Rocholl and So (2009), Cooper, Gulen and Ovtchinnikov (2010), Akey (2015), Acemoglu et al. (2016), and Acemoglu, Hassan and Tahoun (2018). Documented benefits include better access to financing (Khwaja and Mian 2005; Leuz and Oberholzer-Gee 2006; Claessens, Feijen and Laeven 2008; Li et al. 2008), better access to government funds and bail outs (Faccio, Masulis and McConnell 2006; Duchin and Sosyura 2012), more government contracts (Tahoun 2014; Brogaard, Denes and Duchin 2015; Baltrunaite 2018; Schoenherr 2018), and laxer enforcement of regulations (Fisman and Wang 2015).

³Amore and Bennedsen (2013), Cingano and Pinotti (2013), and Schoenherr (2018).

government resources. For example, Khwaja and Mian (2005) use private banks as a control group to identify the effect of political connections on the allocation of state bank credit. As a consequence, the implications of connections between firms and politicians on resource allocation in private markets are largely unexplored. Since in many countries the majority of resources is allocated through private markets, it is important to understand whether and how firms' political connections affect the allocation of resources in these markets.

We attempt to fill this void in this paper. Specifically, we examine whether firms connected to a politician obtain better access to private bank credit through a social network channel. Identifying a specific channel through which political connections affect the allocation of resources in private markets is challenging, as political connections affect firms on several dimensions. For example, a firm that has better access to government resources, or is more likely to be bailed out by the government, may become a better borrower from private banks' perspective, potentially improving access to private bank credit (Houston et al. 2014). Additionally, firms may benefit from the agenda of a connected politician that shares a common ideology improving their investment opportunity set, potentially increasing their demand for and access to credit.

To overcome these challenges, we exploit an institutional setting in Korea that allows us to control for firm-level effects of political connections. After his election as President of Korea in December 2007, Lee Myung Bak (MB) appointed a large number of people from his social networks in important government posts (chief political advisors, ministers, chief prosecutors, CEOs of state-owned firms, the chairmen of the Bank of Korea, the Financial Services Commission, and the Financial Supervisory Service, etc.). One prominent network from which MB recruited employees of his administration was alumni of Korea University (KU), the

school that he graduated from.⁴ In an effort to establish connections to the new administration, private banks also appoint people from the new president's networks as executives (see Section 3.2 for details). The appointment of executives from KU by some private banks leads private firms with a CEO from KU to become connected to these banks through their CEOs' alumni network in addition to their connections to the new president. Cross-sectional variation in the appointment of banks executives from MB's alumni network allows us to compare changes in lending from banks that appoint executives from the KU network (KU banks) and banks that appoint executives from other networks (non-KU banks) to the same firm. This allows us to implement a triple-difference methodology saturated with firm-time fixed effects, which controls for firm-level effects, for example better access to government resources or higher demand for credit after the election.

Before examining changes in credit allocation at the firm-bank relationship level, we assess whether firms connected to the KU network (KU firms) experience an overall increase in credit from private banks, compared to firms not connected to the KU network (non-KU firms). We find that KU firms experience a significant relative increase in private bank credit by 16.69 percent after MB's election. Moreover, interest rates relatively decrease for KU firms by 18 basis points after the election, compared to non-KU firms. To assess whether changes in the volume and price of private bank credit are driven by newly established social connections between KU firms' CEOs and private banks' executives, we exploit cross-sectional variation in the appointment of executives from the KU network.

Out of the fifteen banks in our sample, the number of banks with executives from the KU network increases from two to eight after MB's election. If KU firms experience better access to private credit due to direct effects of their political connections, we would expect them to receive more credit from all banks. Instead,

⁴We focus on the Korea University alumni network due to data availability constraints for other networks.

we find that the increase in credit volume and decrease in interest rates experienced by KU firms are driven by the six banks that become connected to the KU network. KU firms experience a 61.35 percent higher increase in loan volume, and a 54 basis points higher decrease in interest rates from KU banks than from non-KU banks. These changes at the relationship level fully account for the firm-level changes in loan volumes and interest rates for KU firms. Additionally, compared to non-KU firms, KU firms are 9.91 percentage points more likely to start a new relationship with a KU bank after MB's election than with a non-KU bank. These results suggest that KU firms benefit from better access to private bank credit through their newly established social connections to private bank executives.

We confirm this interpretation of the results by examining changes in credit allocation for the two banks that are connected to the KU network through executives before and after MB's election. We find that these banks allocate significantly more credit at a lower rate to KU firms both before and after the election. This suggests that banks with an executive from the KU network do not allocate more credit to KU firms due to direct influence from or policies implemented by the MB administration. Becoming connected to the president has no effect on existing connections between firms and banks through the KU network. This view is further supported by finding similar effects for connections between firm and bank executives from other alumni networks. Upon appointing an executive from a given alumni network, banks start to allocate more credit at a lower rate to firms connected to the same network through their CEO. What set firms connected to MB's alumni network apart is the increase in the number of banks controlled by people from their network after the election. Thus, rather than affecting the intensive margin of the social network effect, the president affects the extensive margin by leading banks to appoint people from his networks, increasing their power over the allocation of private bank credit.

Due to a fixed one term limit for Korean presidents, we can examine the long-run effects of connections to MB's alumni network on access to private bank credit even after MB leaves office. Since MB's successor, Park Geun Hye is not connected to the KU alumni network, private banks cease to appoint executives from the KU network upon her inauguration in 2013 (Table 3). Following the departure of KU executives from private banks, we observe that KU firms experience a decrease in total credit from private banks and increase in interest rates back to levels they experienced before MB's election. This suggests that private firms' connections to the KU network generate better access to private credit only while MB is in office, emphasizing the important role of network connections to the president for better access to private bank credit through a social network channel.

Next, we are interested in underlying mechanism through which connections between firm and bank executives affect credit allocation. Bank executives may be able to extract better soft information from connected firms, allowing them to better identify good quality borrowers among connected firms (Herpfer 2018; Karolyi 2018) or expect better enforcement of contracts through a social collateral channel (Kandori 1992; Guiso, Sapienza and Zingales 2004; Karlan et al. 2009). Alternatively, social connections may induce taste-based discrimination, leading banks to misallocate credit to favor connected firms (Banerjee and Munshi 2004; Haselmann, Schoenherr and Vig 2018).

We find no evidence of an information or enforcement channel. If connections generate better information about firms, banks should be better able to differentiate between good and bad type borrowers. This should lead to higher dispersion in lending decisions and better loan performance (Rajan, Seru and Vig 2015). We observe neither. Instead, we find that dispersion in loan volumes and interest rates relatively declines for KU banks' portfolios of loans to KU firms after the election, compared to non-KU banks' lending to KU firms. While bankruptcy filings for

loans from KU banks to KU firms evolve similarly as for non-KU firms in the years after MB's election, KU banks engage in more debt restructurings with KU firms. After MB leaves office and connections between KU firms and former KU banks are terminated, default rates on loans from banks previously connected to the KU network to KU firms increase substantially. This suggests that KU banks make riskier loans to KU firms and are more willing to renegotiate their debt to avoid default while they are connected. Once connections between the banks and firms cease, their ex ante riskier lending decisions are reflected in higher default rates ex post.⁵ We estimate that favorable lending to KU firms reduces KU banks' overall profits from corporate lending by almost five percent.

Worse performance of connected loans also rules out that lending to firms connected to the same alumni network is driven by shared expertise. If KU alumni shared a common expertise, for example infrastructure projects, an increase in infrastructure spending under MB may lead banks to hire executives from the KU network, who would in turn lend more to KU firms that are more likely to implement infrastructure projects. However, an alternative explanation based on shared expertise is inconsistent with inferior loan performance. Based on observed recovery rates for creditors in bankruptcy, the return on loans from KU banks to KU firms is lower than the risk-free rate. Thus, even if banks were not funding constraint or unobservable cost – such as search costs – were lower for loans to socially connected firms, banks would be better off to invest excess funds in the risk-free asset rather than loans to connected firms. Additionally, lower returns on connected lending are not explained by cross-selling of other services. The fees on alternative financial products would need to be higher than the combined value of the products just to raise the returns on connected lending to the risk-free rate.⁶

⁵A large fraction of the defaults after MB's tenure are cases in which banks previously renegotiated the firm's debt while they were socially connected through KU executives.

⁶We strengthen the validity of the results by conducting additional robustness tests. First, we ensure that endogenous CEO appointments in private firms do not bias our results by redefining

The findings in the paper contribute to the growing literatures on the economic effects of political connections. While existing papers focus on the effects of political connections on the allocation of government resources,⁷ our paper is the first to examine how political connections affect the allocation of resources in private markets. We find that networks connected to powerful politicians experience an increase in the amount of resources that they control, even in private markets. Firms connected to the politician's social network in turn benefit from the increased control over the allocation of resources in private markets for members of their network who channel more resources to them. Thus, our findings suggest that the benefits of political connections for firms are larger than previously assumed, and distortions in resource allocation related to firms' political connections are not limited to the government sector. This further implies that firms have a strong incentive not only to establish direct connections to politicians, but also to become connected to their social networks, as these networks gain an increased role in allocating resources in the economy.

Our findings also provide new insights on the economic implications of social networks. Existing evidence in the literature on the role of social connections in bank lending is mixed. While Haselmann, Schoenherr and Vig (2018) find that social connections between bankers and firms lead to lower returns on bank loans, Engelberg, Gao and Parsons (2012), Herpfer (2018), and Karolyi (2018) argue that social connections improve lending outcomes. Our findings are consistent with a dominant role of taste-based discrimination in connected lending, leading to inferior loan performance. Novel data on debt renegotiations allows us to provides

firms' connectedness to the KU network as having a CEO from the KU network five years before MB's election. Second, we drop all firm-year observations in which firms borrow from government banks to ensure that lending to KU firms is not affected by coordination between private and government banks with executives from the KU network.

⁷For example, Khwaja and Mian (2005), Leuz and Oberholzer-Gee (2006), Claessens, Feijen and Laeven (2008), Li et al. (2008), Faccio, Masulis and McConnell (2006), Duchin and Sosyura (2012), Tahoun (2014), Baltrunaite (2018), Schoenherr (2018), and Fisman and Wang (2015).

more detailed insights on ways through which banks positively discriminate connected firms. Additionally, our data allows us to disentangle the effects of social connections on ex ante loan allocation and ex post outcomes. We show that banks issue riskier loans to connected firms that are more likely to lead to default ex ante and protect connected firms from default ex post through renegotiating connected firms' debt. This suggests that the positive relationship between social connections and loan outcomes documented in the previous literature might be driven by connected banks manipulating the outcome of connected loans. Moreover, the issuance of loans to connected firms that are used for refinancing rather than investment suggests that social connections hamper the allocating of credit to its most profitable usage and contribute to the survival of less productive firms (Caballero, Hoshi and Kashyap 2008).

The results in the paper also have political economy implications. Providing the president with extensive powers to appoint people in important positions in the administrations leads to a dominant role of the president's networks in controlling government and private resources. Combined with the evidence that social connections affect the allocation of resources in favor of connected firms, this suggests that equipping the president with extensive powers concentrates resources in members of some networks with potentially adverse implications for market structure, barriers to entry, or resource allocation across firms (Banerjee and Duflo 2005). The insights from our paper suggest that this may even be true in cases where the majority of resources is allocated through the private sector. Given the pervasiveness of political influence over the appointment of people in important roles in the administration in both developing and developed countries around the world, we expect that this mechanism is relevant beyond the specific context of this study.⁸

⁸For example, the President of the U.S. has the power to fill more than 300 positions without and more than 1200 positions with senate approval.

Finally, the evidence in the paper has implications for how firms select CEOs. If connections to networks with access to political power are valuable even beyond the government sector, firms have an even stronger incentive to hire CEOs based on their social connections rather than other skills that may contribute to the firm's profitability (Gabaix and Landier 2008; Tervio 2008; Oyer and Schaefer 2011; Starmans 2018). Since network connections seem to reallocate rents across firms rather than improving their productivity, this implies that the importance of CEOs' network connections may crowd out management skills that improve the productivity of firms.

3.2 Institutional Background

In this section we describe the institutional details that we exploit for the empirical analysis in this paper.

3.2.1 Definition of Political Connections

We exploit the election of Lee Myung Bak (MB) as President of Korea on December 19, 2007 as variation in political connectedness of firms connected to MB through their CEOs' alumni network. We focus on the Korea University alumni network (KU network) for which data is available and that is large enough to allow us to conduct statistical analysis.

A firm is considered connected to MB's alumni network if its CEO is a KU graduate at the time of MB's election in December 2007. Many firms have one CEO over the entire sample period, often family-controlled firms. Other firms appoint CEOs in fixed cycles of one to three years. Repeated reappointments of the same CEO are common. To mitigate concerns about the endogeneity of CEO appointments with respect to loan applications, we define political connectedness

as a sticky measure that is not updated. The firms connected to one of MB's networks at the time of his election are classified as connected for the full sample period.⁹ In robustness tests, we reclassify connected firms as those that have a connected CEO in 2003, more than four years before the election, to ensure that changes in access to private bank credit are not driven by endogenous CEO appointments in anticipation of MB's election.

3.2.2 Social Network Channel

The Korean president takes a dominant role in government and has the power to appoint a wide range of senior public officers. There is overwhelming evidence of the appointment of connected people during MB's presidency, including people from the KU network. Table 1 lists articles from the popular press about appointments of people from MB's networks in various areas - auditors, state firm CEOs, public prosecutors, the National Intelligence Service, the police, the National Tax Service, the National Research Council, and ambassadors. Additionally, we find that after MB's election, the number of chief prosecutors from Korea University more than doubled from 5 to 11, the fraction of ministers from Korea University increased from 11.7% to 13.3%, the share of chief political advisors (senior secretaries in the Blue House) increased from 14.7% to 22.9%. Ministers, chief prosecutors, state firm CEOs, etc. in turn decide about appointments and promotions of people at lower levels of the administration, leading to a trickle-down effect of appointments and promotions of individuals from the new president's networks.

Private financial firms, who appoint executives and board members independently from the government, responded to the changes in the government sector by appointing executives and board members from MB's networks (see Table 1). We

⁹We find stronger results when we update the connectedness measure, taking later CEO appointments into account.

confirm this in our data. Among the fifteen private banks in Korea, two had executives or board members from the KU network before MB's election, whereas this number increased to eight after the election. Financial firms' attempt to establish connections to the new administration by appointing members of the new president's networks alter the control of members of MB's networks' over the allocation of resources in private credit markets.

For the KU alumni network channel to have a meaningful impact, it is important to note that in Korea graduates feel responsibility towards their fellow alumni even across different cohorts. Alumni networks continue to be actively nurtured and expanded during graduates' professional careers, for example through regular gatherings of fellow alumni working for the same institution both in the private and public sector.¹⁰

3.3 Data

For the empirical analysis in this paper we merge data from different sources. From the Korea Information Service (KIS), we acquire accounting data, data on CEOs' educational and professional background, as well as data on bankruptcy filings and debt renegotiations. Data on CEO appointments of private banks comes from the Commercial Registration System governed by the Supreme Court of Korea. Data on CEOs' educational background for banks is taken from multiple issues of the Annual Dictionary of Korean Business Magnate published by Mailnet & Biz. Data on loans is extracted from firms' annual reports.

¹⁰Schoenherr (2018) documents that firms connected to the KU network through their CEO experience positive abnormal stock returns following the nomination of MB as his party's candidate for the presidential election.

3.3.1 CEO Data

Korean companies are legally required to report information about their board members to the Commercial Registration System supervised by the Supreme Court of Korea. The register lists the appointment, reappointment, and end of term dates. We collect this information for all commercial banks in Korea and match the data between 2003 and 2015 with data from the Annual Dictionary of Korean Business Magnate published by Mailnet & Biz and the CEO dataset provided by KIS, using CEOs' names and dates of birth, and bank name. The Financial Supervisory Service (FSS) defines fifteen lenders in Korea as commercial banks. For these banks comprehensive time-series information on their CEOs is available. Private banks provide about two-thirds of corporate lending in Korea during our sample period and are responsible for almost seventy percent of total loans extended to the firms in our sample. Eight banks have an executive or board member from the KU network after MB's election, two of them also have an executive or board member from the KU network before the election. We can match 9,729 of the private firms for which we can extract loan information from private banks with the CEO database. For 8,649 of these firms accounting data is available. In 2007, 2.36% of firms have a CEO from KU. In 2003, 2.35% of firms have a CEO from KU, with a large overlap between the groups of firms connected in 2003 and 2007.

3.3.2 Loan Data

To gather information on loans, we start by collecting all available annual reports of Korean firms between 2003 and 2015, released by the FSS. Next, we extract the information on all outstanding loans and their interest rates from the reports. The final loan sample comprises all firms for which loan data can be merged with data on CEOs' educational background using a corporate identification number that is

allocated to all registered corporations in Korea, is unique for each firm, and is available in both the loan and CEO datasets. Descriptive statistics on loans are depicted in Panel C of Table 1. The average loan amount is about 4,047m KRW for firms with a CEO from KU, and 3,031m KRW for non-KU firms.¹¹ Interest rates are similar with 5.90 percent for KU firms and 6.14 percent for non-KU firms. The average firm connected to the KU network borrows from 2.49 different banks among the private banks in our sample before the election, for non-KU firms the average number of lending relationships is 2.18. The median firm in both groups borrows from two banks. The total annual loan volume for the average firm is 14,499m KRW for KU firms, and 9,784m KRW for non-KU firms.

3.3.3 Accounting Data

Accounting data is available for 88.90 percent of the sample firms from KIS. Descriptive statistics on accounting data are summarized in Table 1, Panel D. Average firm size is 109,492m KRW for KU firms, and 53,928m KRW for non-KU firms in terms of total assets, and 169 and 114, respectively in terms of the number of employees. This suggests that firms connected to the KU network are larger than the average firm in the sample. Additionally, KU firms exhibit higher net income in line with their larger size. Tangible assets as a fraction of total assets are similar for KU and non-KU firms.

3.3.4 Bankruptcy Data

Data on bankruptcy filings and private workouts is available from KIS. The data includes the filing date, as well as the firm that files for bankruptcy or workout. We obtain data on bankruptcy and workout filings between 2003 and 2015. Workouts are initiated by a firm's primary bank, defined as the bank with the highest expo-

¹¹As a rule of thumb, one billion KRW are roughly equal to one million US dollar.

sure. Other lenders may ask the primary bank to consider initiating a workout. Before starting a workout, the primary bank organizes a meeting with other creditors to decide about initiating the workout. A workout starts if creditors holding three-quarters of the total claims agree. To accept a reorganization plan, creditors holding at least three quarters of the claims need to agree. Dissenting creditors may request assenting creditors to purchase their debt at a fair price, which will be determined by a mediation committee, if no agreement can be reached between the creditors.

3.4 Empirical Strategy

This section describes the empirical strategy to assess the presence of a social network channel through which politically connected firms benefit from better access to private bank credit.

We start by examining changes in total credit from private banks for firms connected to the Korea University alumni network compared to non-connected firms, by estimating:

$$\Delta \log(\text{loans})_i = \alpha + \beta \cdot KU_i + \epsilon_i \quad (3.1)$$

where the dependent variable is the log difference in firm i 's total bank loans from private banks after the election of Lee Myung Bak as Korean president (2008-2012), compared to the period before the election (2003-2007).¹² KU_i is a dummy variable that takes the value of one for firms with a CEO from the KU network at the time of MB's election, and zero for other firms.¹³ To assess changes in the cost of credit, we

¹²Collapsing the data into one observation per firm ensures that our standard errors are consistent despite the serial correlation of the underlying outcomes (Bertrand, Duflo and Mullainathan 2004).

¹³In robustness tests, we define connections based on firm's CEO in 2003 to eliminate concerns about endogenous CEO appointments in anticipation of MB's election.

replace the dependent variable in equation (3.1) with ΔIR_i , the difference in firm i 's average interest rate on all of its private credit after MB's election, compared to the period before the election.

The problem with the interpretation of β in equation (3.1) is that firm-level changes in access to and demand for credit, and changes in interest rates after MB's election could be driven by channels other than connections to newly appointed KU executives at private banks. For example, firms connected to the KU network may benefit from better access to government resources, which could change their investment opportunities and makes them safer borrowers from the perspective of private banks.

To isolate a social network channel from such alternative explanations, we zoom in to the firm-bank relationship level, by estimating:

$$\Delta \log(loans)_{ij} = \alpha + \alpha_i + \alpha_j + \beta \cdot KU_{ij} + \gamma \cdot con_{ij} + \epsilon_{ij} \quad (3.2)$$

where the dependent variable is the log difference in firm i 's loan amount from bank j after MB's election as Korean president, compared to the period before the election.¹⁴ KU_{ij} is a dummy variable that takes the value of one for firm-bank pairs in which firm i has a CEO from the KU network at the time of MB's election and bank j appoints an executive from KU after MB's election, and zero otherwise. Zooming in on the firm-bank relationship level has the advantage of allowing us to absorb any time-series changes in credit access and demand at the firm level. Specifically, equation (3.2) allows us to include firm-fixed effects (α_i) to control for firm-level changes in total credit or interest rates, due to for example changes in firms' access to government resources, investment opportunities, or demand for credit (Khwaja and Mian 2008). Additionally, bank-fixed effects (α_j) control for any time-series changes in the volume of lending and the pricing of credit for a

¹⁴We cluster standard errors at the firm-level in all tests with more than one observation per firm.

given bank. We include con_{ij} , a variable that measures alumni connections between firms and banks for other alumni network than the KU network to avoid a bias in estimating changes in loan volumes and interest rates at the relationship level. Specifically, the variable takes a value between minus one and one, increasing by 0.2 for each year firm i and bank j have an executive or board member from the same alumni network after the election, and decreases by 0.2 for each year they have an executive or board member from the same network before the election.

The coefficient β in equation (3.2) measures whether firms connected to the KU network benefit from newly emerging connections to KU executives in private banks. Remaining concerns with the interpretation of β in equation (3.2) relate to the endogeneity of appointments of executives from KU at the bank level. The reasons for which a given bank appoints an executive from the KU network after MB's election could be correlated with other changes at the bank level that induce systematic changes in lending to private firms with a CEO from KU. For example, MB may be able to directly interfere with lending decisions of private banks with a CEO from the KU network, or private banks that appoint an executive from KU may change the type of projects that they finance to the benefit of KU firms.

To address these concerns, we conduct a placebo test, exploiting the fact that two banks are already connected to the KU network through their executives even before MB's election. If changes in credit supply after MB's election are driven by a social network channel, we should not observe a change in credit supply for these banks, as the social network channel is already present before the election. However, if changes in credit supply are driven by direct influence of MB or policies that affect lending decisions of banks that appoint an executive from KU, we should observe a change in credit supply also for banks that already had an executive from KU before the election. To assess whether credit supply to private firms with a CEO from KU from private banks that already have an executive

before the election changes after the election, we estimate:

$$\Delta \log(\text{loans})_{ij} = \alpha + \alpha_i + \alpha_j + \beta \cdot KU_{ij,pre} + \gamma \cdot \text{con}_{ij} + \epsilon_{ij} \quad (3.3)$$

where $KU_{ij,pre}$ is a dummy variable that takes the value of one for firm-bank pairs in which firm i has a CEO from the KU network at the time of MB's election and bank j has an executive from KU before and after MB's election, and zero otherwise. All other variables are defined as before.¹⁵ A social network channel implies that β in equation (3.3) equals zero.

3.5 Results

This section presents the results from the estimation of equations (3.1) through (3.3), and describes additional tests that assess the mechanism underlying changes in the supply of credit to politically connected firms by private banks and robustness tests that support the interpretation of the results.

3.5.1 Credit Allocation Around MB's Election

We start our analysis with a graphical depiction of changes in loan volumes allocated by private banks in Korea to private firms around MB's election in the top panel of Figure 3.1. The black line shows the values for KU firms, the gray line shows the values for non-KU firms. While private credit to both groups of firms evolves similarly in the three years before MB's election, there is a higher increase in credit to KU firms from 2008, the year when MB comes into office and when six of the private banks appoint executives and board members from the KU network. In Table 2, column I, we confirm the graphical evidence statistically by estimating

¹⁵For this test, we exclude all banks that newly appoint a KU executive after the election, as including them might lead to a downward (upward) bias in β for loan (interest rate) regressions.

equation (3.1). Firms connected to the KU network experience a higher increase in private credit by 16.69 percent after the election, compared to non-KU firms. The results in column II show that the increase in credit volume is not accounted for by higher firm growth. As a fraction of total firm assets, loan volume increases by 1.46 percent more for KU firms after the election.

The bottom panel in Figure 3.1 depicts changes in interest rates for firms connected to the KU network (black line) and non-KU firms (gray line) around MB's election. While interest rates increase for both groups of firms in 2008 and 2009, the increase is smaller for KU firms. Moreover, in the years from 2010 to 2012, the difference in interest rates between firms connected to the KU network and those not connected to the KU network remains larger than before MB's election. The results in column III of Table 2 confirm that KU firms experience a relative 18 basis points decline in interest rates compared to non-KU firms after MB's election.

Together, the results in Figure 3.1 and Table 2 show that firms connected to the KU network experience an increase in the volume of private bank credit and a reduction in the cost of credit. The remainder of this section is devoted to understanding to what extent this increase in credit supply is driven by newly established social connections between private firms with a CEO from the KU network, and private banks that appoint executives and board members from the same network.

3.5.2 Social Network Channel

The previous results suggest that firms connected to the KU network experience an increase in credit from private banks and a reduction in their borrowing costs. A large set of potential mechanisms may explain these findings. There is broad evidence that politically connected firms benefit from government bailouts (Faccio, Masulis and McConnell 2006), government subsidies (Duchin and Sosyura 2012;

Cingano and Pinotti 2013), better access to credit from government banks (Khwaja and Mian 2005; Claessens, Feijen and Laeven 2008), more government contracts (Tahoun 2014; Baltrunaite 2018; Schoenherr 2018), and more lenient enforcement of regulations (Fisman and Wang 2015). These benefits may make politically connected firms better borrowers from private banks' perspective, which could explain why they provide more credit financing to firms connected to the KU network at a lower rate (Houston et al. 2014). Additionally, politically connected firms may systematically benefit from the new president's political agenda. For example, if the government increases investment in sectors that firms connected to the KU network operate in, this may lead to higher investment and demand for credit by KU firms.

To isolate a social network channel from these alternative explanations, we compare the increase in private bank credit to KU firms from banks that appoint an executive member from the KU network (KU banks) and banks that appoint executives and from other networks (non-KU banks). Comparing changes in lending to the same firm by different groups of banks controls for firm-level changes in demand and firm-level effects that could lead to changes in access to private bank credit. For example, if KU firms benefit from better access to government resources making them safer borrowers, they should be able to receive more credit from all private banks. In contrast, if changes in access to credit are driven by firms becoming connected to private banks through their CEOs' social networks ties, we should observe a higher increase in credit supply from KU banks.

Figure 3.2 plots the time-series evolution of average annual credit volumes allocated to KU firms (black lines) and non-KU firms (gray lines) by non-KU banks (top panel) and KU banks (bottom panel). The top panel shows that for non-KU banks credit growth is almost identical for KU and non-KU firms. In contrast, the bottom panel shows that credit growth from KU banks is significantly higher

for KU firms than for non-KU firms from 2008, the year in which MB takes office. In Figure 3.3, we plot the same graphs for interest rates. The top panel shows that interest rates grow at about the same rate for KU and non-KU firms for loans extended by non-KU banks. The bottom panel shows that for loans allocated by KU banks interest rates relatively decline for firms with a CEO from the KU network after the election.

We confirm the insights from the graphical analysis statistically by estimating equation (3.2) in Table 2, columns IV to VI. The relative increase in lending from KU banks after the election is 61.35 percent higher for KU firms than for non-KU firms (column IV), which constitutes a 2.28 percent increase relative to firms' assets (column V). Additionally, KU firms experience a relative decline in interest rates by 54 basis points from KU banks compared to non-KU banks (column VI). In column VII, we examine whether KU firms are more likely to establish a new lending relationship with KU banks after the election by replacing the dependent variable in equation (3.2) with a variable that takes the value of one if firm i establishes a new relationship with bank j after the election, and zero otherwise. KU firms are 9.91 percentage points more likely to start a new lending relationship with a KU bank after the election, compared to non-KU firms.

KU firms have on average 2.49 relationships and 0.77 relationships with a KU bank. Thus, 31 percent of KU firms' lending relationships are with KU banks. When we multiply the additional relationship level increase in total loans and decrease in interest rates from KU banks to KU firms, they predict a firm-level increase in total loans by 18.97 percent and a firm-level decrease in interest rates by 17 basis points. This corresponds almost exactly to the firm-level estimates in columns I and III of Table 2, which implies that the increase in credit supply to KU firms from private banks is entirely driven by the banks that KU firms become connected to after MB's election.

The estimates for other alumni networks (con_{ij}) are similar to those for the KU network.¹⁶ This suggests that better access to credit from connected banks at a lower rate applies to other alumni networks as well and is not limited to the KU network. What sets firms connected to the KU network apart from other firms is that they benefit from an extensive margin network effect following MB's election. While firms generally seem to benefit from alumni network connections of their CEOs for better access to private bank credit, firms connected to the KU network are unique in experiencing a 300 percent increase in the number of alumni network connections to private banks from two to eight after the election.

An alternative interpretation of the results is that changes in lending to firms with a CEO from the KU network may be driven by direct intervention from the president or regulators rather than connections between bank executives and firm CEOs. For example, bank executives from the KU network may favor KU firms to win favors with the MB administration or be coerced into lending to KU firms by the administration. To assess this potential alternative explanation, we conduct a placebo test by estimating equation (3.3). Two banks in the sample already have an executive from the KU network before the election, and continue to be connected to the KU network after the election. For these banks social connections to private firms with a CEO from the KU network exist prior to MB's election. Thus, if social network connections between banks and firms explain the observed differences in private bank lending, we would expect these banks to lend more to firms connected to the KU network even before the election, and to observe no change in lending patterns after MB's election. In contrast, if bank lending decisions are affected by direct influences from the MB administration, we expect lending to KU firms to also increase for banks connected to the KU network before and after the election.

¹⁶Since some firms appoint executives from other networks already before MB's election the effect on new relationships after 2008 is weaker for other networks.

The results are collected in Table 3. We find that firms connected to the KU network receive 40.74 percent more loans from banks with an executive from the KU network before the election (column I). After the election, the effect is similar with 26.67 percent (column II), and there is no significant change in lending to KU firms for banks that are connected to the KU network before and after the election (column III). In column VI, we find that the election of a president from the KU network does not affect the cost of credit for lending relationships between firms and banks connected to the KU network both before and after the election. KU firms pay lower interest rates on loans from KU banks before the election by 31 basis points (column IV), and after the election, by 36 basis points (column V). This strengthens our interpretation of the results that what matters for credit allocation between private banks and firms are social network connections between their executives and suggest that the social network channel operates even in the absence of political connections.

Since the Korean President is only allowed to serve for one term, we are able to assess changes in access to credit and interest rates for KU firms after MB leaves office. Following the election of Park Geun Hye, MB's successor, most private banks cease to reappoint executives from the KU network, leading to a drop in banks with connections to the KU network from eight to three (see Table 3). Applying the same methodology from equations (3.1) and (3.2) for the period from 2010 to 2015, we examine changes in credit allocation and interest rates around Park's election in Table 4.¹⁷ The results in columns I and II indicate that firms connected to the KU network experience a decrease in total lending after MB leaves office and banks cease to appoint executives from KU. Similarly, interest rates on loans extended to firms connected to the KU network increase to similar levels as before MB's election (column III). Together, this suggests that better access to credit for firms with a CEO

¹⁷The last year for which we were able to collect loan data is 2015. We choose the 2010-2015 time window to have a symmetric six year window around the election.

from the KU network is limited to the period of MB's presidency. Consistent with this interpretation, the drop in lending and increase in interest rates is driven by those banks that cease to appoint executives from the KU network after Park's election (columns IV to VII).

3.5.3 Underlying Mechanism

In this section, we seek to differentiate between different mechanisms that could explain more lending at lower rates from KU banks to KU firms after MB's election. Connections between banks and firms may improve the allocation of credit by facilitating the screening of borrowers. For example, bank executives from the KU network may possess superior soft information about firms with a CEO from the same alumni network. This would allow them to better differentiate between good and bad borrowers among the group of socially connected firms, potentially leading banks to provide more credit to connected firms at a lower rate (Herpfer 2018; Karolyi 2018). Social connections between bank and firm executives may also generate social collateral improving monitoring and enforcement for loans allocated to connected firms. This may reduce monitoring costs and ex post default rates for connected loans, allowing banks to provide credit at a lower rate (Guiso, Sapienza and Zingales 2004; Karlan et al. 2009). Alternatively, connections between bank and firm executives may lead to a misallocation of credit due to favorable treatment of connected firms based on taste-based discrimination (Banerjee and Munshi 2004; Haselmann, Schoenherr and Vig 2018).

We start the assessment of the underlying mechanism by comparing changes in credit allocation in the cross-section. Specifically, we examine whether differential changes in lending decisions by KU and non-KU banks are related to changes in observable risk (measured as interest coverage). The results in Table 5 indicate that connected lending from KU banks to KU firms expands more for riskier firms,

which suggests that the credit portfolio of connected loans between KU banks and KU firms becomes riskier after the election. Sorting firms into deciles according to their measure of interest coverage in 2007, we find that riskier KU firms experience a higher relative increase in loans from KU banks by 5.23 percent per default risk decile (column I), or 0.23 percent relative to firms' assets (column II). Similarly, interest rates on loans from KU banks to KU firms relatively decrease more for risky firms by 10 basis points per decile (column III). This suggests that the pricing of loans from KU banks to KU firms becomes less sensitive to risk. Finally, KU banks are significantly more likely to establish a new lending relationship with riskier KU firms by 1.55 percentage points per risk decile (column IV).

An increase in lending to connected firms with higher observable risk may reflect superior soft information that KU banks obtain about KU firms that is unobservable to other banks. Additionally, better monitoring and enforcement mechanisms that help banks to reduce default rates from connected firms for a given level of risk leading to a lower sensitivity of pricing to firm risk. Alternatively, taste-based discrimination towards connected firms could explain more lending to riskier firms at a lower rate by connected banks.

A unique prediction of better information is that it allows banks to better differentiate between good-type and bad-type borrowers. If this was the case, we should expect to see higher dispersion in loan allocation and interest rates for loans allocated to connected firms, compared to firm-bank relationships that lack the additional soft information (Rajan, Seru and Vig 2015). Figures 3.4 and 3.5 depict the distribution of the log of loans and interest rates at the firm-bank relationship level, respectively. The black lines indicate the distribution for KU firms, the gray lines for non-KU firms. The left panels show the distributions for non-KU banks, the right panels depict the distributions for KU banks. The top panels show

the distributions before MB's election, the bottom panels show the distribution after MB's election.

The distribution of relationship level loans and interest rates does not appear to become more dispersed after the election for loans from KU banks to KU firms in Figures 3.4 and 3.5. If anything, the dispersions of lending decisions and interest rates seem to be lower after the election. For loan amounts, we observe a rightward shift of the distribution, consistent with the increase in credit allocated from KU banks to KU firms. Similarly, the distribution of interest rates is shifted leftward after the election without any indication of higher dispersion. Together, these results suggest that changes in credit allocation between connected private banks and firms is not driven by better information.

To confirm the interpretation of the graphical evidence from Figures 3.4 and 3.5 statistically, we follow Rajan, Seru and Vig (2015) in estimating:

$$\Delta\sigma(IR)_{gj} = \alpha + \alpha_j + \beta_1 \cdot KU_g + \beta_2 \cdot KU_{gj} + \epsilon_{gj} \quad (3.4)$$

where $\Delta\sigma(IR)_{gj}$ is defined as the change in the standard deviation of interest rates extended by bank j to the group of firms connected to the KU network and the group of non-KU firms, respectively, for the post-election period compared to the pre-election period. KU_g is a dummy variable that takes the value of one for loans made to KU firms and zero for loans made to non-KU firms, and the variable KU_{gj} takes the value of one for loans from KU banks to the group of KU firms and zero otherwise.¹⁸

The results from estimating equation (3.5) are displayed in columns V and VI of Table 5. They show that the dispersion in loan amounts (column V) and interest rates (column VI) for loans extended by KU banks to KU firms significantly

¹⁸We estimate the same specification, replacing the independent variable by the change in the standard deviation of the log of loans of the portfolios of loans to KU and non-KU firms, respectively, for bank j .

declines after the election when the banks and firms become connected through their executives compared to loans extended to non-KU firms, relative to non-KU banks.¹⁹ This is inconsistent with increased lending of KU banks to KU firms after MB's election being driven by better information transmission.

Next, we assess whether higher credit supply from KU banks to KU firms may be driven by better enforcement mechanisms. Lower interest rates for connected loans could be justified if connected firms are less likely to default on loans. In this case, banks may also be less sensitive to firm risk as differences in default rates may be muted by better enforcement mechanisms, which could also explain the reduced dispersion in lending decisions. Exploiting data on bankruptcy filings and private workouts, we compare changes in default rates around MB's election by estimating:

$$default_{ijt} = \alpha + \alpha_j + \beta_1 \cdot KU_i + \beta_2 \cdot KU_{ij} + \beta_3 \cdot KU_i * Post_t + \beta_4 \cdot KU_{ij} * Post_t + \epsilon_{ijt} \quad (3.5)$$

where $default_{ijt}$ is a measure of bankruptcy filing or workout filing that takes the value of one if firm i defaults on a loan from bank j in year t , and zero otherwise. The variable $Post_t$ takes the value of one for the post-election period from 2008 to 2012, and zero for the pre-election period from 2003-2007. All other variables are defined as before.

Figure 3.6 depicts information about firms' bankruptcy filings and private workouts from 2003 to 2015. The top panel depicts the probability of a firm to file for bankruptcy (solid lines) or workout (dashed lines) while having an outstanding loan from a non-KU bank. The bottom panel depicts the same information for loans from KU banks. The black lines depict information for firms connected to the KU network, and the gray lines for non-KU firms. Before MB's election from 2003 to

¹⁹Changes in the dispersion of lending decisions and interest rates are not driven by changes in the composition of firms. When we restrict the post-election portfolios to firms that banks already lend to before the election, we find qualitatively identical effects.

2007, defaults are rare.²⁰ After MB's election, from 2008 to 2012, we observe very similar patterns in bankruptcy filings for KU and non-KU firms. The probability for either type of firm to file for bankruptcy on loans from KU and non-KU banks increases to around 1-2 percent. However, while bankruptcy filings show similar patterns for KU and non-KU firms irrespective of whether a loan is extended by a KU or non-KU bank, we observe drastically different patterns for private workouts. The probability of KU firms to engage in a private workout with a KU banks increases to about 3-4 percent, whereas we do not observe a similar increase for non-KU firms or for loans from non-KU banks. This suggests that KU banks are more willing to renegotiate credit with connected firms after the election.

We confirm this observation statistically in Table 6. The results in column I indicate that bankruptcy filings for firms connected to the KU network increase at about the same rate regardless of whether they borrow from a KU bank or a non-KU banks after the election. In contrast, workout filings increase by 5.82 percentage points more for loans from KU banks to KU firms after the election compared to loan from non-KU banks (column II). Overall, KU firms are 5.73 percentage points more likely to experience a default or renegotiation on a loan from a KU bank after the election compared to a loan from a non-KU bank (column III).

For a subset of loans, firms' annual reports include information of the usage of the credit. We split loans into those used for investment purposes and those used for financing-related purposes, for example refinancing an existing loan. We find that KU firms experience a 7.39 percentage point increase in the fraction of loans being used for financing-related purposes rather than investment after MB's election (Table reftab:risk, column VII). This increase is entirely driven by loans from KU banks that are 30.06 percentage points more likely to be used for financing-related

²⁰The low number of bankruptcy filings before the election is driven by a combination of high economic growth and a bankruptcy regime that discouraged bankruptcy filings before 2006 (Schoenherr 2017).

purposes by KU firms (column VIII), which supports the evidence that KU banks support KU firms in refinancing and restructuring existing debt.

To disentangle the various ways through which connections affect loan outcomes, we exploit the fact that MB only stays in office for one term. During his presidency, connections between KU banks and firms may affect loan outcomes in multiple ways. They may affect the initial allocation decision with direct implications for loan outcomes. Additionally, connections may affect loan outcomes directly, for example through the leniency of banks in enforcing loan contracts when firms become distressed. The higher incidence of overall defaults suggests that KU banks provide more credit to riskier firms *ex ante* when lending to socially connected firms. A higher incidence of workout filings rather than an increase in bankruptcy filings and the higher fraction of loans being used for financing-related purposes suggest that KU banks are more willing to renegotiate existing credit with connected firm and provide them with new credit to pay off existing loans. Both may protect connected firms from default leading to better observed loan outcomes.

Due to the limitation of MB's presidency to one term, we can observe outcomes for loans allocated to KU firms by KU banks in the long-run when KU banks cease to appoint KU executives and social connections to KU firms are thus terminated. These loans are subject to potential distortions in the *ex ante* allocation of loans while banks and firms are connected through their executives' connections to the KU network. However, after the banks terminate their connection to the KU network, loans made to KU firms are no longer subject to *ex post* effects of connections on loan outcomes. Examining default rates after 2012 in Figure 3.6, we find that after banks separate from KU executives, workouts with KU firms start to decline, consistent with *ex post* loan outcomes no longer being impacted by higher leniency of connected banks. Instead, bankruptcy filings increase drastically after 2012 for

these loans to about five percent of all loans in a given year. This suggests that once social connections between KU firms and KU banks are terminated, the higher riskiness of the ex ante loan allocation to connected firms is reflected in higher bankruptcy filings ex post.

Taken together, the results in this section are most consistent with taste-based discrimination underlying the social network channel of credit allocation from private banks to socially connected firms through the KU network.

3.6 Alternative Explanations

In this section we discuss alternative explanations for the results documented in the previous section.

3.6.1 Endogenous CEO Appointments

Firms that anticipate to apply for loans at a particular bank may endogenously appoint a CEO from the KU network leading to a correlation between firms' connectedness to the KU network and their demand for credit from KU banks. To mitigate this concern, we reclassify connected firms based on their CEOs' alumni network connection in 2003, before MB's election was anticipated. That is, we classify firms as connected to the KU alumni network if they have a CEO from the KU network in 2003 in Table 5.

We find almost identical results for this alternative definition of connectedness to the KU network. Additionally, we observe very few appointments of CEOs from the KU network in the years before MB's election. This suggests that the main results are not affected by endogenous CEO appointments by firms in anticipation of executive appointments at private banks around MB's election.

3.6.2 Shared Expertise

A potential alternative explanation for an increase in loans from KU banks to KU firms after MB's election is that KU alumni share a common expertise. For example, KU alumni may be experts in infrastructure projects. If the MB administration intended to increase spending on infrastructure projects, more banks may be compelled to hire executives from the KU alumni network that have expertise in infrastructure investment-related lending. If banks hire KU alumni as executives in anticipation of increasing lending to firms engaged in infrastructure projects, they may lend more to firms with a CEO from KU, who might be more likely to implement infrastructure projects.

The main argument against an alternative explanation based on common expertise is that it should lead to better, or at least equally good, loan outcomes. Instead, the results in Section 3.5.3 show that loans from KU banks to KU firms perform worse with a significantly higher incidence of defaults despite lower interest rates. Additionally, the results in columns VII and VIII of Table 5 imply that rather than using the additional lending for investment, firms connected to the KU network use a larger fraction of loans from KU banks for refinancing after MB's election. Together, inferior loan performance and their usage for refinancing rather than investment are inconsistent with an explanation based on shared expertise.

3.6.3 Coordination with State Banks

Another potential concern with our interpretation of the results is that private banks with an executive from the KU network may be able to coordinate lending to KU firms with state banks after the election. This may reduce the risk of lending to KU firms, if these firms are likely to be bailed out by government banks. In Table 6, we drop all firm-years in which KU banks lend to firms that also borrow from

a state bank. We observe qualitatively identical results with similar magnitudes, compared to our main tests in Table 2. Additionally, the results on loan outcomes do not indicate that KU banks extend lending to KU firms by coordinating lending to KU firms with state banks that are more likely to bail out KU firms. Together, this suggests that coordinated lending between state banks and private banks with connections to the KU network does not explain the results.

3.6.4 Renegotiation Frictions

Higher default rates on loans from KU banks to KU firms, despite lower interest rates, could be justified by higher recovery rates for connected loans in case of default. Loans made to connected firms may be better collateralized, or banks may be better able to renegotiate debt with socially connected firms leading to higher recovery rates, which could also explain the higher number of workouts.

While higher recovery rates could justify higher default rates and lower interest rates for connected loans, loans from KU banks to KU firms yield lower returns than loans to non-KU firms during the 2008 to 2015 period for any recovery rate. Returns on loans from KU banks to KU and non-KU firms can be computed as: $(1 - 0.0343 - 0.0291) * 1.0582 + 0.0343 * rec_B + 0.0291 * rec_W - 1 = r_{connected}$, and $(1 - 0.0131 - 0.0010) * 1.0613 + 0.0131 * rec_B + 0.0010 * rec_W - 1 = r_{non-connected}$, respectively, where rec_B is the recovery rate in bankruptcy and rec_W is the recovery rate in workout cases. Due to the higher default rates (0.0343 vs. 0.0131) and lower interest rates (0.0582 vs. 0.0613), there are no values for rec_B and rec_W between zero and one, such that $r_{connected} / ger_{non-connected}$.

Evidence on collateral for the subsample of loans for which such information is available shows that connected loans are less collateralized than loans between firms and banks without social connections. Loans from KU banks to KU firms are slightly more collateralized than loans from non-KU banks to KU firms before the

election (46.95 percent vs. 45.64 percent). However, after the election when KU firms become connected to KU banks, collateral relatively declines for these loans (36.41 percent vs. 42.42 percent). For loans to non-KU firms, we observe similar rates of collateral for KU and non-KU banks before (43.50 percent vs. 43.68 percent) and after (43.51 percent vs. 41.58 percent) the election. Additionally, better ability to renegotiate debt with socially connected firms is inconsistent with the high number of bankruptcy filings after 2012 for KU firms that previously renegotiated their debt with KU banks during the 2008 to 2012 period, which suggests that workouts between socially connected banks and firms merely postponed rather than prevented defaults by connected firms.

Another interpretation of the results on loan outcomes could be that social connections lead to better enforcement leading to lower default rates while firms and banks are connected. This could explain why default rates increase after 2012 when social connections between firms and banks cease. However, this interpretation of the results is inconsistent with several observations. First, it does not explain why connected loans need to be renegotiated more frequently while depicting similar or even slightly higher default rates. Second, it cannot explain why default rates are substantially higher after 2012 for loans from KU banks to KU firms than for other firms.

3.6.5 No Funding Constraints

Even if returns on loans to KU firms are relatively lower than for loans extended to non-KU firms, lending to KU firms may be profitable for KU banks, if they are not funding constraint and the additional loans to connected firms generate a positive return. A lower bound for the return on loans to connected firms is the risk-free rate, as banks always have the option to invest in the risk-free assets in case of having excess funds.

For KU banks to break even on loans allocated to KU firms relative to the average risk-free rate over the period from 2008 to 2015 the following inequality needs to be satisfied: $1.0582 * (1 - 0.0343 - 0.0291) + 0.0343 * rec_B + 0.0291 * rec_W - 1 \geq 0.0408$, where rec_B is the recovery rate in bankruptcy cases and rec_W is the recovery rate in workout cases. While there is no publicly available data on recovery rates in workouts, the Korea Financial Investment Association publishes value-weighted recovery rates for corporations with assets above 7bn KRW, which are precisely the firms for which accounting information is available. For our sample period the recovery rate in bankruptcy (rec_B) is 19.27 percent. With this recovery rate, loans from KU banks to KU firms yield returns of 1.40 percentage points below the risk-free rate even if we assume rec_W to equal one. To break even, the recovery rate for rec_B would need to be 60 percent, which is implausible given the observed recovery rates in bankruptcy cases in Korea. This suggests that KU banks would be better off to invest in the risk-free asset rather than lending to KU firms between 2008 and 2015. Thus, even in the absence of funding constraints, loans to KU firms between 2008 and 2015 are unprofitable from the perspective of KU banks.

3.6.6 Unobserved Costs

Our comparison of the profitability of KU banks' lending to KU firms and non-KU firms relies on observable costs and benefits. It is possible that we miss unobservable costs that are lower for connected loans. For example, screening and monitoring costs may be lower for loans to firms that are socially connected. However, while the omission of this type of costs may bias the comparison of loans allocated to KU and non-KU firms, transaction costs for investing in the risk-free asset are low. Given that for plausible levels of recovery rates, loans from KU banks to KU firms generate returns below the risk-free rate, adding additional

costs would make connected loans even less profitable compared to the risk-free rate, even if these costs were lower than for unconnected loans.

3.6.7 Cross-Selling

KU banks may be willing to accept lower returns on loans extended to KU firms, if they are able to secure related business transactions, for example advisory fees, through their lending to these firms. For all major banks in Korea, fees for consulting services are included in interest rates and not charged separately. The main alternative source of income for banks from lending relationships with firms is to sell them additional financial products, for example derivatives. Given an average loan size of 4,047bn KRW, the additional income from selling other financial products would need to amount to 55.85m KRW per firms for loans from KU banks to KU firms to at least match the risk-free rate given the 1.38 percent below the risk-free rate returns from connected lending. While some of the larger firms in the sample rely on additional financial products, the majority of firms in the sample does not list any or very little other financial assets on their balance sheet. From their balance sheet, we find that the average KU firm has outstanding derivatives of 18.15m KRW in a given year. Even if all of this was provided by KU banks, the profits from the sales would not recover 55.85m KRW in profits for connected banks. While there might be additional products other than consulting and derivatives that are not visible on the balance sheet, the volume that would be required to generate sufficiently high profits to increase overall returns to the risk-free rate seems implausibly high.

3.6.8 Additional Credit and Credit Reallocation

Since our estimates are derived from a difference-in-differences model, the increase in credit allocated to KU firms relative to non-KU firms is the sum of extra credit allocated to KU firms and a reduction in credit allocated to non-KU firms. If part of the extra credit allocated to KU firms would have been allocated to non-KU firms, our estimates may be biased upwards. While it is not possible for us to explore whether the additional credit allocated to KU firms would have been allocated to non-KU firms in the absence of connections, we check how much our estimates could be biased under the most conservative assumptions. If we reallocated all extra credit allocated to KU firms to non-KU firms for our main test in Table ??, column I, since KU firms constitute 2.36 percent of the firms in the sample, credit to non-KU firms would increase by $0.1669 \times (0.0236 / 0.974) = 0.0040$. This implies that under the most conservative assumption, our estimate would decline only slightly from 0.1669 to 0.1629. Similarly, our estimate at the firm-bank relationship level in column IV would decline only slightly from 0.6135 to 0.5986. Thus, concerns about potential estimation biases due to effects of connections on credit allocation to non-KU firms do not qualitatively affect our results and the economic magnitude is minor even under the most conservative assumptions.

3.7 Costs of Connected Lending

The results in the paper suggest that private banks earn lower returns by lending to socially connected firms compared to lending to non-connected firms. In this section, we provide an estimate of the aggregate losses to banks from engaging in favorable lending to connected firms. This requires us to make several assumptions and our estimates should therefore only be interpreted as an approximation.

The evidence from higher incidence of workouts and lower collateralization of connected loans suggests that recovery rates are rather lower for connected loans than for non-connected loans. However, as a conservative choice, we apply the same recovery rate to both types of loans and assume recovery rates in renegotiations (rec_W) to be 100 percent, which is likely to lead us to underestimate differences in returns of loans from KU banks to KU firms and non-KU firms.

Based on these assumptions, the return on loans from KU banks is $(1 - 0.0343 - 0.0291) * 1.0582 + 0.0343 * 0.1927 + 0.0291 * 1 - 1 = 0.0268$ for loans to KU firms, and $(1 - 0.0131 - 0.0010) * 1.0613 + 0.0131 * 0.2 + 0.0010 * 1 - 1 = 0.0500$ for loans to non-KU firms. If KU banks could generate the same average return on the funds used to lend to KU firms as for their other loans to non-KU firms, they could generate 2.32 percentage points higher returns. The return differential for loans from KU banks to KU firms and non-KU firms is similar to existing evidence in the literature. Haselmann, Schoenherr and Vig (2018) find that in Germany private banks earn 2.23 percentage points lower return on loans to socially connected firms, which is close to our estimate of 2.32 percentage points. This return differential between connected and non-connected firms for private banks is smaller than what is observed for state banks based on estimates in Khwaja and Mian (2005) with 6.08 percentage points in Pakistan and Haselmann, Schoenherr and Vig (2018) with 6.63 percentage points in Germany. The similarity of the estimates across these different institutional settings suggests that the forces at work may apply more broadly and the effects we document are relevant beyond the Korean context.

10.11 percent of KU banks' loans are allocated to KU firms. Thus, KU banks generate $0.0232 * 0.1011 = 0.235$ percentage points or $0.235/5 = 4.69$ percent lower returns on their loan portfolio. Across all private banks, 6.21 percent of loans go to firms connected to the bank through executives' alumni networks. Assuming the same return differential between connected and non-connected loans for all

alumni network, private bank lending is $0.0232 * 0.0621 = 0.144$ percentage points or $0.144/5 = 2.88$ percent less profitable due to connected lending based on alumni networks alone. In terms of returns over the risk free rate, the costs of connected lending reduce private banks' returns by 15.66 percent ($0.144/(5.00 - 4.08)$).

The aggregate annual lending of private banks to firms in Korea amounts to 28 percent of GDP. Thus, the loss of 0.144 percentage points of banks returns on corporate loans translates into a loss of $0.144 * 0.28 = 0.04$ percent of GDP. Credit misallocation is likely to generate additional costs beyond those accruing to banks, but these are more difficult to quantify. For example, the higher share of connected loans being used for refinancing rather than productive investment suggests that banks' funds may not be allocated to their most profitable use.

3.8 Conclusion

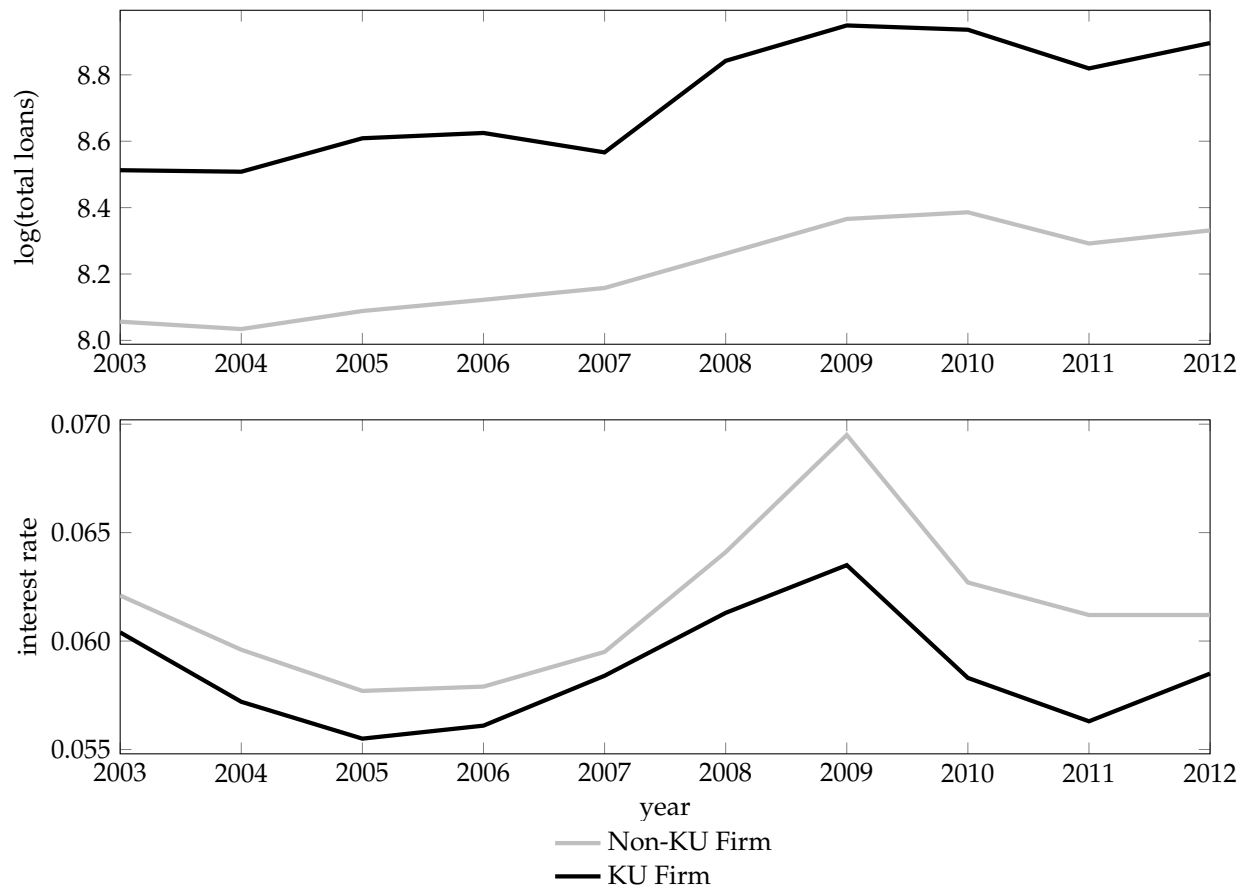
In this paper, we document that changes in political power have important spillover effects on resource allocation in private markets. We show that by seeking connections to the administration dominated by people from the new president's network, private banks that are not directly controlled by the government increase the number of executives linked to the president's networks. This increases the control over resource allocation for people from the president's networks in the private sector. In turn, private firms connected to the president's social network benefit from better access to resources in private markets through this social network channel. While the social network channel operates independently of political connections, connections to a politician amplify the magnitude of the network effect by increasing the amount of resources allocated through the network.

With respect to the mechanism underlying better access to private bank loans for firms connected to banks through their executives, we find that the dominant

mechanism is taste-based discrimination. Private banks allocate more credit at a cheaper price to connected firms and protect connected firms from default by showing a higher willingness to renegotiate connected firms' debt. Once connections between firm and bank executives cease, the higher riskiness of the loans allocated to connected firms ex ante is reflected in higher default rates ex post.

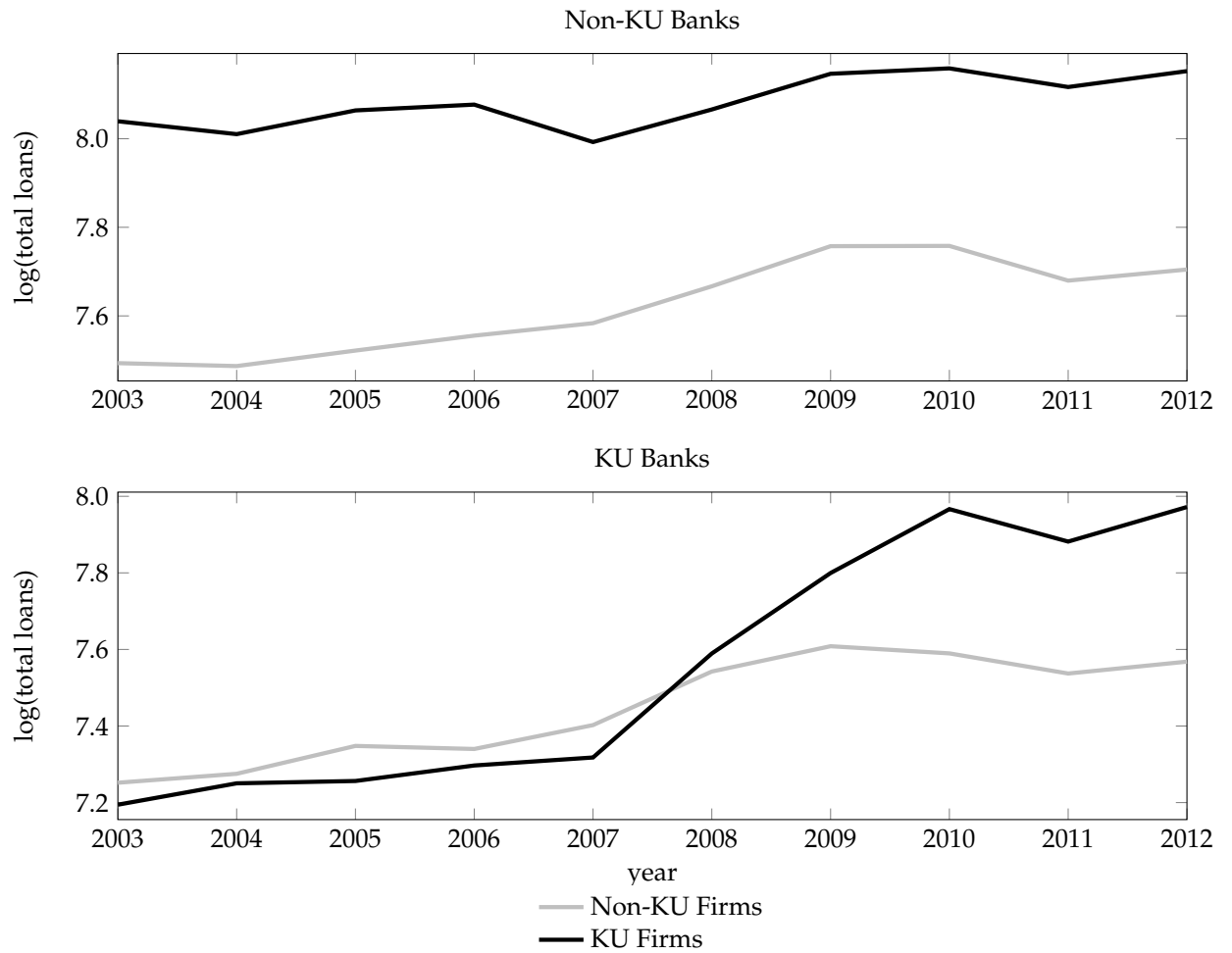
The additional benefits that politically connected firms receive in private markets through the social network channel contribute to the explanation of the high returns to politically connected firms that have been documented for many countries around the world since the original paper by Fisman (2001). The large benefits of social connections to private banks additionally suggest that social connections are an important consideration for firms when hiring CEOs. Since social connections seem to mainly reallocate rents across firms rather than generating productive returns, hiring CEOs based on their social connections may crowd out other skills that improve firms' productivity. Additionally, social connections of bankers seem to distort the allocation of capital, consistent with the evidence in Haselmann, Schoenherr and Vig (2018). By allocating capital to ailing connected firms that use a larger fraction of the credit for refinancing, banks may withhold capital from more productive usage (Caballero, Hoshi and Kashyap 2008) and thwart creative destruction (Schumpeter 1942).

Figure 3.1: Change in Credit and Interest Rates - Firm-Level



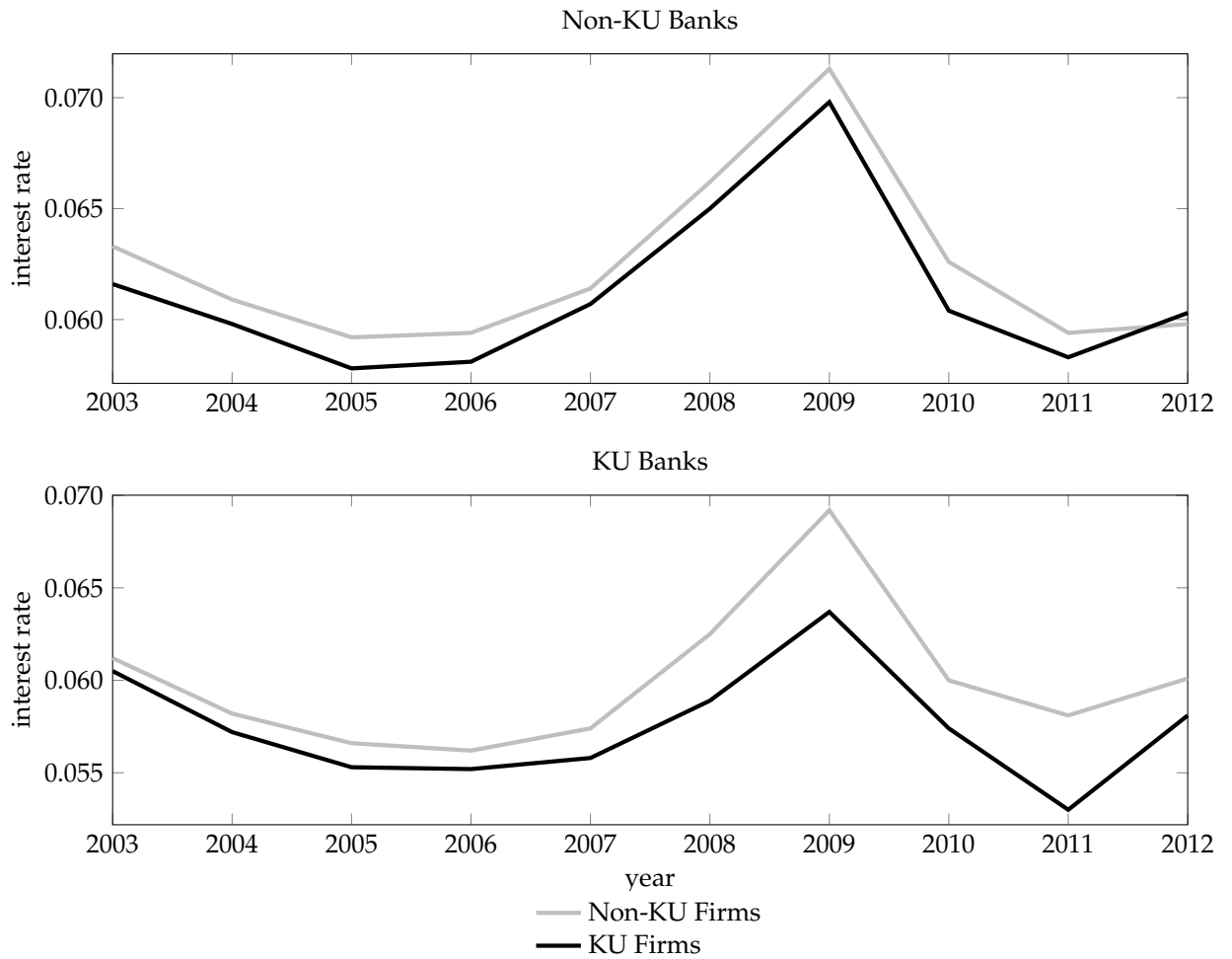
This figure depicts the average log of annual loans in the top panel and the average interest rate in the bottom panel separately for KU firms (black lines) and for non-KU firms (gray lines).

Figure 3.2: Change in Credit - Relationship-Leve



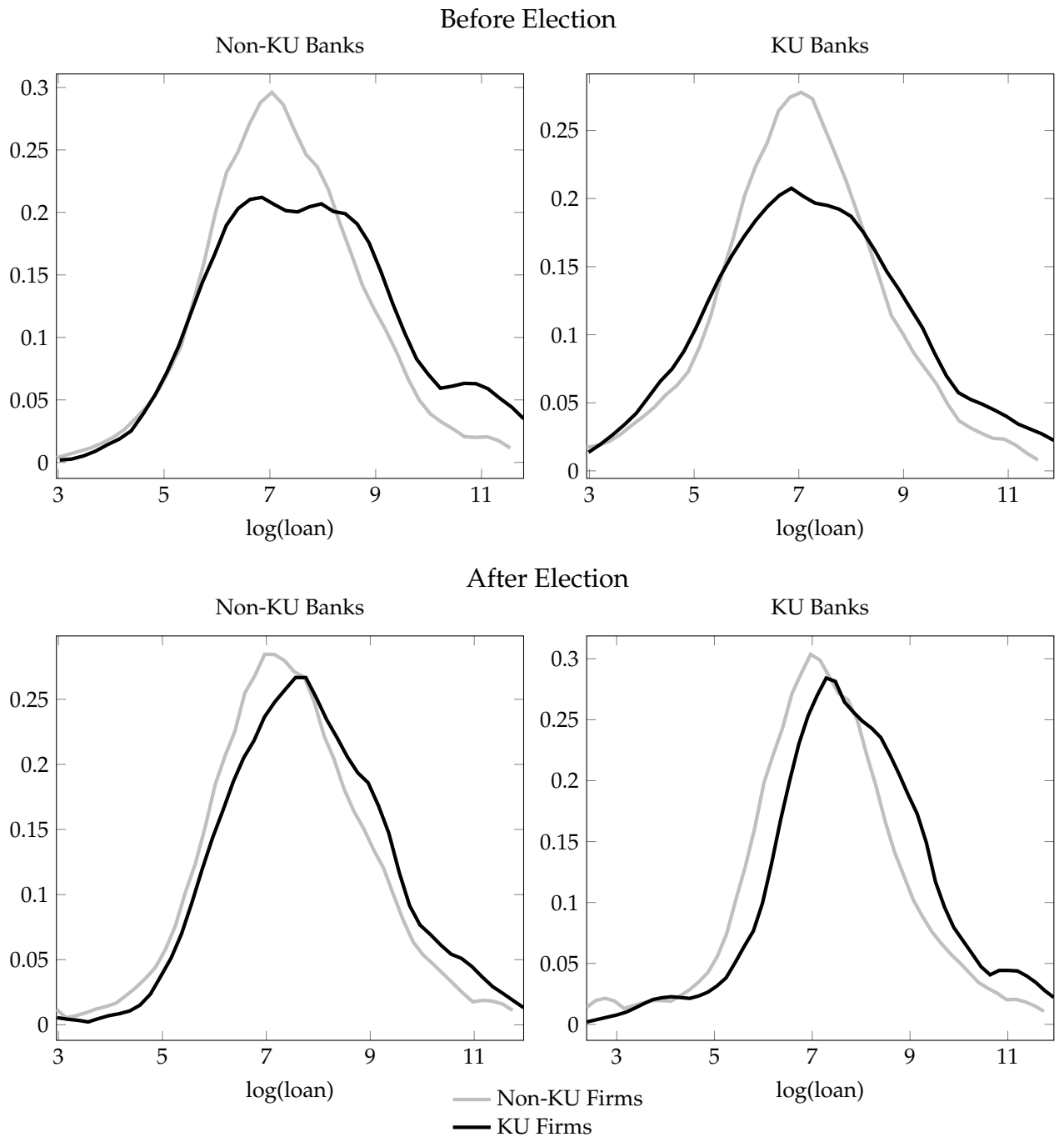
This figure depicts the average log of annual loans for KU firms (black lines) and non-KU firms (gray lines). The top panel plots the graph for non-KU banks, the bottom panel plots the graph for KU banks.

Figure 3.3: Change in Interest Rates - Relationship-Level



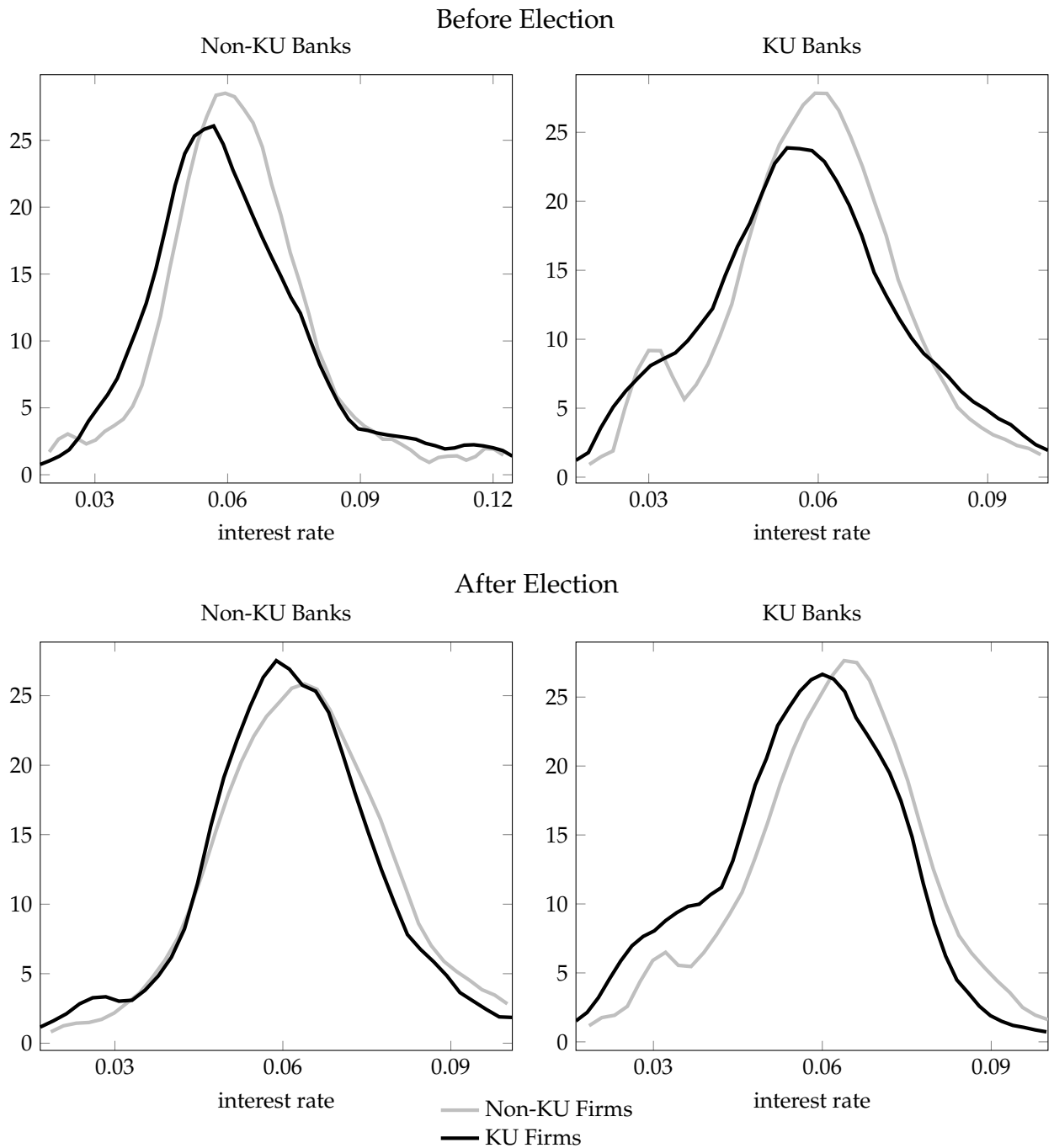
This figure depicts the average interest rates for KU firms (black lines) and non-KU firms (gray lines). The top panel plots the graph for non-KU banks, the bottom panel plots the graph for KU banks.

Figure 3.4: Loan Distribution



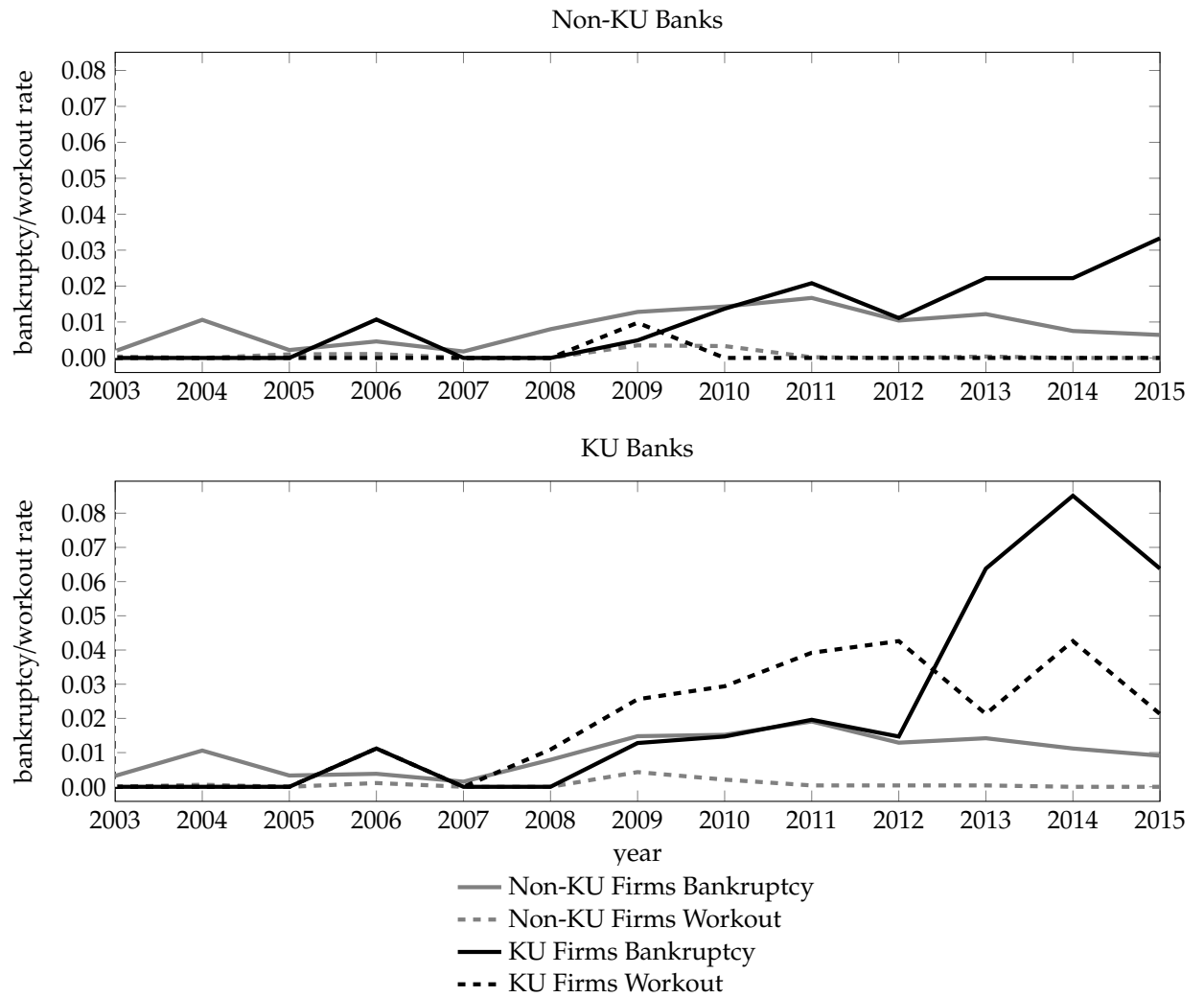
This figure shows kernel density plots of the log of loans on the firm-bank relationship level for the pre-election (top panel) and the post-election (bottom panel) periods for KU banks (right panel) and non-KU banks (left panel), and for KU firms (black lines) and non-KU firms (gray lines).

Figure 3.5: Interest Rate Distribution



This figure shows kernel density plots of interest rates on the firm-bank relationship level for the pre-election (top panel) and the post-election (bottom panel) periods for KU banks (right panel) and non-KU banks (left panel), and for KU firms (black lines) and non-KU firms (gray lines).

Figure 3.6: Defaults



This figure depicts the fraction of firms borrowing from KU banks (bottom panel) and non-KU banks (top panel) that file for bankruptcy (solid lines) or workout (dashed line) among KU firms (black lines) and non-KU firms (gray lines) between 2003 and 2015.

Table 1: Descriptive Statistics

Panel A: Sample				
Number of firms	9,729			
Number of firms with accounting data	8,649			
Panel B: CEO & Network Data				
	in 2007	in 2003		
Korea University firms	2.36%	2.35%		
Panel C: Loan Data				
	Obs.	Mean	Median	Std.
Individual loans (in million KRW)				
KU Firms	3,595	4,047	1,585	5,443
Non-KU Firms	97,488	3,031	1,121	4,507
Interest rates				
KU Firms	3,595	0.0590	0.0592	0.0169
Non-KU Firms	97,488	0.0614	0.0615	0.0168
Lending relationships				
KU Firms	401	2.49	2.00	1.60
Non-KU Firms	13,077	2.18	2.00	1.36
Total annual loan amount (in million KRW)				
KU Firms	1,146	14,499	5,857	18,308
Non-KU Firms	33,055	9,784	4,226	13,973
Panel D: Accounting Data (in million KRW)				
	Obs.	Mean	Median	Std.
Assets (in million KRW)				
KU Firms	867	109,492	42,160	80,175
Non-KU Firms	30,613	53,928	26,651	64,620
Employees				
KU Firms	849	169	107	153
Non-KU Firms	29,614	114	70	125
Net income (in million KRW)				
KU Firms	830	2,310	940	5,165
Non-KU Firms	30,614	973	506	4,275
Tangible Assets/Assets				
KU Firms	867	0.3594	0.3216	0.2453
Non-KU Firms	30,249	0.3653	0.3413	0.2675

This table provides descriptive statistics. Panel A shows information on the number of firms, Panel B depicts information on CEOs' network connections, Panel C provides information on the loan data, and Panel D depicts descriptive statistics from accounting data.

Table 2: Credit Volumes and Interest Rates

	I	II	III	IV	V	VI	VII
	Firm-Level			Relationship-Level			
Dep. Var.:	$\Delta \log(\text{loans})_i$	$\Delta \left(\frac{\text{loans}}{\text{assets}} \right)_i$	ΔIR_i	$\Delta \log(\text{loans})_{ij}$	$\Delta \left(\frac{\text{loans}}{\text{assets}} \right)_{ij}$	ΔIR_{ij}	Δrel_{ij}
KU_i	0.1669** (0.0666)	0.0146*** (0.0054)	-0.0018** (0.0008)				
KU_{ij}				0.6135*** (0.1338)	0.0228*** (0.0055)	-0.0054*** (0.0014)	0.0991** (0.0426)
con_{ij}				0.5915*** (0.1505)	0.0299*** (0.0084)	-0.0045** (0.0019)	0.0215 (0.0426)
Firm FE	-	-	-	yes	yes	yes	yes
Bank FE	-	-	-	yes	yes	yes	yes
Clustered SE	-	-	-	firm	firm	firm	firm
Observations	3,738	3,528	3,749	6,110	5,835	6,136	23,339
R-squared	0.001	0.001	0.001	0.660	0.0694	0.691	0.710

This table shows the results from estimating equation (3.1) in columns I to III, and equation (3.2) in columns IV to VII. The dependent variable is the log change in total loans for firm i after the election (2008-2012) compared to before the election (2003-2007) in column I, the change in loans to assets for firm i in columns II, the change in the average interest rate for firm i in column III, the log change in total loans from bank j to firm i in column IV, the change in loans from bank j to firm i , scaled by firm i 's assets in column V, the change in the average interest rate on loans from bank j to firm i in column VI, and takes the value of one if bank j and firm i start a new lending relationship after the election, and zero otherwise, in column VII. The variable KU_i takes the value of one for firms with a CEO from Korea University in 2007, and zero otherwise, the variable KU_{ij} takes the value of one for firm-bank relationships in which firm i has a CEO from the KU network in 2007 and bank j appoints an executive from Korea University after the election. The variable con_{ij} takes a value between minus one and one, and increases by 0.2 for each year in which firm i has a CEO from an alumni network from which bank j has an executive after the election, and decreases for each year in which firm i and bank j are connected to the same alumni network before the election. Standard errors are reported in parentheses. ***, and ** indicate statistical significance at the 1%, and 5% levels, respectively.

Table 3: Credit Volumes and Interest Rates - Pre-Connected Banks

	I	II	III	IV	V	VI
Dep. Var.:	$\log(\text{loans})_{ijt}$		$\Delta \log(\text{loans})_{ij}$	IR_{ijt}		ΔIR_{ij}
	pre	post		pre	post	
$KU_{ij,pre}$	0.4074*** (0.1221)	0.2667** (0.1342)	-0.0557 (0.1323)	-0.0031** (0.0015)	-0.0036** (0.0014)	0.0006 (0.0019)
con_{ijt}	0.6457*** (0.2495)	0.4904** (0.2305)		-0.0049** (0.0022)	-0.0064** (0.0026)	
con_{ij}			0.7427*** (0.2472)			-0.0067*** (0.0026)
Firm FE	yes	yes	yes	yes	yes	yes
Bank FE	yes	yes	yes	yes	yes	yes
Clustered SE	firm	firm	firm	firm	firm	firm
Observations	12,770	11,156	4,049	12,806	11,178	4,068
R-squared	0.734	0.816	0.754	0.723	0.795	0.771

This table shows the results from estimating equation (3.3). The dependent variable is the log of total lending from bank j to firm i in columns I and II, the log change in total loans from bank j to firm i after the election (2008-2102) compared to before the election (2003-2007) in column III, the average interest rate on loans from bank j to firm i in columns IV and V, and the change in the average interest rate on loans from bank j to firm i in column VI. In columns I and IV the sample is restricted to the pre-election period, in column II and V the sample is restricted to the post-election period. The variable $KU_{ij,pre}$ takes the value of one for firm-bank pairs in which firm i has a CEO from Korea University in 2007 and bank j has an executive from Korea University before and after the election. The variable con_{ijt} takes a value between zero and one increasing by 0.2 for each year firm i and bank j are connected to the same alumni network before the election in columns I and IV, or after the election in columns II and V. The variable con_{ij} takes a value between minus one and one and increases by 0.2 for each year in which firm i has a CEO from an alumni network from which bank j has an executive after the election, and decreases for each year in which firm i and bank j are connected to the same alumni network before the election. Standard errors are reported in parentheses. ***, and ** indicate statistical significance at the 1%, and 5% levels, respectively.

Table 4: Credit Volumes and Interest Rates - Post MB Era

	I	II	III	IV	V	VI	VII
	Firm-Level			Relationship-Level			
Dep. Var.:	$\Delta \log(\text{loans})_i$	$\Delta \left(\frac{\text{loans}}{\text{assets}} \right)_i$	ΔIR_i	$\Delta \log(\text{loans})_{ij}$	$\Delta \left(\frac{\text{loans}}{\text{assets}} \right)_{ij}$	ΔIR_{ij}	Δrel_{ij}
KU_i	-0.4236*** (0.1216)	-0.0154** (0.0074)	0.0038** (0.0019)				
KU_{ij}^P				-0.5573*** (0.1457)	-0.0251** (0.0112)	0.0064** (0.0032)	-0.0991*** (0.0337)
con_{ij}^P				-0.5828*** (0.1713)	-0.0400*** (0.0125)	0.0064** (0.0029)	-0.0576 (0.0368)
Firm FE	-	-	-	yes	yes	yes	yes
Bank FE	-	-	-	yes	yes	yes	yes
Clustered SE	-	-	-	firm	firm	firm	firm
Observations	2,726	2,662	2,730	4,283	4,200	4,293	29,314
R-squared	0.004	0.001	0.002	0.652	0.717	0.713	0.644

This table shows the results from estimating equation (3.1) in columns I and II, and equation (3.2) in columns III to V. The dependent variable is the log change in total loans for firm i after the election of Park Geun Hye (2013-2015) compared to before her election (2010-2012) in column I, the change in the average interest rate for firm i in columns II, the log change in total loans from bank j to firm i in column III, the change in the average interest rate on loans from bank j to firm i in column IV, and takes the value of minus one if bank j and firm i end a lending relationship after the election, and zero otherwise, in column V. The variable KU_i takes the value of one for firms with a CEO from Korea University in 2007, and zero otherwise, the variable KU_{ij}^P takes the value of one for firm-bank relationships in which firm i has a CEO from the KU network in 2007 and bank j ceases to appoint an executives from Korea University after park's election. The variable con_{ij}^P takes a value between minus one and one, and increases by 0.2 for each year in which firm i has a CEO from an alumni network from which bank j has an executive before Park's election, and decreases for each year in which firm i and bank j are connected to the same alumni network after the election. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5: Underlying Mechanism

	I	II	III	IV	V	VI	VII	VIII
Dep. Var.:	$\Delta \log(\text{loans})_{ij}$	$\Delta \left(\frac{\text{loans}}{\text{assets}} \right)_{ij}$	ΔIR_{ij}	Δrel_{ij}	$\Delta \sigma(\log(\text{loans}))_{ij}$	$\Delta \sigma(IR)_{ij}$	$\Delta fin\ share_i$	$\Delta fin\ share_{ij}$
KU_i					0.1365** (0.0610)	-0.0003 (0.0004)	0.0739** (0.0299)	
KU_{ij}	0.4285** (0.2145)	0.0113 (0.0075)	-0.0011 (0.0547)	-0.0330 (0.0520)	-0.1812* (0.0981)	-0.0027*** (0.0006)		0.3006*** (0.0765)
con_{ij}	0.6083*** (0.1529)	0.0308*** (0.0085)	-0.0044** (0.0019)	0.0071 (0.0504)				0.3329* (0.1720)
$KU_j * Risk_i$	-0.0107 (0.0134)	0.0001 (0.0007)	0.0000 (0.0001)	-0.0021 (0.0031)				
$KU_{ij} * Risk_i$	0.0523* (0.0275)	0.0023* (0.0012)	-0.0010** (0.0005)	0.0155** (0.0061)				
Firm FE	yes	yes	yes	yes	-	-	-	yes
Bank FE	yes	yes	yes	yes	yes	yes	-	yes
Clustered SE	firm	firm	firm	firm	firm	firm	-	firm
Observations	5,993	5,804	6,018	14,903	59	59	1,620	2,356
R-squared	0.658	0.694	0.284	0.444	0.680	0.825	0.012	0.792

The dependent variable in this table is the log change in total loans from bank j to firm i after the election compared to before the election in column I, the change in total loans from bank j to firm i scaled by firm i 's assets in column II, the change in the interest rates for loans from bank j to firm i in columns III, a dummy variable that takes the value of one if bank j and firm i start a new lending relationship, and zero otherwise in column IV, the change in the standard deviation of the log of loans and interest rates on loans to KU and non-KU firms, respectively for bank j after compared to before the election in columns V and VI, the change in the fraction of loans that firm i uses to refinance existing loans in column VII, and the fraction of loans from bank j to firm i used for refinancing existing loans in column VIII. The variable KU_i takes the value of one for firms with a CEO from Korea University in 2007, and zero otherwise, the variable KU_j takes the value of one if bank j appoints an executive from Korea University after the election, the variable KU_{ij} takes the value of one for firm-bank relationships in which firm i has a CEO from the KU network in 2007 and bank j appoints an executive from Korea University after the election, the variable con_{ij} takes the value of one if firm i has a CEO from an alumni network from which bank j has an executive only after the election, minus one if firm i and bank j are connected to the same alumni network only before the election, and zero otherwise, and the variable $Risk_i$ sorts firms into risk deciles based on their interest coverage with the variable taking the value of one for the safest up to ten for the riskiest firms. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Loan Outcomes - Defaults and Workouts

	I	II	III
Dep. Var.:	<i>bankruptcy_{it}</i>	<i>workout_{it}</i>	<i>total defaults_{it}</i>
KU_i	-0.0027 (0.0056)	-0.0010* (0.0005)	-0.0037 (0.0057)
KU_{ij}	-0.0003 (0.0016)	0.0003 (0.0004)	0.0001 (0.0017)
$KU_i * Post_t$	-0.0061 (0.0107)	0.0040 (0.0045)	-0.0017 (0.0115)
$KU_{ij} * Post_t$	0.0061 (0.0142)	0.0582*** (0.0197)	0.0573** (0.0230)
Bank FE	yes	yes	yes
Time FE	yes	yes	yes
Clustered SE	firm	firm	firm
Observations	29,475	29,475	29,475
R-squared	0.006	0.009	0.007

This dependent variable in this table is a dummy variable that takes the value of one if firm i files for bankruptcy in year t and zero otherwise in column I, a dummy variable that takes the value of one if firm i files for workout in year t , and zero otherwise in column II, and a dummy variable that takes the value of one if firm i files for either bankruptcy or workout in year t , and zero otherwise in column III. The variable KU_i takes the value of one for firms with a CEO from Korea University in 2007, and zero otherwise, the variable KU_{ij} takes the value of one for firm-bank relationships in which firm i has a CEO from the KU network in 2007 and bank j appoints an executive from Korea University after the election, and the variable $Post_t$ takes the value of one for the post-election period (2008-2012), and zero for the pre-election period (2003-2007). Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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Appendix A

Supplementary Materials for Chapter 1

A.1 Institutional Details

A.1.1 Corporate and Capital Tax Rates in Korea

Corporate Income Tax Rates on Profits

In Korea, from 2005 to 2007, the corporate income tax rate is 13% for profits below \$100,000, and 25% for profits above. From 2008 to 2011, the profit threshold had increased to \$200,000, and the tax rates below and above the cutoff were reduced to 10% and 22%, respectively. From 2012, the government has added a third profit threshold of \$20 million, has reduced the tax rate in the middle category to 20%, and has kept the top corporate tax rate at 22%. Although there were changes in corporate income tax rates across time in Korea, the last change happened at the end of 2011, which was 3 years before the main reform that I exploit for identification. More importantly, the profit threshold was even low for many firms in my sample, so unlikely had influenced their investment following the reform in 2014. I confirm that this issue does not affect my results using placebo tests.

Dividend Tax Rates

In Korea, dividends are taxed similarly as individual incomes. If an investor's dividend income in a given year is less than \$20,000, then the tax rate is 15.4%. However if dividend income is above \$20,000, then it becomes part of the investor's income, and the marginal tax rate can rise up to 38%, depending on his total income. From 2005 and 2010, the top dividend tax rate was 35% and increased to 38% in 2011.

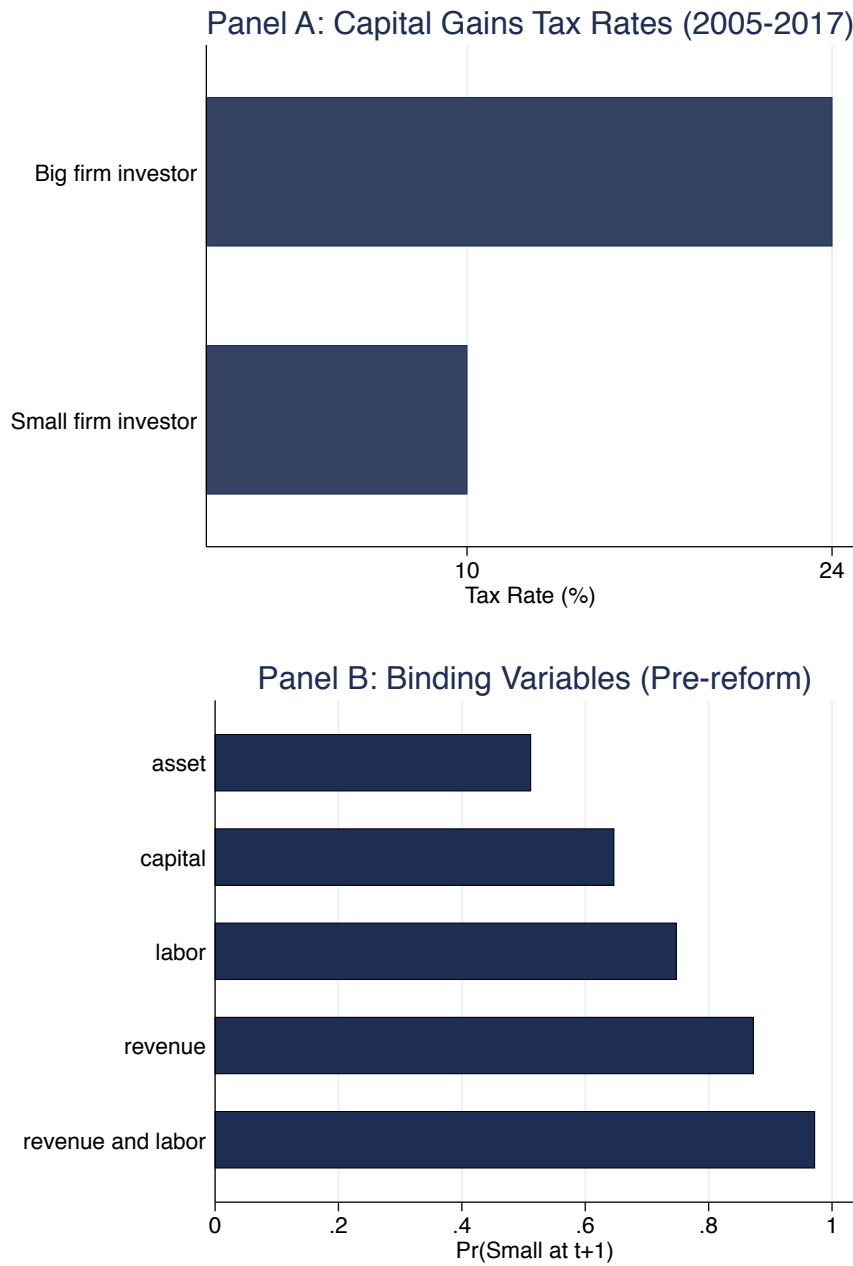
Binding Variables and Conditional Density

Table 1: Conditional Probability Matrix for each running variable

	Binding		Less Binding	
	(1) Rev below cutoff	(2) Lab below cutoff	(3) Cap below cutoff	(4) Asset below cutoff
Rev below cutoff	1 (0)	0.779 (0.415)	0.734 (0.442)	0.607 (0.488)
Lab below cutoff	0.872 (0.334)	1 (0)	0.774 (0.418)	0.678 (0.467)
Cap below cutoff	0.945 (0.229)	0.890 (0.313)	1 (0)	0.785 (0.411)
Asset below cutoff	0.996 (0.0642)	0.993 (0.0857)	0.999 (0.0259)	1 (0)
Observations	5799	6493	7467	9509

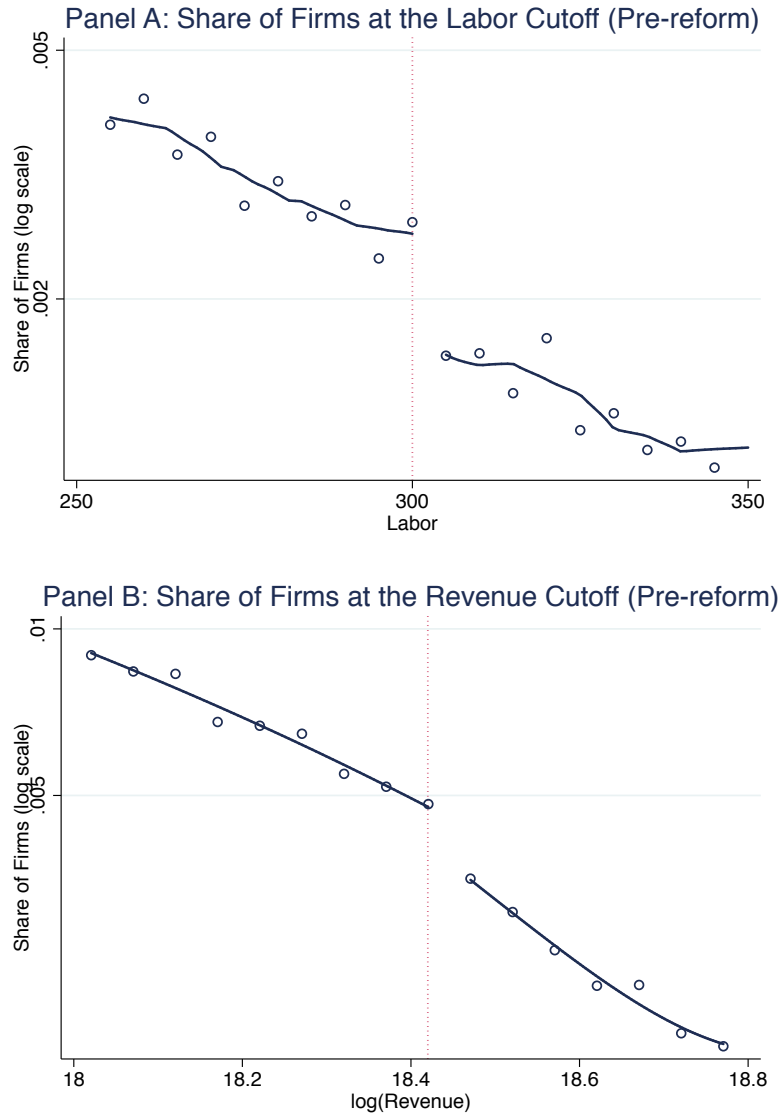
Notes: This table reports the conditional probability matrix for each of the running variable determining firm size prior to the reform 2014. Each row represents the probability of a running variable being below its own threshold conditional on the other running variable is either below or above its own threshold. For example, the cell in column 1 and row 2 presents the conditional probability that a firm is below the labor cutoff, given that the firm is already below the revenue threshold. On average, 87.2% of the firms that are below the revenue cutoff are also below the labor threshold. The standard deviation for each estimate is reported in the parenthesis. From these conditional probabilities, I conclude that the most binding determinants of firm size were total revenues and average employees.

Figure A.1: Capital Gains Tax Rates (2005 - 2017) and Binding Variables (Pre-reform)



Notes: The figure in Panel A shows the capital gains tax rates in South Korea from 2005 to 2017. The y-axis indicates the type of investors and the x-axis indicates the average tax rates by firm size. These rates remained the same from 2005 to 2017. The figure in Panel B shows the probability that a given firm becomes or remains small in the next period conditional that the firm is below each threshold in the current period. The x-axis represents the probability that a firm is small in the next period. The y-axis represents that the firm is below each threshold in the current period.

Figure A.2: Firm density at Firm-size Cutoff (Pre-reform)



Notes: Panel A in this figure shows the firm density at the labor cutoff, conditional that the firms are jointly below the other thresholds (revenue, total capital, and assets). The cutoff is at the labor of 300, and the bin size is 5 average employees. The hallow dots indicate the share of firms at a given bin. The solid lines are the local polynomial smooth plots, fitted to below and above the cutoff separately. The McCrary (2008) test rejects the null that the coefficient at the jump is statistically not different from zero. Panel B shows the firm density at the revenue cutoff, conditional that the firms are jointly below the other thresholds (labor, total capital, and assets). The cutoff is at the revenue of 100 million dollars, and the bin size is 5 log points in revenues. The McCrary (2008) test rejects the null that the coefficient at the jump is statistically not different from zero.

A.1.2 How to Account for Subsidiaries for Tax Purposes

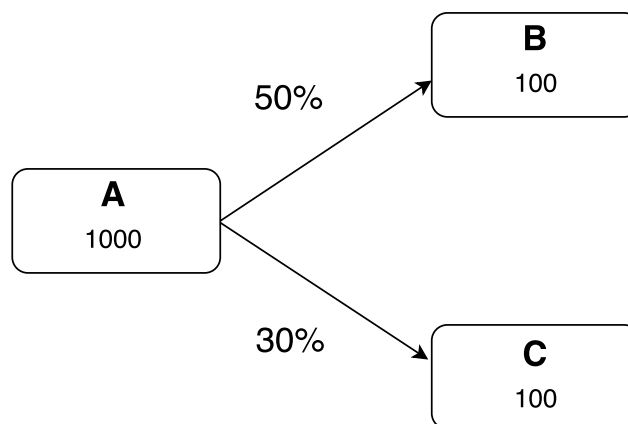
Table 2: Computing Accounting Variables for Tax Purposes

Firm	Relationship	Labor	Ownership	Labor Size for Tax Purposes
Case 1				
A	Parent to B & C	1000	-	$1000 + (1.0) * 100 + (0.3) * 100 = 1130$
B	Subsidiary to A	100	50%	$100 + (1.0) * 1000 = 1100$
C	Subsidiary to A	100	30%	$100 + (0.3) * 1000 = 400$
Case 2				
X	Parent to Y	3000	-	$3000 + (1.0) * 2000 + (0.5) * 1000 = 5500$
Y	Parent to A	2000	50%	$2000 + (1.0) * 3000 + (1.0) * 1000 + (0.5) * 100 = 6050$
A	Parent to B	1000	50%	$1000 + (0.5) * 3000 + (1.0) * 2000 + (1.0) * 100 + (0.5) * 50 = 4625$
B	Parent to C	100	50%	$100 + (0.5) * 2000 + (1.0) * 1000 + (1.0) * 50 = 2150$
C	Subsidiary to B	50	50%	$50 + (0.5) * 1000 + (1.0) * 100 = 650$

Notes: This table shows how to compute values for a firm's accounting variables for tax purposes. In Case 1 example, I assume that firm A is the parent company with two subsidiaries, namely B and C. Assume that each of the subsidiary does not own any other subsidiary (if it does, then it will just become a part of the parent firm's subsidiary). The column, "Labor", denotes how many average employees each firm used in a given operating period of time. Given the rules described in Section 1.2, each firm's labor input for tax purposes is computed as shown in the last column. For example, to compute the parent company's labor size for tax purposes, we add a subsidiary's labor input multiplied by the ownership rate if the rate is less than 50% and add the entire labor input of firm y since A owns at least 50%. In Case 2 example, we compute the accounting values for parent firms subsidiaries in a similar way, except that if a parent firm owns a grandchild firm through its subsidiary, then the parent firm's ownership of that firm is equal to its subsidiary's ownership rate of that firm if the ownership rate is at least 50%. If the ownership rate is less than 50%, then the parent firm's ownership of the grandchild firm is computed by multiplying two ownership rates together. To compute the values for other accounting variables (i.e. revenues, total capitals, assets), we repeat the same exercise for each variable.

Figure A.3: Computing Accounting Values

Case 1



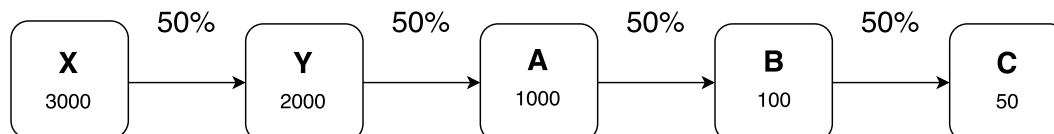
Notes: This figure shows how to compute accounting values for firms in a case where firm A owns just two subsidiaries, B and C. Suppose that firm A owns 50% of firm B and 30% of firm C, and that neither B nor C owns any subsidiary. Also, suppose that in a given year, firm A, B, and C used 1000, 100, and 100 employees on average, respectively. The government computes the average employee used for each firm in the following way:

(1) firm A: $1000 \times (100\% \text{ of firm A}) + 100 \times (100\% \text{ of firm B}) + 100 \times (30\% \text{ of firm C}) = 1130$

(2) firm B: $1000 \times (100\% \text{ of firm A}) + 100 \times (100\% \text{ of firm B}) = 1100$

(3) firm C: $1000 \times (30\% \text{ of firm A}) + 100 \times (100\% \text{ of firm C}) = 400$

Case 2



Notes: This figure shows how to compute accounting values for firms in a case where firm X owns 50% of Y, which owns 50% of A, which owns 50% of B, which owns 50% of C. Suppose that there's no other subsidiary involved in any of the firms. Also, suppose that in a given year, firm X, Y, A, B, and C used 3000, 2000, 1000, 100, and 50 employees on average, respectively. The government computes the average employee used for each firm in the following way:

(1) firm X: $3000 + (1.0) \times 2000 + (0.5) \times 1000 = 5500$

(2) firm Y: $2000 + (1.0) \times 3000 + (1.0) \times 1000 + (0.5) \times 100 = 6050$

(3) firm A: $1000 + (0.5) \times 3000 + (1.0) \times 2000 + (1.0) \times 100 + (0.5) \times 50 = 4625$

(4) firm B: $100 + (0.5) \times 2000 + (1.0) \times 1000 + (1.0) \times 50 = 2150$

(5) firm C: $50 + (0.5) \times 1000 + (1.0) \times 100 = 650$

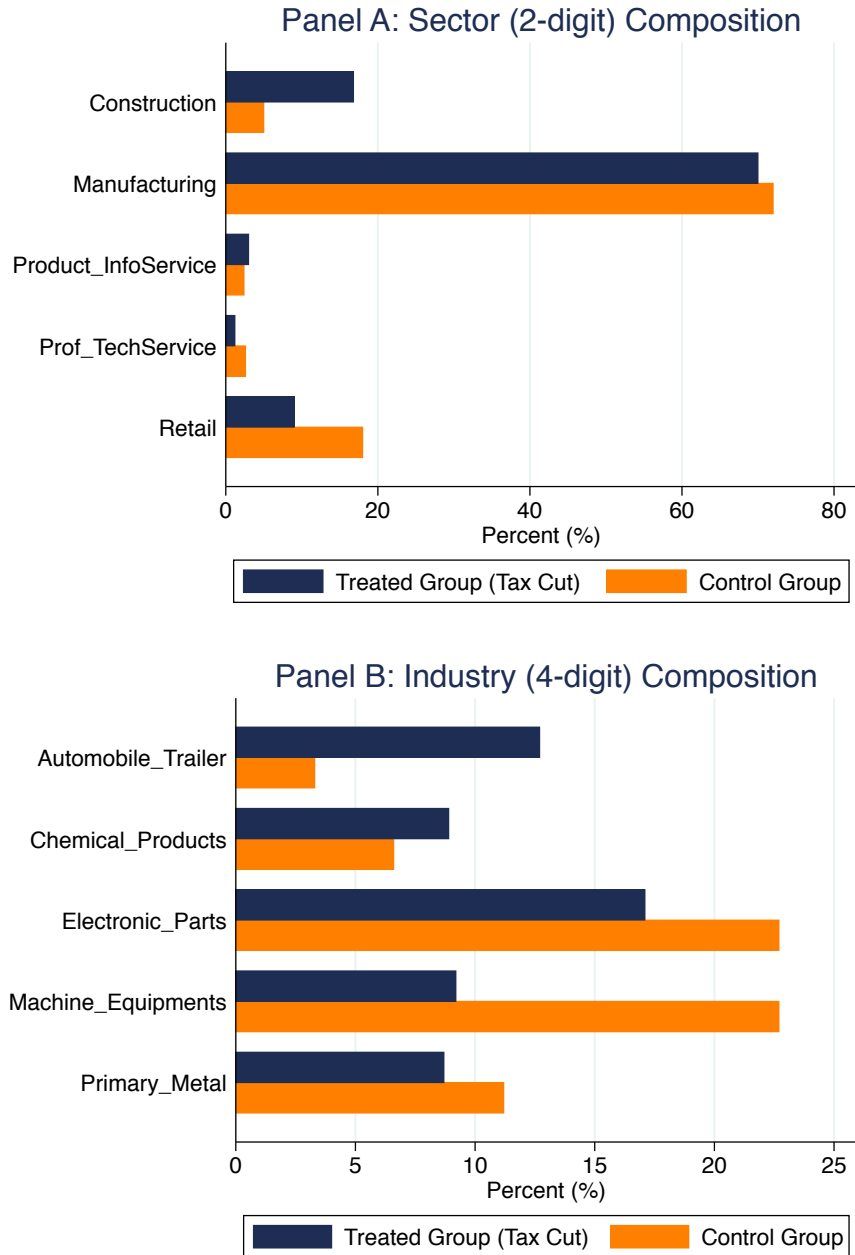
Reform Effects Across Industries

Table 3: New Threshold Across Industries

Sector	Industry Name	4-digit SIC	Avg Revenue
Manufacturing	Clothing and Textile	3140	150 million
Manufacturing	Leather, Bags, and Shoes	3150	150 million
Manufacturing	Pulp, Paper, and Paper Products	3170	150 million
Manufacturing	Primary Metal	3240	150 million
Manufacturing	Electronic Equipment	3280	150 million
Manufacturing	Furniture	3320	150 million
Manufacturing	Food	3100	100 million
Manufacturing	Tobacco	3120	100 million
Manufacturing	Fiber Product	3130	100 million
Manufacturing	Wooden Product	3160	100 million
Manufacturing	Coke and Oil Refinement	3190	100 million
Manufacturing	Chemical Material and Product	3200	100 million
Manufacturing	Rubber and Plastic Product	3220	100 million
Manufacturing	Metal Product	3250	100 million
Manufacturing	Electronic Parts, Computer, Telecom	3260	100 million
Manufacturing	Other Machine and Equipments	3290	100 million
Manufacturing	Automobile and Trailer	3300	100 million
Construction	Construction	6410	100 million
Retail	Retail	7470	100 million

Notes: This table describes how the reform in 2014 differentially impacted firms in different sectors and in different industries. Even though firms across all sectors had their labor threshold removed for firm size requirement and changed the revenue threshold into an average over past three years, the average cutoff increased to \$ 150 million only for the manufacturing sector and only for certain industries within the manufacturing sector. Firms within manufacturing sector and in other sectors, where the average revenue threshold did not increase to \$150 million are used as part of the control group if their average revenue was between \$100 million and \$150 million in 2014. Finally, firms in other industries or sectors whose average threshold decreased to \$80 million after the reform are used as part of the control group because the government granted them a three-year grace period so that their tax rate did not change during my sample period.

Figure A.4: Sector and Industry Composition by Treated and Control Groups



Notes: Panel A in this figure shows the sector composition of firms by treated and control groups. Panel B in this figure shows the industry composition of firms by treated and control groups.

A.1.3 Additional Tax Benefits for Small Firms

This subsection describes a set of tax benefits associated with being a small firm, other than the lower capital gains tax rates. I first describe each of these additional benefits, and then show related tests to argue that these additional benefits were not the main driver of the investment responses following the reform in 2014. For example, if there were additional tax benefits for being a small firm other than lower capital gains taxes, then the investment increase following the reform in 2014 would yield an over-estimate of the investment elasticity with respect to the net of capital gains tax rate. To test whether this was the case, I conduct several heterogeneity analysis based on firms more likely to get these additional tax benefits, and test whether these firms increased investment more. I find that the investment response is not larger for these firms more likely to get additional tax benefits, so I conclude that my findings are inconsistent with the potential upward bias, and these additional benefits are not big enough to provide investment incentives for these firms at the margin. Note that small firms that are eligible to apply for the additional tax benefits described below cannot apply for more than one extra benefit. For example, if a firm can deduct corporate tax burdens by being located in a rural area, then it cannot apply for research and development tax credits.

Small Firms in Rural Areas

Small firms in my analysis sample can get up to 15% reduction in corporate taxes if they are physically located in rural areas.¹ For example, if a small firm located in a rural region generates 1 million dollars in profits and faces marginal corporate tax rates of 10% (up to \$200,000) and 20% (above \$200,000), then it can deduct 15% of the tax burden (which amounts to \$180,000), so the effective tax rate decreases

¹In Korea, rural areas are defined as cities or provinces other than Seoul, Incheon, and Gyeong-Gi Province. In my sample, roughly 35% of firms are located in rural areas.

from 18% to 15.3%. Therefore, the amount of reduction in corporate tax burdens depends on the firm's profits in a given year for firms located in rural areas. If this additional corporate tax benefit provides investment incentives for small firms, then I would expect to see even a greater investment response for treated firms in rural areas compared to treated firms in urban areas. Thus, I repeat the same analysis for the main outcomes, just cutting the sample by their locations.

Table 4 shows the main results in Section 1.5, where I interact the dummy for $Treated \times Post$ with the dummy for whether a firm is located in a rural area (which is fixed at the time of the reform and accounts for roughly 35% of the analysis sample). As shown in the first column of the table, the difference-in-differences coefficient is positive and statistically significant, implying that affected firms in urban areas increase investment following the reform. However, the triple difference coefficient is statistically insignificant, but negative, suggesting that affected firms in rural areas did not increase investment more relative to those in urban areas, even if there were additional tax benefits to do so. Therefore, the fact that there were additional benefits for small firms in rural areas does not seem to suggest that my main estimates are upward-biased.

Research and Development (R&D) Tax Credits

Small firms can deduct up to 25% of expenditures on research and development (R&D) from corporate income tax burdens.² For example, if a small firm generates 1 million dollars in profits and faces a tax burden of \$180,000, and spends \$100,000 on R&D, then it can deduct \$25,000 from \$180,000, so the effective tax rate decreases from 18% to 15.5%. Therefore, the amount of reduction in corporate tax burdens depends on the firm's R&D expenditures. Large firms can also deduct up to 8%

²R&D expenditures are defined as costs associated with conducting research, such as hiring a team of researchers, and developing new scientific and technological methods to improve both quality and quantity of products.

of R&D expenditures from corporate tax burdens, so the effective tax rate would have decreased from 18% to 17.2% for large firms. If this additional corporate tax benefit provides investment incentives for small firms, then I would expect to see even a greater investment response for treated firms that are more R&D intensive (higher expenditures relative to profits). Therefore, I repeat the same analysis for main outcomes, just cutting the sample by firms' R&D expenditures.

Table 5 shows the main results in Section 1.5, where I interact the dummy for *Treated* $\times$ *Post* with the dummy for whether a firm is R&D intensive (which is defined as 1 if firms' expenditures on R&D are above the median and is fixed at the time of the reform). As shown in the first column of the table, the difference-in-differences coefficient is positive and statistically significant, implying that affected firms that are less R&D intensive increase investment following the reform. However, the triple difference coefficient is statistically insignificant and close to zero, suggesting that affected firms that are more R&D intensive did not increase investment more, even if there were additional tax benefits to do so. Therefore, the fact that there were additional benefits for R&D-heavy small firms does not seem to suggest that my main estimates are upward-biased.³

Other Benefits

There are other tax benefits associated with being small firms, such as financial and organizational support, and venture capitals and start-ups support. However, many of these benefits apply to only a specific set of small firms. For example,

³One may find it surprising that firms that were more R&D intensive did not increase investment more compared to less R&D intensive firms, given that there were additional corporate tax incentives to do so. In my sample, R&D intensive firms spend about 16% of their profits on R&D and make about 40 million dollars in profits on average in a given year, so their corporate tax rates decrease from about 20% to 16% (18.7% for large firms) by being re-classified as a small firm after the reform. However, R&D intensive firms may find the tax difference small or their R&D spending as partial substitutes for capital investment. Therefore, R&D intensive firms may not increase investment more compared to less R&D intensive firms after a capital gains tax cut, even if there were additional tax corporate tax benefits to do so.

venture capital and start-up firms are rare in the main analysis because firm entry rate is less than 1% from 2009 to 2017. Moreover, these additional benefits are unlikely to drive the main results, given that I find no significant differences in investment responses for firms more likely to get the main extra tax credits. Therefore, I conclude that extra benefits associated with small firms might be too specific to a certain set of firms to affect the main results.

Table 4: Main Results by Firms' Location

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payout	(4) Share Buyback
Treated x Post	0.483*** (0.135)	0.047*** (0.014)	0.001 (0.001)	0.001 (0.001)
Treated x Post x Rural	-0.193 (0.205)	-0.004 (0.025)	0.001 (0.002)	0.001 (0.002)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean (Rural=0)	14.435	0.032	0.005	0.001
Implied Elasticity wrt (1-tau) (Rural=0)	2.62	8.03	1.68	3.25
Pre-reform Treated Mean (Rural=1)	14.68	0.0421	0.00714	0.00192
Implied Elasticity wrt (1-tau) (Rural=1)	1.57	5.49	1.54	3.71
R-squared	0.63	0.29	0.72	0.28
Observations (firm-years)	4732	4503	4688	4715
Clusters (firms)	541	541	541	541

Notes: This table reports the tax effects on the main corporate outcomes based on the triple difference estimation. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 1.4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). The dummy for "Rural" is defined as 1 if firms are located in rural areas (defined in Section A.1.3). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 5: Main Results by Firms' R&D Expenditures

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payout	(4) Share Buyback
Treated x Post	0.429*** (0.140)	0.064*** (0.016)	0.002 (0.001)	0.000 (0.001)
Treated x Post x RD Intensive	-0.070 (0.199)	-0.054*** (0.020)	-0.000 (0.002)	0.001 (0.002)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean (RD=0)	14.285	0.039	0.004	0.001
Implied Elasticity wrt (1-tau) (RD=0)	2.33	8.99	2.24	3.58
Pre-reform Treated Mean (RD=1)	14.99	0.0289	0.00912	0.00274
Implied Elasticity wrt (1-tau) (RD=1)	1.95	1.93	0.93	3.02
R-squared	0.63	0.29	0.72	0.28
Observations (firm-years)	4732	4503	4688	4715
Clusters (firms)	541	541	541	541

Notes: This table reports the tax effects on the main corporate outcomes based on the triple difference estimation. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 1.4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). The dummy for "RD Intensive" is defined as 1 if firms' expenditures are above the median (As defined in Appendix A.1.3). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

A.2 Model Extensions

In this section, I extend the two-period investment model in Section 1.3 to incorporate (1) dividend payouts and (2) agency conflicts, separately. Then I discuss both theoretical and empirical implications of these extended models.

A.2.1 Extended Model with Dividend Payouts

In this section, I extend the simple model by allowing firms to payout dividends in the first-period. Suppose that the firm can also payout dividends, D , in the first period, which will be taxed at the effective dividend tax rate, τ_d . The neoclassical model in Section 1.3 can be extended in the following way:

$$Max V = \underbrace{\{(1 - \tau_g)R + (1 - \tau_d)D - E\}}_{\text{period 1 cash flow}} + \overbrace{\frac{(1 - \tau_g)[(1 - \tau_c)f(I) + C - R - D] + E}{1 + r}}^{\text{net-of-tax return to shareholders}}$$

Similar to the model predictions under the “New View”, cash-rich firms that pay $R > 0$ would set D such that $\tau_g = \tau_d$, so that lowering capital gains tax rates would decrease D , creating a partial substitution between dividends and share repurchases. Cash-constrained firms that raise new equity $E > 0$ set $R = 0$ and $D = 0$. Therefore, under the “Old View”, lowering capital gains tax rates will have the same results as in the model without the dividend payout option, while under the “New View” it would predict that dividend payouts will decrease.

A.2.2 Extended Model with Agency Conflicts

I extend the model in Section 1.3 by incorporating agency conflicts (Shleifer and Vishny 1986; Chetty and Saez 2010).⁴ The main source of departure from the model in Section 1.3 is that the firm's manager can also invest in pet projects, J , to maximize his private utility. In period 1, the firm's CEO chooses $\{I, J, R, E\}$ to maximize his utility such that $I + J + R = C + E$. In period 2, net-of-tax profits are distributed to shareholders. Therefore, the manager's problem is:

$$\begin{aligned}
 \text{Max } V^M &= \underbrace{\alpha_M}_{\text{CEO share}} \underbrace{\{(1 - \tau_g)R - E + \frac{(1 - \tau_g)[(1 - \tau_c)f(I) + C - R] + E}{1 + r}\}}_{\text{period 1 cash flow}} + \underbrace{\frac{g(J)}{(1 + r)}}_{\text{private returns}} \\
 &= \underbrace{\omega \left\{ R - \frac{E}{(1 - \tau_g)} + \frac{[(1 - \tau_c)f(I) + C - R] + \frac{E}{(1 - \tau_g)}}{1 + r} \right\}}_{\text{firm's profit-maximization side}} + \underbrace{\frac{g(J)}{(1 + r)}}_{\text{CEO's private utility}}
 \end{aligned}$$

where $\omega = \alpha_M(1 - \tau_g)$ is the relative weight manager puts on firm's profit side.

I assume that (1) $f(I)$ & $g(J)$ strictly concave, (2) firms can raise additional funds only through issuing new equity, (3) firms do not pay dividends, and (4) there is no other corporate governance mechanism to change the manager's incentives towards firm's profits side.⁵

The source of agency problems in this setting is a divergence of objectives of the manager and shareholders. A self-interested manager can invest in "pet projects"

⁴I acknowledge that I directly borrow the model set-up and theoretical framework from Chetty and Saez (2010) in order to highlight the intuition and draw comparative statics suitable for my paper.

⁵Examples of the corporate governance mechanisms in corporate finance literature include stronger or independent board structure, higher level of monitoring or hiring a member from the founding families to become the manager. One can extend the model by explicitly putting monitoring costs or other measures of corporate governance on the weight, as in Chetty and Saez (2010). In this way, strengthening corporate governance would have similar effects as giving more shares to managers, since higher levels of monitoring or governance would make it more costly for CEOs to deviate from profit-maximization. Empirically testing this channel is beyond the scope of this paper, and would be an interesting extension.

that yield no profits to shareholders but generates utility only to himself. Therefore, the manager can also use C to invest in J that gives private benefits of $g(J)$. Assume that $g'(0) > \omega f'(C)$, which ensures an interior optimum in investment response. Then I and R are determined by the following first-order conditions:

$$(1 - \tau_c)\omega f'(I) = g'(C - I - R)$$

$$\omega r \leq g'(C - I - R)$$

Let $I(\omega)$ and $R(\omega)$ denote the investment and share repurchase choices of the CEO as a function of the weight. To characterize the properties of these functions, define the cutoff $\bar{\omega} = \frac{g'(C - I^*)}{r} \geq 0$, where I^* denotes the optimal investment level from the shareholders' perspective. Note that this cutoff is monotonically decreasing in C . Then $R(\omega)$ and $I(\omega)$ follow the following threshold rules: (1) If $\omega \leq \bar{\omega}$ then $R(\omega) = 0$ and $I(\omega)$ is chosen such that $(1 - \tau_g)\omega f'(I) = g'(C - I)$ and (2) If $\omega > \bar{\omega}$ then $R(\omega) > 0$ and $I(\omega) = I^*$ is chosen such that $\omega r = g'(C - I^* - R)$.⁶ Intuitively, it means that depending on the weight that the CEO puts on the firm's profit maximization, the firm's initial level of share repurchases and investment would be set differently. This also implies that when the weight changes, either through a higher ownership or decrease in the tax rate, then the share repurchase and investment responses would be different, depending on the initial weight.

Given this setting, we can make the following predictions on how share repurchases and investment would change as the weight changes. If the CEO has a weak incentive towards the firm's profit maximization, then the CEO retains as much cash as possible for pet projects and do not buy back shares. By contrast, for CEOs with $\omega > \bar{\omega}$, any increase in the weight leads to increases in share repurchases and

⁶See Chetty and Saez (2010) for detailed proofs behind this.

decrease in pet projects on the intensive margin: for $\omega > \bar{\omega}$

$$R'(\omega) = -\frac{r}{g''(J(\omega))} > 0 \quad \& \quad I'(\omega) = 0$$

On the other hand, when $\omega \leq \bar{\omega}$, the CEO doesn't buy back shares, and splits cash between I and J , where he chooses I to equate his private marginal returns on investment in the two projects. Therefore, any increase in ω increases I and reduces J : for $\omega \leq \bar{\omega}$

$$I'(\omega) = -\frac{(1 - \tau_g)f'(I(\omega))}{(1 - \tau_g)\omega f''(I(\omega)) + g''(C - I(\omega))} > 0 \quad \& \quad R'(\omega) = 0$$

Intuitively, if the CEO has $\omega > \bar{\omega}$, then he has enough cash to do share repurchases, and sets $I = I^*$. Any increase in ω , say by a lower tax rate, increases the marginal return on the profitable investment, as much as it increases the opportunity cost of investment, which equals the amount of cash he can spend on share repurchases to pay himself. Therefore, an empirical prediction based on this theoretical framework is that following a tax cut, firms whose CEOs own a lower fraction of firms' stock would increase investment more relative to firms whose CEOs own a larger share.

A.2.3 Model Prediction on the Magnitude

In this subsection, I use the model described in Section 1.3 to predict the magnitude of the investment response to lowering capital gains tax rates under a set of assumptions. I start with the optimality condition in equation (1.5):

$$(1 - \tau_g)(1 - \tau_c)f'(I^*) = r \quad (B.3)$$

First, I assume that $f(I) = AI^\alpha$, where $0 < \alpha < 1$. Note that based on the first order conditions,

$$I^* = [(1 - \tau_g)(1 - \tau_c)A\frac{\alpha}{r}]^{\frac{1}{1-\alpha}}$$

Implicitly differentiating equation (B.3) with respect to $(1 - \tau_g)$ gives the following:

$$\frac{\partial I^*/I^*}{\partial(1 - \tau_g)} = \frac{1}{(1 - \alpha)(1 - \tau_g)}$$

Therefore, multiplying both sides by $(1 - \tau_g)$ yields the investment elasticity with respect to the net of tax rate:

$$\frac{\partial I^*/I^*}{\partial(1 - \tau_g)/(1 - \tau_g)} = \frac{1}{(1 - \alpha)}$$

In other words, the magnitude of the investment elasticity with respect to $(1 - \tau_g)$ depends on the value of α , namely the output elasticity with respect to investment. Assuming that $\alpha \in (0.5, 0.7)$, the predicted investment elasticity with respect to one minus the tax rate falls within $(2.0, 3.3)$, and the difference-in-differences estimate of 2.77 among the sample of the treated listed firms falls within this range.

Three caveats apply to this type of exercise. First, the theoretical prediction strongly relies on the model assumptions. Second, the production function, regardless of its specific technology, is usually a function of both capital and labor,

and not as a function of investment. Third, the magnitude is sensitive to the parameter, so it is important to remember that this model prediction on the magnitude is giving us some bounds on the investment elasticity.

A.2.4 Alternative Model Prediction on the Magnitude

In this subsection, I describe a static investment model to predict the magnitude of the capital stock elasticity with respect to the net of tax rate.

Setup I start with a standard production function framework:

1. Output is given by $y = f(L, K) = AL^{\alpha_L}K^{\alpha_K}$, where $0 < \alpha_L + \alpha_K < 1$.
2. Wage rate, w , for the cost of labor is exogenously given.
3. The cost of capital is $\frac{r}{(1-\tau_g)(1-\tau_c)}$, where r, τ_g, τ_c are the expected rate of return, capital gains tax rate, and corporate tax rate, as described in equation (1.12) in Section 1.6, with $\rho = 0$.

The firm optimally chooses L and K to minimize the cost:

$$\min_{L, K} wL + \frac{r}{(1-\tau_g)(1-\tau_c)}K \quad \text{s.t.} \quad y = AL^{\alpha_L}K^{\alpha_K}$$

The Lagrangian is equal to:

$$\mathcal{L} = wL + \frac{r}{(1-\tau_g)(1-\tau_c)}K + \lambda(y - AL^{\alpha_L}K^{\alpha_K})$$

The first order conditions are given by:

$$y = AL^{\alpha_L}K^{\alpha_K} \tag{A.1}$$

$$w = \alpha_L \frac{y}{L} \tag{A.2}$$

$$r = \alpha_K(1-\tau_g)(1-\tau_c) \frac{y}{K} \tag{A.3}$$

We derive the cost function $C(w, r, \tau, y)$ and marginal cost function $MC(w, r, \tau, y)$:

$$C(y; w, r, \tau_g, \tau_c) = (\alpha_L + \alpha_K) \left[\frac{y}{A} \left(\frac{w}{\alpha_L} \right)^{\alpha_L} \left(\frac{r}{\alpha_K(1 - \tau_g)(1 - \tau_c)} \right)^{\alpha_K} \right]^{\frac{1}{\alpha_L + \alpha_K}} \quad (\text{A.4})$$

$$MC(y; w, r, \tau_g, \tau_c) = \left[\frac{y^{1 - \alpha_L - \alpha_K}}{A} \left(\frac{w}{\alpha_L} \right)^{\alpha_L} \left(\frac{r}{\alpha_K(1 - \tau_g)(1 - \tau_c)} \right)^{\alpha_K} \right]^{\frac{1}{\alpha_L + \alpha_K}} \quad (\text{A.5})$$

Equilibrium Suppose we have a downward sloping (inverse) product demand $p = Dy^{\frac{1}{\epsilon}}$, where ϵ is the product demand elasticity. Then total revenue is $TR(y; \epsilon) = Dy^{\frac{1}{\epsilon} + 1}$ and marginal revenue $MR(y; \epsilon) = \left(\frac{1}{\epsilon} + 1\right) Dy^{\frac{1}{\epsilon}}$. A profit-maximizing firm sets $MR(y; \epsilon) = MC(y; w, r, \tau_g, \tau_c)$:

$$y = \left[\left(\frac{1}{\epsilon} + 1 \right)^{\alpha_L + \alpha_K} AD^{\alpha_L + \alpha_K} \left(\frac{\alpha_L}{w} \right)^{\alpha_L} \left(\frac{\alpha_K(1 - \tau_g)(1 - \tau_c)}{r} \right)^{\alpha_K} \right]^{\frac{1}{1 - (\alpha_L + \alpha_K)\left(\frac{1}{\epsilon} + 1\right)}} \quad (\text{A.6})$$

$$K = \left[\left(\frac{1}{\epsilon} + 1 \right) A^{(1 + \frac{1}{\epsilon})} D \left(\frac{\alpha_L}{w} \right)^{\alpha_L(1 + \frac{1}{\epsilon})} \left(\frac{\alpha_K(1 - \tau_g)(1 - \tau_c)}{r} \right)^{(1 - \alpha_L(1 + \frac{1}{\epsilon}))} \right]^{\frac{1}{1 - (\alpha_L + \alpha_K)\left(\frac{1}{\epsilon} + 1\right)}} \quad (\text{A.7})$$

Elasticity The change in the total capital stock for a small change in $(1 - \tau_g)$ is given by:

$$\frac{\partial K^*}{\partial(1 - \tau_g)} = \left(\frac{1 - \alpha_L(1 + \frac{1}{\epsilon})}{1 - (\alpha_L + \alpha_K)\left(1 + \frac{1}{\epsilon}\right)} \right) \frac{K^*}{(1 - \tau)} \quad (\text{A.8})$$

We can also write the elasticity of capital stock with respect to $(1 - \tau_g)$ as:

$$\frac{\partial K^*/K^*}{\partial(1 - \tau_g)/(1 - \tau_g)} = \frac{1 - \alpha_L(1 + \frac{1}{\epsilon})}{1 - (\alpha_L + \alpha_K)\left(1 + \frac{1}{\epsilon}\right)} \quad (\text{A.9})$$

A large decrease in τ_g from τ_0 to τ^* (or increase in the keep rate from $1 - \tau_0$ to $1 - \tau^*$) increases capital from K_0 to K^* as follows:

$$dK = \left[\left(\frac{1}{\eta} + 1 \right) A^{\left(\frac{1}{\eta} + 1\right)} D \left(\frac{\alpha_L}{w} \right)^{\alpha_L \left(1 + \frac{1}{\eta}\right)} \left(\frac{\alpha_K}{r} \right)^{\left(1 - \alpha_L \left(1 + \frac{1}{\eta}\right)\right)} \right]^{\frac{1}{1 - (\alpha_L + \alpha_K) \left(\frac{1}{\eta} + 1\right)}} \left((1 - \tau^*)^{\frac{\left(1 - \alpha_L \left(1 + \frac{1}{\eta}\right)\right)}{1 - (\alpha_L + \alpha_K) \left(\frac{1}{\eta} + 1\right)}} - (1 - \tau_0)^{\frac{\left(1 - \alpha_L \left(1 + \frac{1}{\eta}\right)\right)}{1 - (\alpha_L + \alpha_K) \left(\frac{1}{\eta} + 1\right)}} \right) \quad (\text{A.10})$$

$$\frac{dK/K_0}{d(1 - \tau_g)/(1 - \tau_g)} = \left[\left(\frac{1 - \tau^*}{1 - \tau_0} \right)^{\frac{1 - \alpha_L \left(1 + \frac{1}{\epsilon}\right)}{1 - (\alpha_L + \alpha_K) \left(1 + \frac{1}{\epsilon}\right)}} - 1 \right] * \frac{1 - \tau_0}{\tau_0 - \tau^*} \quad (\text{A.11})$$

Assuming that $\alpha_K \in (0.05, 0.25)$, $\alpha_L \in (0.45, 0.65)$, and $\epsilon \in (-10, -1)$, the predicted elasticity falls within $(1.0, 2.45)$. Note that the lower bound for this elasticity is 1 (when $\epsilon = -1$, with $\epsilon < -1$ for elastic normal goods). Based on the sample of treated listed firms, the difference-in-differences estimate of the capital stock elasticity with respect to the net of tax rate is 0.56, which falls below the model prediction.

A.2.5 Extended Measure for User Cost of Capital

In this subsection, I consider an extended measure for the user cost of capital closely following Barro and Furman (2018).⁷ The user cost measure is derived from a neoclassical framework, as used in Hall and Jorgenson (1967) and King and Fullerton (1984). A profit-maximizing firm faces the following steady-state user cost of capital:

$$MPK = C_K = \frac{(1 - \tau_c \lambda)}{(1 - \tau_c)(1 - \tau_g)}(r + \delta) \quad (\text{A.12})$$

where MPK is the marginal product of capital, C_K is the user cost of capital, τ_c is the corporate income tax rate, τ_g is the capital gains tax rate, λ is the expensing rate, r is the expected return of capital, and δ is the depreciation rate. Note that if the tax system allows only for depreciation deductions based on δ , then the expensing rate would be $\lambda = \frac{\delta}{r + \delta}$.

Assuming that $\tau_c = 0.22$, $r = 0.082$ (Barro and Furman, 2018), $\delta = 0.1$, and $\lambda = 0.55$, a decrease in capital gains tax rate from 0.24 to 0.1 would reduce the cost of capital from 0.269 to 0.228, which is roughly a 15 percent reduction.⁸ Panels A of Figure A.5 shows the sensitivity of the changes in the cost of capital across different depreciation rates (from 0 to 0.2), for a given required return of 8.2 percent. As shown in the figure, the change in the user cost is stable around 15 percent across different depreciation rates. Panel B of Figure A.5 shows the same exercise, for a given required return of 3 percent. The change in the user cost is stable around 15 percent across different depreciation rates.

⁷An alternative framework to estimate the user cost of capital is explained and implemented in Auerbach and Hassett (1992) and Cohen, Hansen and Hassett (2002). This alternative approach estimates a firm-specific cost of capital in a given year, which accounts for asset-specific depreciation rates.

⁸In this framework, I assume that the after-tax expected return r is not affected by changes in capital gains tax rates. In practice, the after-tax required return, which accounts for the risk-free rate and the risk premium, may change after a tax cut.

As in Barro and Furman (2018), I also incorporate debt financing in the measure for the user cost. I extend the measure in (A.12) as follows:

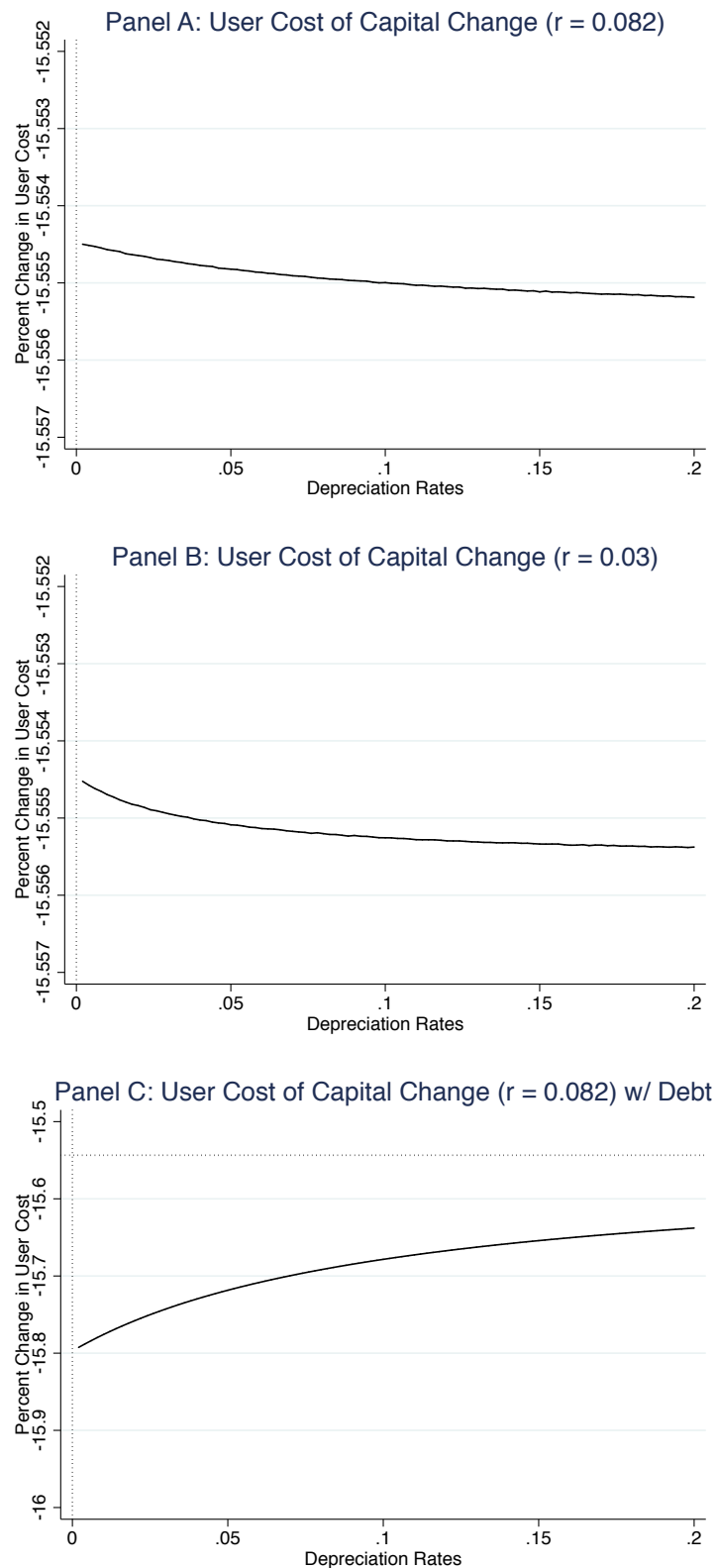
$$MPK = C_K = \frac{1 - \tau_c \lambda}{(1 - \tau_c)(1 - \tau_g)}(r + \delta) - \frac{\theta - 1}{\theta} \left(\frac{\tau_c}{1 - \tau_c} \right) \Delta i$$

where θ is the elasticity of default-associated costs with respect to the debt-asset ratio, Δ is the debt-asset ratio, and i is the nominal interest rate on corporate bonds.⁹

Assuming that $\theta = 2$ (Barro and Furman, 2018), $\Delta = 0.5$, $i = 0.03$, $r = 0.082$, and $\delta = 0.1$, a decrease in capital gains tax rate from 0.24 to 0.1 would reduce the cost of capital by roughly 15 percent. Panel C of Figure A.5 shows the change in the user cost across different depreciation rates, which is stable around 15 percent. Therefore, based on this framework, accounting for the debt-financing does not seem to significantly affect the estimated change in the user cost.

⁹Details on the derivation of the measure, along with how the parameters are chosen, are explained in Barro and Furman (2018).

Figure A.5: Sensitivity Analysis for the Changes in User Cost Across Different Parameters



Notes: These figures show the sensitivities for the changes in the user cost across different depreciation rates, expected rates of returns, and accounting for the debt-financing.

A.3 Additional Heterogeneity Results

In this section, I show additional heterogeneity analysis results based on firms' cash constraints and ownership structure.

A.3.1 Additional Results by Firms' Cash-Constraints

In Section 1.5, I use retained earnings scaled by assets (averaged over three past years at the time of the reform) as a proxy for firms' cash constraint, and define that firms are cash-constrained if their measure is below the median. There are other popular measures of financial constraints, such as firm age, and well-known indices, such as Kaplan-Zingales, Whited-Wu, and Hadlock-Pierce. I repeat the same heterogeneity analysis using each of these different measures of financial constraints.

I define Firm Age as the number of years since the firm has been established, and define that the firm is cash-rich if its age is above the median. I run the triple difference model as in Section 1.5, substituting the dummy variable for Cash-Rich with this new indicator. Column (1) in Table C.1 shows the results based on this triple difference estimation. The difference-in-differences coefficient for investment is positive and significant, implying that financially-constrained firms increase investment after the reform. Moreover, the triple-difference coefficient is negative and statistically significant, suggesting that less financially-constrained firms increase investment less.

I construct Kaplan-Zingales (KZ) Index following Lamont, Polk and Saa-Requejo (2001).¹⁰ I sort firms into median based on their index values at the time of the reform, and define that the firm is cash-rich if its index is below median. I run

¹⁰KZ index is constructed as $-1.001909[(ib + dp)/laggedppent] + 0.2826389[(at + prcc_f \times csho - ceq - txdb)/at] + 3.139193[(dltt + dlc)/(dltt + dlc + seq)] - 39.3678[(dvc + dvp)/laggedppent] - 1.314759[che/laggedppent]$. The variables in italics are Compustat-equivalent data items.

the triple difference model as in Section 1.5, substituting the dummy variable for Cash-Rich with this new indicator. Column (2) in Table 6 shows the results based on this triple difference estimation. The difference-in-differences coefficient for investment is positive and significant, implying that financially-constrained firms increase investment after the reform. Moreover, the triple-difference coefficient is negative, but statistically insignificant, suggesting that less cash-constrained firms increase investment less, although the difference is insignificant.

I construct Whited-Wu (WW) Index following Whited and Wu (2006) and Hennessey and Whited (2007).¹¹ I sort firms into median based on their index values at the time of the reform, and define that the firm is cash-rich if its index is below median. I run the triple difference model as in Section 1.5, substituting the dummy variable for Cash-Rich with this new indicator. Column (3) in Table 6 shows the results based on this triple difference estimation. The difference-in-differences coefficient for investment is positive and significant, implying that financially-constrained firms increase investment after the reform. Moreover, the triple-difference coefficient is negative and statistically significant, suggesting that less financially-constrained firms increase investment less.

I construct Hadlock-Pierce (HP) Index following Hadlock and Pierce (2010).¹² I sort firms into median based on their index values at the time of the reform, and define that the firm is cash-rich if its index is below median. I run the triple difference model as in Section 1.5, substituting the dummy variable for Cash-Rich with this new indicator. Column (4) in Table 6 shows the results based on this triple difference estimation. The difference-in-differences coefficient for

¹¹WW index is constructed as $-0.091[(ib + dp)/at] - 0.062[1(dvc + dvp > 0)] + 0.021[dltt/at] - 0.044[\log(at)] + 0.102[ARG] - 0.035[RG]$. The variables in italics are Compustat-equivalent data items, ARG is average industry sales growth, estimated separately for each SIC industry and each year, and RG is the sales growth (annual percentage increase in sales).

¹²HP index is constructed as $-0.737Size + 0.043Size^2 - 0.040Age$. Size is the log of inflation-adjusted total asset (Compustat-equivalent *at*, Age is the number of years the firm is listed, and Size and Age are capped at \$4.5 billion and at 37 years, respectively.

investment is positive and significant, implying that financially-constrained firms increase investment after the reform. Moreover, the triple-difference coefficient is negative, but statistically insignificant, suggesting that less cash-constrained firms increase investment less, although the difference is insignificant.

In summary, additional tests based on different measures of financial constraints, such as firm age or popular indices, show results that are qualitatively similar to the main results using retained earnings as a proxy for firms' cash-constraints.

Table 6: Main Results by Financial Constraints

	log(Investment) by different measures of cash constraints			
	(1) Age	(2) Kaplan-Zingales	(3) Whited-Wu	(4) Hadlock-Pierce
Treated x Post	0.622*** (0.147)	0.367** (0.182)	0.648*** (0.150)	0.554** (0.222)
Treated x Post x Cash-Rich	-0.500** (0.194)	-0.204 (0.271)	-0.411** (0.208)	-0.291 (0.293)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean (CR=0)	14.523	14.733	14.363	14.129
Implied Elasticity wrt (1-tau) (CR=0)	3.38	1.99	3.52	3.01
Pre-reform Treated Mean (CR=1)	14.79	14.95	15.05	14.92
Implied Elasticity wrt (1-tau) (CR=1)	0.67	0.88	1.29	1.43
R-squared	0.63	0.61	0.63	0.60
Observations (firm-years)	4732	3983	4732	4183
Clusters (firms)	541	459	541	478

Notes: This table reports the tax effects on the main corporate outcomes based on the triple difference estimation. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). The dummy for "Cash-Rich" is defined as 1 if the firm's financial constraint index is below median (less financially constrained). Investment is defined as log of expenditures on physical capital assets. The main outcomes are winsorized at 99% level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. All specifications include time and firm FEs. The sample is restricted to publicly listed companies. The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

A.3.2 Ownership Structure

I show additional heterogeneity results based on firms' ownership structure. Since investors pay capital gains taxes when they sell their stocks or firms buyback shares, long-term investors may care less about capital gains taxes compared to short-term investors (Poterba 1987; Auerbach and Siegel 2002). On average, investors care about capital gains taxes due to unanticipated shocks to their liquidity in each period. However, long-term investors will be less responsive to any transitory liquidity shocks, so firms composed with more long-term investors may not react to a reduction in capital gains taxes as much as firms with a larger share of short-term investors.

I define Long-term Investors as the investors whose ownership is at least 5% of the firms' stock (also defined as "large-ownership investors" in Korea), and define that the firm has a higher share of long-term investors if its share is above the median. I run the triple difference model, where I interact the dummy $Treated \times Post$ with the indicator for the firm's share of long-term investors. Table 7 shows the results based on this triple difference estimation. The difference-in-differences coefficient for investment is positive and statistically significant, implying that firms whose share of long-term investors is below the median increased investment after the reform. Moreover, the triple-difference coefficient is negative and statistically significant, suggesting that firms with above-median share of long-term investors increased investment less.

Table 7: Main Results by Ownership Structure (Publicly Listed Firms)

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.777*** (0.178)	0.022 (0.014)	0.002 (0.002)	-0.000 (0.001)
Treated x Post x Long-term Investors	-0.522** (0.225)	-0.027 (0.018)	-0.001 (0.002)	0.001 (0.002)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean (Long=0)	14.845	0.036	0.004	0.002
Implied Elasticity wrt (1-tau) (Long=0)	4.22	3.41	2.35	-0.37
Pre-reform Treated Mean (Long=1)	14.98	0.0275	0.00951	0.00191
Implied Elasticity wrt (1-tau) (Long=1)	1.38	-0.84	0.31	2.79
R-squared	0.65	0.39	0.79	0.34
Observations (firm-years)	4208	4020	4164	4195
Clusters (firms)	477	477	477	477

Notes: This table reports the tax effects on the main corporate outcomes based on the triple difference estimation. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 1.4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). The dummy for "Long-term Investors" is defined as 1 if the firm's share of long-term investors (those with at least 5% ownership) is above the median. Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

A.4 Robustness Checks

In Appendix A.4, I provide a set of robustness tests for the main results in Section 1.5.

A.4.1 Without Controls

I repeat the main analysis in equation (1.8), without basic or additional controls and with only basic controls. Table 8 shows the main results without any basic or additional controls, and Table 9 shows the main results with only basic controls. The coefficient estimates are smaller when I do not include any controls or when I include only basic controls, but the results are qualitatively similar to the ones from the main specification in equation (1.8).

A.4.2 With Different Levels of Winsorizing

I repeat the main analysis using the same specification as in equation (1.8), winsorizing (top-coding) the main outcome variables at the 95% level, instead of at the 99% level. Table 10 shows the main results based on this different level of winsorizing. The coefficient estimate for $\log(\text{investment})$ is nearly identical to the one from winsorizing at the 99% level. The coefficient estimate for equity issuance is smaller, implying that equity issuance variable is more right-skewed than investment. However, the estimates on this table suggest that the main results are robust to and qualitatively similar when using different levels of winsorizing.

A.4.3 Using Balanced Panel

I repeat the main analysis using the same specification as in equation (1.8), using a balanced panel. Table 11 shows the main results based on using the balanced panel.

The coefficient estimate for $\log(\text{investment})$ is larger in the balanced sample, but the main results are qualitatively similar whether the sample is balanced or not.

A.4.4 Including Firms in Other Sectors

I repeat the main analysis using the same specification as in equation (1.8), including firms in other sectors. Table 12 shows the main results based on using firms in other sectors in addition to the firms in the main analysis sample. The coefficient estimate for $\log(\text{investment})$ remains similar when I include firms in other sectors.

A.4.5 Different Measures of Investment

For additional measures of investment, I scale expenditures on physical capital assets by lagged tangible assets and lagged total assets. Moreover, since firms may disinvest (i.e. sell off their machines or equipments), I use net expenditures on physical capital assets scaled by lagged tangible assets to account for potential disinvestment. Finally, I define investment rate as annual changes in tangible assets scaled by lagged tangible assets, and use it as an alternative measure of investment. Table 13 shows the main results based on these different scaling and measures of investment. The coefficient estimates are smaller when we use a larger denominator (i.e. total assets), but the investment elasticity is reasonably comparable to the main estimate, in which I use log of expenditures on physical capital assets. Therefore, the results are robust to different measures of investment.

A.4.6 Dollar-weighting by Revenues

To make each observation in my sample contribute to the main estimates according to its economic scale, I weight each observation by its one-year lagged revenue. In this sense, my estimates are "dollar-weighted", so that firms with higher revenues

(bigger firms) will carry higher weights in their investment estimates. However, the key issue with weighting by revenues (as done in Yagan (2015)) in my setting is that revenues partially determined whether firms were treated by the reform, so weighting observations by revenues might potentially bias my estimates. For example, firms with very high revenues are mostly used as control firms, so by over-weighting observations of the control firms and by under-weighting observations of treated firms, my estimates would be likely downward biased. Table 14 shows the main results based on weighting by lagged revenues. The coefficient estimates are smaller than the ones from without weighting, but both estimates are quite comparable. Therefore, the results are robust to dollar-weighting.

A.4.7 DFL Re-weighting

As an additional robustness check, I use the method of DiNardo, Fortin and Lemieux (1996) to flexibly control for any time-varying firm-level productivity shocks. DFL-reweighting procedure is less parametric, but similar to matching algorithm; reweighting is useful when comparing outcomes across firms that differ along observable characteristics, such as size or productivity, given that control firms are relatively bigger than treated firms by definition. One may reweight the sample to hold the distribution of observable traits fixed across groups. To do so, one first divides all observations into equal-sized bins, q , according to the traits. Then to make the within-group distribution of weights across bins equal to the original cross-bin distribution of weights in some base group, one inflates or delates weights in every group-bin. For example, if the 2014 treated group had relatively more productive firms than the 2014 control group firms, then the DFL method will down-weight more productive firms and up-weight less-productive firms in the 2014 treated group, so that the distribution of observable traits is fixed across two groups.

Since I compare outcomes across treatment groups and over time, I DFL-reweight across 18 ($= 2 \text{ groups} \times 9 \text{ years } 2009\text{-}2017$) groups g . I define the base group g_b to be the pre-reform (2014) treated group. Then I divided all observations into five equal-sized bins (quintiles) q according to their operating profits to control for any underlying productivity differences. Therefore, I use each observation's operating profits (revenues minus operating costs) to bin it into one of quintiles, q , where the bins are defined using the 2014 treated group. Note that this procedure is comparable to controlling for dummies for the pre-reform (2014) operating profits interacted with dummies for each year, as used in the main analysis. Table 15 reports the main estimates with DFL-reweighting. The coefficients are smaller with DFL-reweighting, but still quite comparable to the main estimates. Therefore, my main results are robust to different ways of controlling for the observable characteristics.

A.4.8 Accounting for Potential Selection of Treated Firms

One potential concern that can overstate this paper's main findings is that treated firms that experienced a tax reduction might be "selected" firms that would have increased investment regardless of the tax cut. The logic behind this concern is that firms that passed the old thresholds only did so because they saw investment opportunities before the reform in 2014, with the expected benefits outweighing the costs of crossing the thresholds. If most of the treated firms passed the threshold just before the reform (i.e. in 2013), then they would have increased investment independent of the reform, so their investment response might be driven by the tax cut and their growth potential. To address this potential concern, I restrict the treated firms to be those that passed the threshold by 2010 (about 60% of the treated group), so that their investment response after 2014 would be less likely driven by their investment opportunities in 2010. Table 16 shows the main results based on

this subset of treated firms. The coefficient estimates are almost identical to the main estimates. Therefore, my results are robust to the potential selection issue.

A.4.9 Firms that Bunched at Old Cutoffs

I repeat the main analysis on investment using firms that were bunching at either of the old cutoffs prior to the reform. I use a sample of both publicly listed and private firms for efficiency. Since firms were bunching precisely to avoid higher capital gains tax rates, removing the old cutoffs may increase their incentive to invest. Table 17 shows results just using firms that were bunching as treated and unaffected firms as control. Columns (1) shows that their investment response is slightly higher than the average investment response from the treated firms with a tax cut. Furthermore, the tax effects on investment are stronger for firms that bunched at the revenue cutoff than those that bunched at the labor cutoff, suggesting that firms right below the labor cutoff might have been over-investing in capital as a substitute for labor.

A.4.10 Forward-looking Firms (Partially Treated by the Reform)

I repeat the main analysis on investment using firms that were growing below the bunching firms. Even though these forward-looking firms did not experience a tax cut or bunch to avoid higher taxes, their investment response might be still impacted by the reform in a dynamic sense. For example, suppose a small firm with 150 average employees and 50 million in total revenues decided to start new long-term (5-year) projects in 2012. Prior to the reform, the manager had to think about how crossing the thresholds would affect their cost of capital. However, since the old cutoffs became no longer binding, this firm may grow even more or

faster after the reform in 2014. Therefore, this type of forward-looking firms could be partially and indirectly treated by the reform.

To use these forward-looking firms as additional treated groups, I first define that firms are partially treated if they are below the bunching points (285 in average employees and 90 million in total revenues). Then I incrementally reduce the size of the treated group either in the labor dimension (in groups of five employees) or in the revenue dimension (in groups of five million dollars). For example, along the labor dimension, the first group will contain firms above labor size of 0 and below the revenue size of \$90 million. The second group will contain firms above the labor size of 5 (so between 5 and 280) and below the same revenue size. The final group will contain firms between the labor size of 275 and 280 and below the same revenue size. I repeat the same exercise along the revenue dimension in groups of \$5 million. Since firms far away from the bunching points will be much less likely to be impacted by the reform, one interesting prediction is that as we include firms only closer to the bunching firms, the investment response should be greater.

Panel A in Figure A.7 shows these results along the labor dimension. As shown in the figure, the difference-in-differences estimates get larger as we reduce the size of the treated group to include firms closer to the labor bunching point. Panel B in Figure A.7 shows these results along the revenue dimension. Once again, the difference-in-differences estimates get larger as we reduce the size of the treated group to firms closer to the revenue bunching point. These results suggest that firms below the bunching points may be indirectly impacted by the reform. Because of this reason, it is not reasonable to use these firms as control or as part of the main treated group.

As an alternative way to use the forward-looking firms, I use the firms below the bunching thresholds as an additional control group and estimate a separate

difference-in-differences model by comparing the main treated firms with a tax cut with the firms above the new threshold and this additional control group. Table 18 shows the estimates based on this specification: the difference-in-differences coefficient for the tax effects on investment is attenuated, consistent with the story that the additional control group is partially treated by the reform and increased investment.

A.4.11 Placebo Test using Earlier Period

Since other time-varying shocks, such as different policy reforms, may coincide or occur close to the reform in 2014, I conduct a placebo test using an earlier time period. For example, there was a corporate tax reform at the end of 2011, which moderately changed the tax schedule for firms making under \$200,000. Even though this change was small enough so that it would not necessarily affect the overall results, I still use year 2012 as the placebo date and set *Post* equal to 1 if it is after 2012. Table 19 shows the results based on the placebo test. The coefficients are not statistically different from zero even at the 90% confidence level, suggesting that there were no other changes that could have affected the main results around this period of time.

A.4.12 Placebo Test using Random Cutoff

To ensure that the mains results on investment are driven by the policy variation, which generated a set of firms whose investment incentives have changed due to changes in regulations on firm size, I conduct a placebo test using a random cutoff to define the treated group. For example, I set arbitrarily much higher value of \$450 million (instead of \$150 million) for the new revenue threshold, so that many of the unaffected firms are defined now as treated firms. The placebo

tests are qualitatively similar if I use other random cutoffs (much lower or higher threshold). Table 20 shows the results based on the placebo test. The coefficients are not statistically different from zero even at the 90% confidence level, suggesting that the policy variation based on the reform in 2014 was the main driver of the investment responses for the affected firms.

A.4.13 Dropping Firms Above the Old Cutoffs & Around the New Cutoff

Including treated firms right above the old cutoffs may attenuate the main estimates of the tax effects on investment if these firms had high adjustment costs and would not respond to the tax cut within the first three years. By contrast, including these firms may overstate the results if these firms were growing and would increase investment regardless of the tax cut. To account for this potential bias, I incrementally drop firms above both the old cutoffs in paired-bins of 5 employees and 5 million dollars in revenues. Figure A.8 shows the main results, where the first dot indicates the main estimate where I drop firms between 300 and 305 employees and between 100 million and 105 million dollars in revenues. The dashed horizontal line indicates the main estimate without dropping any of these firms. As we see, the estimates across different paired bins are close to the original estimate and thus, my results are robust to accounting for these potential sources of bias.

Similarly, including treated firms right below the new threshold may attenuate the main estimates of the tax effects on investment if these firms had incentives to decrease investment and suppress sales to stay below the new cutoff. By contrast, including the control firms right above the new threshold may overstate the results if these firms also had incentives to decrease investment to go below the new cutoff. To account for this potential bias, I incrementally drop firms around the new threshold in the range between one to ten percent. Figure A.9 shows the main

results, where the first dot indicates the main estimate where I drop firms 1 percent around the new threshold. The dashed horizontal line indicates the main estimate from dropping firms one percent around the new cutoff. As we see, the estimates across different bins are close to the original estimate and thus, my results are robust to accounting for these potential sources of bias.

A.4.14 Re-scaling Estimates Using an Instrumental Variable

As discussed in Section 1.4, the dummy, $Treated_i$, is fixed at the reform date, which could attenuate my estimates. About 6% of the treated firms crossed the new threshold by 2017, while about 5% of the control firms went below the new cutoff by 2017. To address this potential bias, I re-scale my estimates using the dummy for $Treated_{it}$ in 2015 as an instrumental variable for dummies for $Treated_{it}$ in 2016 and 2017. The intuition is that even though $Treated_{it}$ for $t \in (2016, 2017)$ may be endogenous, $Treated_{i,2015}$ is exogenously determined by the reform and highly correlated with treatment dummies in latter years. Table 21 shows the results based on this IV approach, where the coefficient estimates are indeed slightly larger than the unscaled estimates from holding $Treated_i$ fixed.

A.4.15 Measuring Cash-constraints Using Different Cuts (Tercile)

In Section 1.5, I use retained earnings scaled by assets (averaged over three past years at the time of the reform) as a proxy for firms' cash constraint, and define that firms are cash constrained if their measure is below the median. In this subsection, I use a different cut (tercile instead of the median) to see if the investment result is robust to using a different way of cutting the sample. Table 22 shows the results based on defining the cash-constrained firms as the ones below the thirty

third percentile of average retained earnings in 2014. I find that the results are qualitatively similar to the one where I cut the sample by the median.

A.4.16 Measuring CEO Incentives Using Different Cuts (Tercile)

In Section 1.5, I use CEOs' stock shares at the time of the reform as a proxy for CEOs' incentives, and define that their incentives were more aligned with firms' profit maximization if CEOs' share was above the median. In this subsection, I use a different cut (tercile instead of the median) to see if the investment result is robust to using a different way of cutting the sample. Table 23 shows the results based on defining the low-ownership CEOs as the ones below the thirty third percentile of CEOs' stock share in 2014. I find that the results are qualitatively similar to the one where I cut the sample by the median.

A.4.17 Competition Effects

One valid concern is that treated firms with the tax cut could be replacing capital investment from the control firms that were competing within the same industries after the reform, which could lead to an upward bias in my estimates because the control group would be negatively impacted by the reform. To address this potential concern, I use the variation in treatment intensity across different industries to check whether the tax effects on investment are stronger in industries with potentially a greater degree of competition due to a greater share of treated firms. The underlying assumption is that the differences in the tax effects across different industries are due to the competition effects driven by the treatment intensity. In practice, the differences in the tax effects on investment might be different across these treatment groups due to industry-level heterogeneity (i.e., capital intensity, productivity, etc).

To implement this, I first bin industries into quintiles based on the share of treated firms in a given industry. Then I estimate the tax effects on investment based on the same specification (1.8) separately for each bin. Then I plot the coefficient estimates across the treatment intensity groups, where each group indicates an average share of treated firms in each quintile. Figure A.10 plots the difference-in-difference estimates for the tax effects on investment across the treatment groups. The first dot indicates the difference-in-differences estimate of the tax effects on investment for the group where the average share of treated firms is about 14 percent. The last dot indicates the estimate for the group where the average share of treated firms is about 53 percent. If the competition effects are upward-biasing my estimates, then I should find a larger estimate for the group with the greater share of treated firms, where the competition effect would be greater. However, I fail to reject that the slope of the tax effects across the treatment intensity is not statistically different from zero, which is suggestive evidence that my main estimates are not driven by the potential competition effects.

Table 8: Main Results Without Basic or Additional Controls

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.409*** (0.103)	0.046*** (0.012)	0.001 (0.001)	0.001 (0.001)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.525	0.035	0.006	0.001
Implied Elasticity wrt (1-tau)	2.22	6.97	1.05	3.40
R-squared	0.63	0.29	0.73	0.28
Observations (firm-years)	4732	4503	4710	4715
Clusters (firms)	541	541	541	541

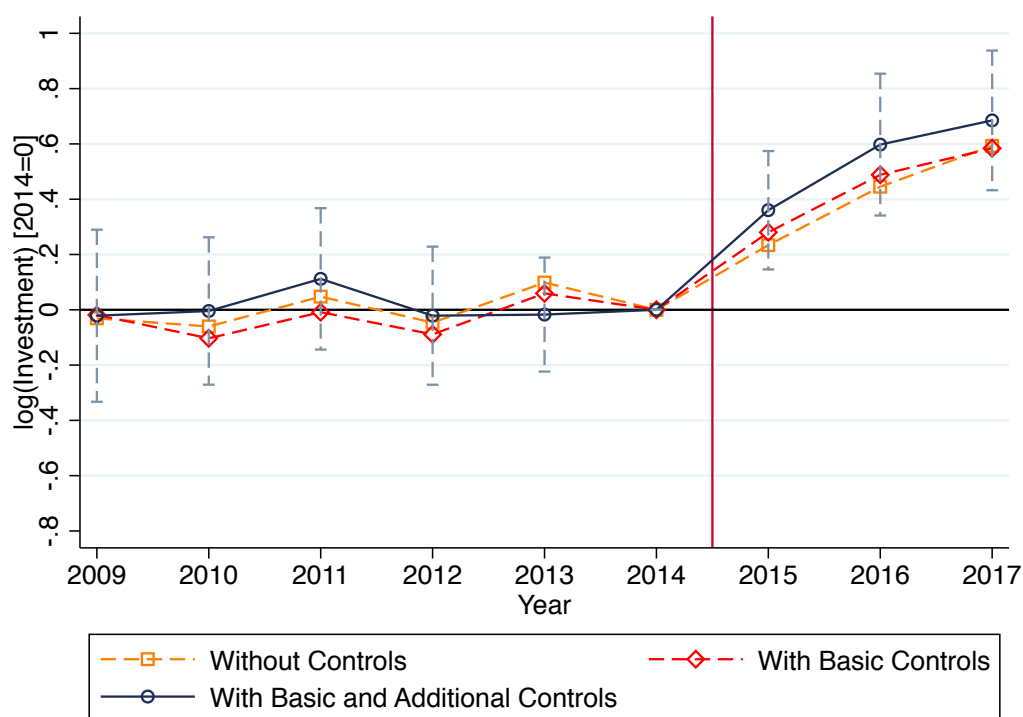
Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 9: Main Results With Only Basic Controls

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.449*** (0.107)	0.046*** (0.011)	0.001 (0.001)	0.001 (0.001)
Basic Control	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.525	0.035	0.006	0.001
Implied Elasticity wrt (1-tau)	2.44	7.00	0.92	2.64
R-squared	0.66	0.29	0.73	0.28
Observations (firm-years)	4732	4503	4710	4715
Clusters (firms)	541	541	541	541

Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Figure A.6: Tax Effects on Real Outcomes of Affected and Unaffected Firms



Notes: This figure shows the coefficients on $Treated \times Time$ for firms' investment, defined as $\log(\text{expenditures on physical capital assets})$, in equation (1.7). The dashed lines indicate 95% confidence intervals for these coefficient estimates. The solid vertical line indicates the reform year. The orange dash line indicates the coefficients in equation (1.7) without any controls and the red dash line indicates those with only basic controls.

Table 10: Main Results with Different Level (95%) of Winsorizing

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.508*** (0.110)	0.015*** (0.004)	0.002 (0.001)	0.001 (0.001)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.525	0.022	0.006	0.001
Implied Elasticity wrt (1-tau)	2.76	3.65	1.82	3.89
R-squared	0.68	0.31	0.74	0.28
Observations (firm-years)	4732	4503	4701	4715
Clusters (firms)	541	541	541	541

Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 11: Main Results by Balanced panel

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.549*** (0.121)	0.023** (0.010)	0.001 (0.001)	0.001 (0.001)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.817	0.034	0.007	0.001
Implied Elasticity wrt (1-tau)	2.98	3.64	1.23	4.91
R-squared	0.65	0.31	0.74	0.29
Observations (firm-years)	4263	4067	4243	4248
Clusters (firms)	477	477	477	477

Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 12: Main Results by Including Firms in Other Sectors

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.519*** (0.108)	0.046*** (0.012)	0.002 (0.001)	0.001 (0.001)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.514	0.035	0.006	0.001
Implied Elasticity wrt (1-tau)	2.82	7.04	1.44	2.79
R-squared	0.69	0.29	0.73	0.28
Observations (firm-years)	5255	5004	5208	5234
Clusters (firms)	602	602	602	602

Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 13: Main Results by Different Measures of Investment

	CAPEX/PPE	Net CAPEX/PPE	CAPEX/ASSET	Investment Rate	ln(PPE)
	(1)	(2)	(3)	(4)	(5)
Treated x Post	0.048*** (0.018)	0.044** (0.017)	0.015*** (0.006)	0.069*** (0.021)	0.103** (0.048)
Basic Control	Yes	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	0.207	0.195	0.066	0.112	16.825
Implied Elasticity wrt (1-tau)	1.25	1.22	1.27	3.34	0.56
R-squared	0.40	0.37	0.48	0.23	0.93
Observations (firm-years)	4435	4435	4435	4435	4435
Clusters (firms)	541	541	541	541	541

Notes: This table reports the tax effects on investment. The dummy for *Treated_i* equals 1 if a firm *i* had a tax reduction of 14 ppt, as explained in Section 4. The dummy for *post_t* equals 1 if the time period is after the end of the reform year (2014). Column (1) scales Investment by the lagged physical capital assets. Column (2) uses net expenditures on physical capital assets scaled by lagged tangible assets. Column (3) scales investment by lagged total assets. Column (4) uses investment rate, defined as annual changes in tangible assets, scaled by lagged tangible assets. The main outcomes are winsorized at 99% level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm FEs. The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 14: Main Results with Dollar-weighting by Revenues

	Investment		Capital Structure	
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payouts	Share Buybacks
Treated x Post	0.434*** (0.125)	0.025*** (0.008)	0.002 (0.001)	0.001 (0.001)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.525	0.035	0.006	0.001
Implied Elasticity wrt (1-tau)	2.36	3.88	1.78	4.86
R-squared	0.71	0.25	0.76	0.34
Observations (firm-years)	4731	4503	4701	4715
Clusters (firms)	541	541	541	541

Notes: This table reports the tax effects on investment. The dummy for *Treated_i* equals 1 if a firm *i* had a tax reduction of 14 ppt, as explained in Section 4. The dummy for *post_t* equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 15: Main Results with DFL-reweighting

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payouts	Share Buybacks
Treated x Post	0.448*** (0.120)	0.033** (0.013)	0.001 (0.001)	0.000 (0.001)
Basic Control	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.525	0.035	0.006	0.001
Implied Elasticity wrt (1-tau)	2.43	5.07	1.06	0.73
R-squared	0.64	0.42	0.72	0.33
Observations (firm-years)	4732	4503	4710	4715
Clusters (firms)	541	541	541	541

Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 16: Main Results with Accounting for Potential Selection of Treated Firms

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payouts	Share Buybacks
Treated x Post	0.511*** (0.129)	0.060*** (0.013)	0.002 (0.001)	0.001 (0.001)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.436	0.031	0.005	0.001
Implied Elasticity wrt (1-tau)	2.77	10.45	1.91	2.34
R-squared	0.68	0.29	0.74	0.28
Observations (firm-years)	4494	4280	4466	4477
Clusters (firms)	514	514	514	514

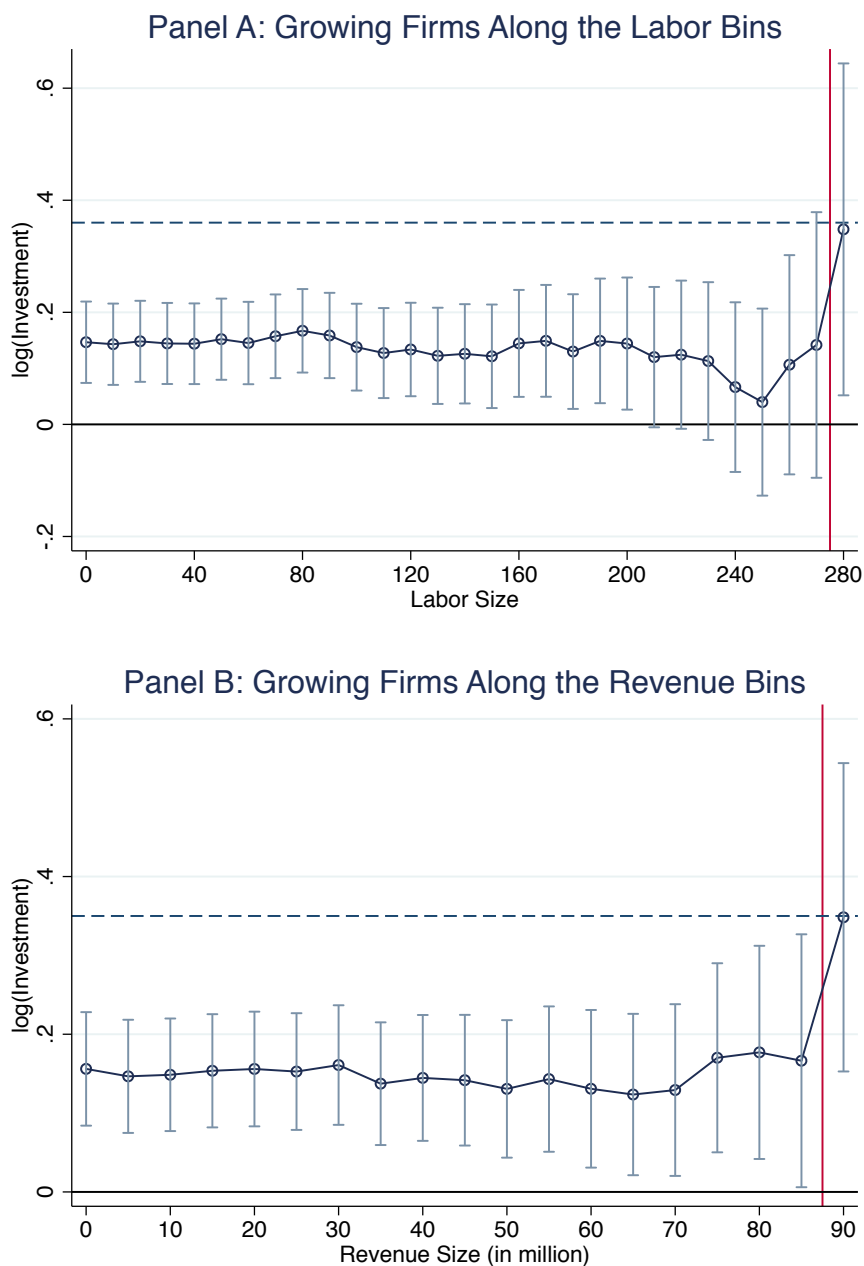
Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt (that passed any threshold in 2010), as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 17: Main Results by Treated Firms with Bunching at Old Cutoffs

	Bunching Firms	Bunching Below Revenue Cutoff	Bunching Below Labor Cutoff
	(1) ln(CAPEX)	(2) ln(CAPEX)	(3) ln(CAPEX)
Treated x Post	0.388*** (0.102)	0.409*** (0.121)	0.332** (0.163)
Controls	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes
Pre-reform Treated Mean	14.376	14.334	14.528
Implied Elasticity wrt (1-tau)	2.11	2.22	1.80
R-squared	0.78	0.78	0.79
Observations (firm-years)	10024	9741	9360
Clusters (firms)	1172	1139	1093

Notes: This table reports the tax effects on the main corporate outcomes based on the difference-in-differences estimation, where I define treated firms as those that were bunching at the old cutoffs prior to 2014. The dummy for $Treated_i$ equals 1 if a firm i either had a tax reduction of 14 ppt or faced an incentive to invest, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenues. The main outcomes are winsorized at 99% level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample includes both publicly listed and private companies. All specifications include time and firm FEs. The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Figure A.7: Coefficients on Treated x Post for $\ln(\text{Investment})$: Growing Firms



Notes: Panel A in this figure shows the coefficients on the interacted dummies (Treated x Time) for each time period, for firms that were growing along the labor dimension (as discussed in Appendix A.4). The outcome variable is the firm's investment, as defined by $\log(\text{expenditures on physical capital assets})$. The dashed lines indicate the 95% confidence intervals for those coefficient estimates. The solid vertical line indicates the bin that separates bunching firms from growing firms. The bin size is the average employees of five. Panel B in this figure shows the coefficients along the revenue dimension, where the bin size is total revenues of \$5 million. The sample includes both publicly listed and private firms.

Table 18: Main Results with Forward-looking firms as an Additional Control

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.383*** (0.102)	0.042** (0.016)	0.002 (0.001)	0.001 (0.001)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.507	0.039	0.006	0.001
Implied Elasticity wrt (1-tau)	-2.08	-5.86	-1.48	-4.45
R-squared	0.69	0.35	0.68	0.24
Observations (firm-years)	7616	7211	7584	7566
Clusters (firms)	922	922	922	922

Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section D.10. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 19: Placebo Test using earlier periods

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.138 (0.096)	0.004 (0.005)	0.002 (0.001)	0.000 (0.001)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.711	0.025	0.006	0.001
Implied Elasticity wrt (1-tau)	0.75	0.96	1.56	1.02
R-squared	0.62	0.29	0.73	0.32
Observations (firm-years)	4649	4426	4628	4644
Clusters (firms)	541	541	541	541

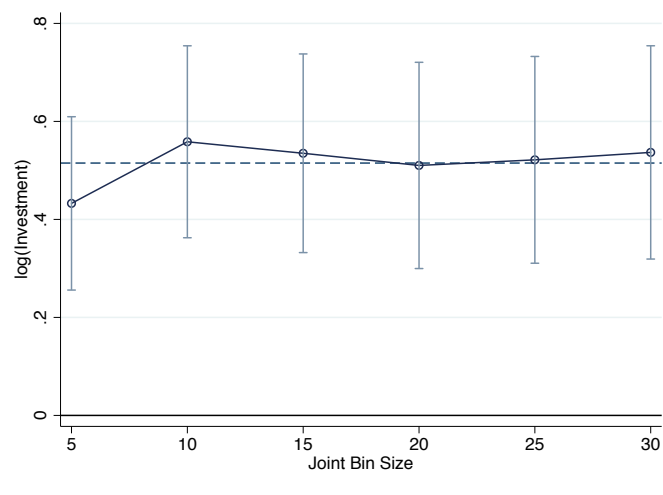
Notes: This table reports the tax effects on the main corporate outcomes as a placebo test. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the placebo year (2011). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 20: Placebo Test using random cutoffs

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.065 (0.088)	0.017 (0.011)	0.002 (0.001)	0.001 (0.001)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	15.377	0.035	0.008	0.002
Implied Elasticity wrt (1-tau)	0.35	2.60	1.27	3.29
R-squared	0.62	0.22	0.72	0.27
Observations (firm-years)	4624	4408	4620	4614
Clusters (firms)	533	533	533	533

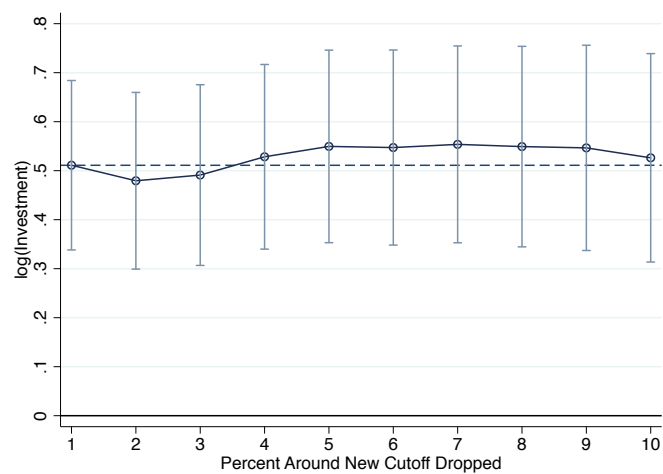
Notes: This table reports the tax effects on the main corporate outcomes as a placebo test. I use a random cutoff of \$450 million to define treated firms. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Figure A.8: Dropping Treated Firms Right Above the Old Cutoffs



Notes: This figure shows the coefficients on the interacted dummies (Treated x Time) for each time period, where I incrementally drop firms in paired-bins of 5 employees and 5 million dollar in revenues (as discussed in Appendix A.4). The outcome variable is the firm's investment, as defined by $\log(\text{expenditures on physical capital assets})$. The dashed horizontal line indicates the main estimates without dropping any of these firms. The dashed lines indicate the 95% confidence intervals for those coefficient estimates.

Figure A.9: Dropping Treated Firms Right Around New Threshold



Notes: This figure shows the coefficients on the interacted dummies (Treated x Time) for each time period, where I incrementally drop firms around the new cutoff in the range between one to ten percent (as discussed in Appendix A.4). The outcome variable is the firm's investment, as defined by $\log(\text{expenditures on physical capital assets})$. The dashed horizontal line indicates the main estimates without dropping any of these firms. The dashed lines indicate the 95% confidence intervals for those coefficient estimates.

Table 21: Re-scaling Main Estimates using an Instrumental Variable

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.548*** (0.120)	0.047*** (0.012)	0.002 (0.001)	0.001 (0.001)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.525	0.035	0.006	0.001
Implied Elasticity wrt (1-tau)	2.97	7.29	1.43	3.91
R-squared	0.68	0.30	0.74	0.28
Observations (firm-years)	4732	4503	4710	4715
Clusters (firms)	541	541	541	541

Notes: This table reports the tax effects on investment using $Treat_{it}$ for $t = 2015$ as an IV for $Treated_{it}$ for $t \in (2016, 2017)$. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 22: Results on Investment and Capital Structure by Cash Constraints (Listed)

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.660*** (0.194)	0.057* (0.030)	0.001 (0.001)	0.000 (0.001)
Treated x Post x Cash-Rich	-0.565** (0.265)	-0.031 (0.032)	-0.001 (0.002)	-0.000 (0.001)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean (CR=0)	14.164	0.067	0.001	0.001
Implied Elasticity wrt (1-tau) (CR=0)	3.58	4.64	2.23	2.10
Pre-reform Treated Mean (CR=1)	14.7	0.013	0.011	0.00072
Implied Elasticity wrt (1-tau) (CR=1)	0.52	10.64	-0.16	2.50
R-squared	0.64	0.32	0.76	0.31
Observations (firm-years)	3123	2955	3104	3104
Clusters (firms)	357	357	357	357

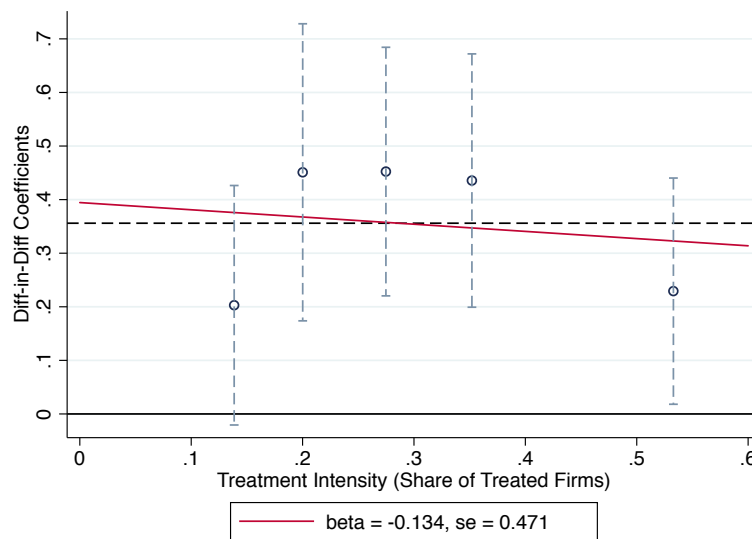
Notes: This table reports the tax effects on investment based on specification (1.10). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, as explained in Section 1.4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). The dummy for CR_i is 1 if the firm is cash-rich firm, as defined in Section D.15. Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 23: Results on Investment and Capital Structure by CEO Ownership (Listed)

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payout	(4) Share Buyback
Treated x Post	0.526*** (0.160)	0.104*** (0.022)	0.004 (0.003)	0.000 (0.001)
Treated x Post x CEO	-0.550** (0.230)	-0.102*** (0.026)	-0.006 (0.004)	0.000 (0.002)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean (CEO=0)	14.003	0.046	0.002	0.001
Implied Elasticity wrt (1-tau) (CEO=0)	2.86	12.20	9.15	2.46
Pre-reform Treated Mean (CEO=1)	15.0	0.024	0.012	0.0026
Implied Elasticity wrt (1-tau) (CEO=1)	-0.13	0.54	-0.88	0.89
R-squared	0.64	0.31	0.74	0.30
Observations (firm-years)	2969	2825	2956	2959
Clusters (firms)	340	340	340	340

Notes: This table reports the tax effects on investment based on specification (1.11). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, as explained in Section 1.4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). The dummy for CEO_i is 1 if the firm's CEO has a high stock share, as defined in Section D.16. Investment is defined as log of expenditures on physical capital assets. Newly issued equity is measured as non-negative changes in total paid-in capital, scaled by lagged revenue (Yagan 2015). Dividend payouts and share repurchases are scaled by lagged revenue. The main outcomes are winsorized at the ninety-ninth level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Figure A.10: Tax Effects on Investment Across Different Treatment Intensity Groups



Notes: This figure plots difference-in-differences coefficients in each quintile of treatment groups based on their industry-level treatment intensity (as discussed in Section D.17). The first dot indicates the coefficient estimate for the first quintile of treatment groups where the average share of treated firms is about 14 percent. The last dot indicates the coefficient estimate for the last quintile of treatment groups where the average share of treated firms is about 53 percent.

A.5 External Validity

In Appendix A.5, I provide a set of descriptive statistics, comparing (1) listed-treated firms and private firms in Korea, and (2) treated firms in Korea and C-corporations in the U.S., for external validity.

A.5.1 Comparing Treated-listed firms to Private firms in Korea

Table 24 provides descriptive statistics on listed and private companies in Korea during the sample period. The main comparison group is listed-treated firms and average private firms - Columns (2) and (6). Even though listed companies in my analysis sample, which account for a third of the affected firms, are relatively small compared to an average private firm in terms of total revenue, these affected firms are comparable in terms of labor, total asset, total capital and capital expenditures, compared to an average private firm in Korea. Therefore, the fact that the affected firms are relatively small in my sample does not imply that they are small compared to an average (representative) firm in Korea.

A.5.2 Comparing firms to C-corporations in the U.S.

Table 25 provides comparison of C-corporations in the U.S. (based on Table 1 of Yagan (2015)) and treated firms (publicly listed and private) in Korea. Average firm-size characteristics, such as revenue, asset, and investment, are larger for firms in Korea compared to C-corporations. However, other corporate outcomes, such as buyback and dividend payouts, are similar to one another.

Table 24: Descriptive Statistics for Listed and Private Companies in Korea

	Treated			Combined		
	(1) All	(2) Listed	(3) Private	(4) All	(5) Listed	(6) Private
Total Revenue (in million)	74.69 (45.16)	95.13 (44.34)	67.10 (43.06)	208.1 (172.7)	252.4 (187.5)	179.1 (155.6)
Labor (Average Employees used)	243.1 (174.5)	254.3 (135.5)	238.9 (186.9)	356.3 (278.0)	431.5 (290.9)	307.0 (257.6)
Total Asset (in million)	75.02 (64.65)	123.1 (68.29)	57.16 (53.15)	197.2 (181.5)	274.4 (197.7)	146.7 (149.9)
Total Capital (in million)	40.50 (41.94)	74.52 (46.68)	27.86 (31.83)	102.7 (103.3)	150.4 (110.2)	71.48 (85.07)
CAPEX (in million)	4.067 (6.086)	5.763 (7.647)	3.436 (5.257)	9.456 (12.76)	12.06 (14.03)	7.750 (11.54)
CAPEX / lagged PPE	0.219 (0.264)	0.186 (0.224)	0.231 (0.277)	0.206 (0.242)	0.181 (0.212)	0.222 (0.258)
Observations	2635	714	1921	10672	4223	6449

Notes: Sample years include 2009-2017. Small firms are jointly below the revenue, labor, total capital, and asset thresholds prior to 2014. Tangible assets are the book value of tangible capital assets. Revenue growth is percentage change in revenues. Age is the year of the audit report filed minus the establishment year. Labor is the average employees used in a given year. CAPEX is expenditures on physical capital assets, such as plants, property, and equipment. Investment is scaled by lagged physical capital assets.

Table 25: Descriptive Statistics for C-corporations (US) and Listed & Private firms (Korea)

Variables	C-corporations (Mean)	Listed & Private Firms (Mean)
revenue	69.21	74.69
assets	45.33	75.02
revenue growth	0.15	0.18
age	26	19
investment	2.25	4.07
investment/lagged tangible assets	1.608	0.22
share buyback / lagged revenue	0.003	0.003
dividends / lagged revenue	0.012	0.01
equity issued / lagged revenue	0.239	0.04
Number of firm-year observations	195033	4573

Notes: The descriptive statistics for C-corporations are derived from Table 1 in Yagan (2015). More details on the exact definitions and other variables can be found in the paper. Tangible assets are the book value of tangible capital assets. Revenue growth is percentage change in revenues. Age is the year of the audit report filed minus the establishment year. CAPEX is expenditures on physical capital assets, such as plants, property, and equipment. Investment is scaled by lagged physical capital assets. Share buybacks, dividend payouts, and equity issuances are only for publicly listed firms in Korea.

A.6 Other Outcomes

In Appendix A.6, I provide a set of estimates of the tax effects on other outcomes: (1) average employees, (2) total revenues, (3) Capital Structure, (4) return on assets, (5) probability of firms going public through initial public offerings, and (6) expenditures on R&D.

Table 26 shows the effects of reducing capital gains tax rates from 24 percent to 10 percent on firms' average employees and revenues. Column (1) shows that the coefficient is 0.092, implying that affected firms increased employees by 9 log points on average after the reform, compared to unaffected firms, based on a sample of both publicly listed and private firms. The tax effects on employment are similar for publicly traded and private firms, although the coefficient for publicly trade firms is marginally significant at the 10 percent significance level. Note that the implied elasticity of labor with respect to the net of tax rate, 0.54, is remarkably similar to the capital stock elasticity of 0.56 with respect to the net of tax rate.

Column (2) of Table 26 shows the effects of reducing capital gains tax rates from 24 percent to 10 percent on firms' total revenues. The coefficient is 0.197, implying that affected firms increased revenues by 20 log points after the reform, compared to unaffected firms, based on a sample both publicly listed and private firms. The tax effects on revenues are the same for just private firms, although the effects are 4.3 log points lower for publicly trade firms. Overall, increases in total revenues are consistent with that these affected firms increased investment and average employees, although increasing revenues would potentially make affected firms cross the new threshold after the reform.

Table 27 shows the effects of reducing capital gains tax rates on firms' capital structure, profitability, probability of going public, and R&D expenditures. Column (1) shows that the effects of lowering capital gains taxes on debts are both economically and statistically insignificant. However, Column (2) shows that the

affected firms decreased debt to equity ratio, implying that the book value of total equity increased for these firms. Moreover, Column (3) shows that the paid-in-capital relative to debts increased significantly, consistent with the finding that they increased newly issued equity and issuing new equity is relatively cheaper than borrowing after the capital gains tax rates decreased. Column (4) shows that the effects of lower capital gains taxes on firms' returns on assets are not statistically different from zero. This could be due to the fact that it might take more than three years for firms to recoup the benefits of investment. Column (5) shows that lowering capital gains tax rates increased the probability of private firms becoming publicly traded through initial public offerings by 16 percentage points after the reform, compared to unaffected firms. Note that the about 61 percent of affected firms switched from private to publicly listed during the pre-reform period. This proportion increased by 16 percentage points after the reform, compared to unaffected firms, after the affected firms experienced a reduction in capital gains tax rates from 24 percent to 10 percent. Finally, Column (6) shows that lowering capital gains tax rates increased affected firms' expenditures on research and development, scaled by lagged revenue, by 0.6 cents per dollar of lagged revenue. Therefore, most of these other outcomes were affected in a way that was consistent with the increases in investment and equity issuances of treated firms.

Table 26: Results on Employees and Revenues (Publicly Listed and Private Firms)

	Listed and Private Firms		Listed Firms Only		Private Firms Only	
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Employees)	ln(Revenues)	ln(Employees)	ln(Revenues)	ln(Employees)	ln(Revenues)
Treated x Post	0.092*** (0.027)	0.197*** (0.031)	0.100* (0.053)	0.154*** (0.054)	0.083*** (0.032)	0.197*** (0.040)
Basic Control	Yes	Yes	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	5.085	17.693	5.399	17.889	5.002	17.629
Implied Elasticity wrt (1-tau)	0.50	1.07	0.54	0.84	0.45	1.07
R-squared	0.94	0.91	0.91	0.91	0.94	0.90
Observations (firm-years)	11069	12582	4231	4735	6838	7847
Clusters (firms)	1477	1477	541	541	936	936

Notes: This table reports the tax effects on other corporate outcomes based on the difference-in-differences estimation. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 1.4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Employees are defined as the average number of workers employed over the total operating period in a given year. Revenues are defined as the total revenues. Basic controls are quartics in firm age and industry dummies interacted with time dummies. Additional controls are dummies for pre-reform (2014) operating profit quintile interacted with time dummies. The main outcomes are winsorized at 99% level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample includes both publicly listed and private companies. Basic controls are All specifications include time and firm fixed effects (FEs). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 27: Results on Other Outcomes (Publicly Listed Firms)

	Capital Structure			Profits	Initial Public Offerings	Other Investment
	(1)	(2)	(3)	(4)	(5)	(6)
	Debts	Debt-Equity	Paid-in-Cap	Return on Assets	Pr(Going Public)	RnD Expenditures
Treated x Post	0.033 (0.070)	-0.161*** (0.061)	0.048** (0.023)	0.051 (0.045)	0.159*** (0.038)	0.006*** (0.003)
Time and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	17.127	1.041	0.258	0.285	0.614	0.017
Implied Elasticity wrt (1-tau)	0.18	-0.84	1.02	0.98	1.40	1.78
R-squared	0.90	0.77	0.74	0.61	0.73	0.69
Observations (firm-years)	4736	4736	4736	4736	4736	4735
Clusters (firms)	541	541	541	541	541	541

Notes: This table reports the tax effects on other corporate outcomes based on the difference-in-differences estimation. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 1.4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Debts are the book value of total liabilities. Debt to Equity ratio is the book value of total liabilities scaled by the book value of total equity (total assets minus total liabilities). Paid-in-Cap is the paid-in-capital relative to the book value of total liabilities. Return on assets is defined as net profits scaled by total assets. $Pr(GoingPublic)_{it}$ is the probability that a firm i becomes publicly listed through a initial public offering in year t . RnD Expenditures are firms' expenses on research and development activities (i.e. hiring researchers and buying lab equipments), scaled by lagged revenue. Basic controls are quartics in firm age and industry dummies interacted with time dummies. Additional controls are dummies for pre-reform (2014) operating profit quintile interacted with time dummies. The main outcomes are winsorized at 99% level. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (FEs). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Appendix B

Supplementary Materials for Chapter 2

B.1 Robustness Checks

In Appendix B.1, we provide a set of robustness tests for the main results in Section 2.3.

B.1.1 Without Controls

We repeat the main analysis in equation (2.1), without basic or additional controls and with only basic controls. Panel A of Figure B.1 shows the main results on $\log(\text{investment})$ without any basic or additional controls, and Panel B of Figure B.1 shows the main results on $\log(\text{tangible assets})$ without any basic or additional controls. The coefficient estimates after the year 2014 are smaller when we do not include any controls or when we include only basic controls, but the results are qualitatively similar to the ones from the main specification in equation (2.1).

B.1.2 With Different Levels of Winsorizing

We repeat the main analysis using the same specification as in equation (2.2), winsorizing (top-coding) main outcome variables at the 95% level, instead of at

the 99% level. Table 1 shows the main results based on this different level of winsorizing. The estimates on this table suggest that the main results are robust to and qualitatively similar when using different levels of winsorizing.

B.1.3 Using Balanced Panel

We repeat the main analysis using the same specification as in equation (2.2), using a balanced panel. Table 2 shows the main results based on using the balanced panel. The main results are qualitatively similar whether the sample is balanced or not.

B.1.4 Including Firms in Other Sectors

We repeat the main analysis using the same specification as in equation (2.2), including firms in other sectors. Table 3 shows the main results based on using firms in other sectors in addition to the firms in the main analysis sample. The main results are qualitatively similar when we include firms in other sectors.

B.1.5 Firms that Bunched at Old Cutoffs

We repeat the main analysis on investment using firms that were bunching at either of the old cutoffs prior to the reform. Since firms were bunching precisely to avoid higher capital gains tax rates, removing the old cutoffs may increase their incentive to invest. Table 4 shows results just using firms that were bunching as treated group and unaffected firms as control. As columns (1) and (3) show, their investment response is slightly higher than the average investment response from the overall affected firms that include both types of treated firms, although the difference is not statistically different from one another.

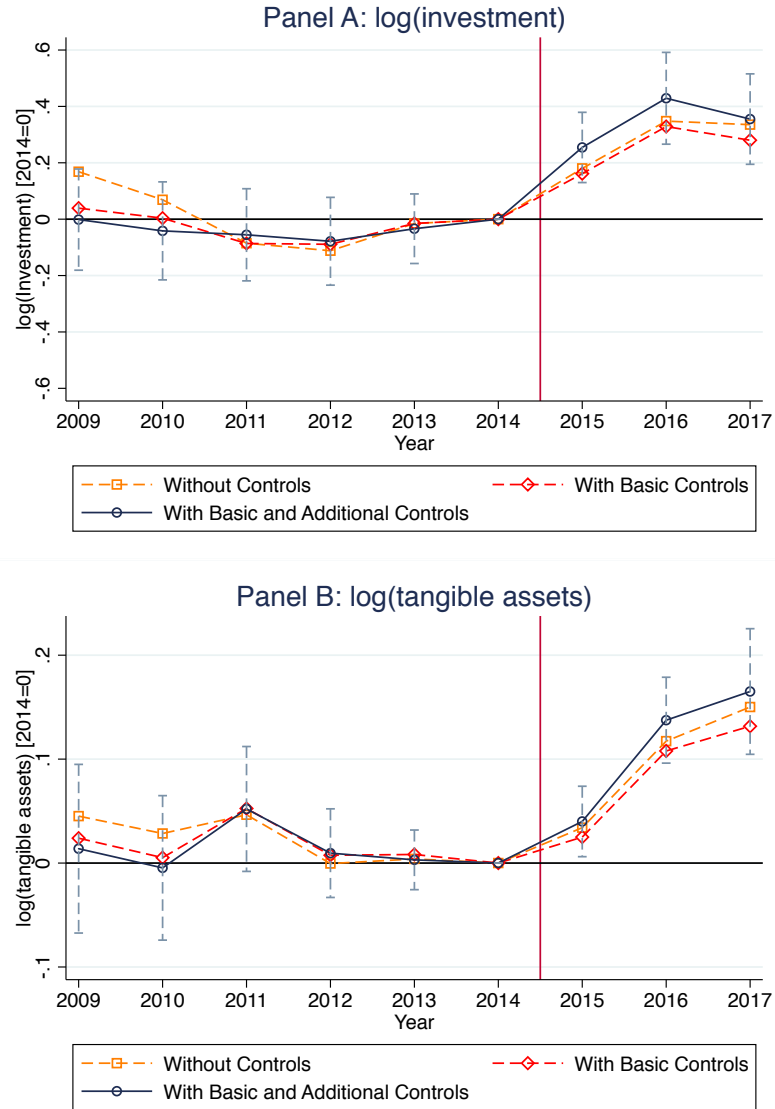
B.1.6 Placebo Test using earlier periods

Since other time-varying shocks, such as different policy reforms, may coincide or occur close to the reform in 2014, we conduct placebo tests using earlier time periods. For example, there was a corporate tax reform in 2012, which moderately changed the tax schedule for firms making under \$200,000. Even though this change was small enough so that it would not necessarily affect the overall results, we still use year 2012 as the placebo date and set *Post* equal to 1 if it is after 2012. Table 5 shows the results based on the placebo test. The coefficients are not statistically different from zero even at the 90% confidence level, suggesting that there were no other changes that could have affected the main results around this period of time.

B.1.7 Placebo Test using random cutoffs

To ensure that the mains results on investment are driven by the policy variation, which generated a set of firms whose investment incentives have changed due to changes in regulations on firm size, we conduct a placebo test using random cutoffs to define the treated group. For example, we set an arbitrarily much higher value of \$450 million (instead of \$150 million) for the new revenue threshold, so that many of the unaffected firms are defined now as treated firms. The placebo tests are qualitatively similar if we use other random cutoffs (much lower or higher threshold). Table 6 shows the results based on the placebo test. The coefficients are not statistically different from zero even at the 90% confidence level, suggesting that the policy variation based on the reform in 2014 was the main driver of the investment responses for the affected firms.

Figure B.1: Tax Effects on Real Outcomes of Affected and Unaffected Firms



Notes: Panel A of this figure shows the coefficients on $Treated \times Time$ for firms' investment, defined as $\log(\text{expenditures on physical capital assets})$, in equation (2.1). The dashed lines indicate 95% confidence intervals for these coefficient estimates. The solid vertical line indicates the reform year. The orange dash line indicates the coefficients in equation (2.1) without any controls and the red dash line indicates those with only basic controls. Panel B of this figure shows the coefficients on $Treated \times Time$ for firms' $\log(\text{tangible assets})$.

Table 1: Main Results with Different Level (95%) of Winsorizing

	Investment	Capital Stock	Net Investment	Investment Rate
	(1)	(2)	(3)	(4)
	$\ln(I)$	$\ln(K)$	dK/K	I/K
Treated x Post	0.353*** (0.068)	0.087** (0.041)	0.037*** (0.011)	0.039*** (0.011)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.139	16.323	0.062	0.141
Implied Elasticity wrt (1-tau)	1.92	0.47	3.21	1.49
R-squared	0.72	0.93	0.23	0.43
Observations (firm-years)	12496	12693	12700	4732
Clusters (firms)	1477	1477	1477	1477

Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 2.3. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Variables are as defined in Section 2.3. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 2: Main Results by Balanced panel

	Investment	Capital Stock	Net Investment	Investment Rate
	(1)	(2)	(3)	(4)
	$\ln(I)$	$\ln(K)$	dK/K	I/K
Treated x Post	0.362*** (0.073)	0.123*** (0.033)	0.036*** (0.014)	0.044*** (0.012)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.322	16.580	0.094	0.150
Implied Elasticity wrt (1-tau)	1.96	0.67	2.10	1.60
R-squared	0.71	0.94	0.19	0.43
Observations (firm-years)	10813	10890	10891	4263
Clusters (firms)	1223	1223	1223	1223

Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 2.3. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Variables are as defined in Section 2.3. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 3: Main Results by Including Firms in Other Sectors

	Investment	Capital Stock	Net Investment	Investment Rate
	(1) ln(I)	(2) ln(K)	(3) dK/K	(4) I/K
Treated x Post	0.349*** (0.065)	0.119*** (0.039)	0.038*** (0.013)	0.044*** (0.012)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.124	16.283	0.083	0.150
Implied Elasticity wrt (1-tau)	1.90	0.64	2.47	1.58
R-squared	0.72	0.92	0.20	0.42
Observations (firm-years)	15012	15250	15258	5255
Clusters (firms)	1814	1814	1814	1814

Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 2.3. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Variables are as defined in Table Section 2.3. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 4: Main Results by Treated Firms with Either Tax Cut and/or Bunching

	Bunching Firms	Tax Cut Firms	Bunching and Tax Cut Firms
	(1) ln(I)	(2) ln(I)	(3) ln(I)
Treated x Post	0.388*** (0.109)	0.356*** (0.068)	0.361*** (0.062)
Controls	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes
Pre-reform Treated Mean	14.376	14.140	14.188
Implied Elasticity wrt (1-tau)	2.11	1.93	1.96
R-squared	0.71	0.72	0.71
Observations (firm-years)	10024	12496	13461
Clusters (firms)	1172	1477	1591

Notes: This table reports the tax effects on the main corporate outcomes based on the difference-in-differences estimation, where I combine treated firms for those that face lower tax rates after the reform with firms that were bunching prior to 2014. The dummy for $Treated_i$ equals 1 if a firm i either had a tax reduction of 14 ppt or faced an incentive to invest, as explained in 2.3. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Variables are as defined in Section 2.3. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 5: Placebo Test using an Earlier Period

	Investment	Capital Stock	Net Investment	Investment Rate
	(1)	(2)	(3)	(4)
	$\ln(I)$	$\ln(K)$	dK/K	I/K
Treated x Post	0.128 (0.097)	0.052 (0.048)	0.020 (0.018)	0.023 (0.017)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.126	16.250	0.100	0.146
Implied Elasticity wrt (1-tau)	0.70	0.28	1.06	0.86
R-squared	0.72	0.93	0.20	0.42
Observations (firm-years)	12247	12440	12447	4649
Clusters (firms)	1476	1476	1476	1476

Notes: This table reports the tax effects on the main corporate outcomes as a placebo test. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 2.3. The dummy for $post_t$ equals 1 if the time period is after the end of the placebo year (2012). Variables are as defined in Table Section 2.3. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 6: Placebo Test using a Random Cutoff

	Investment	Capital Stock	Net Investment	Investment Rate
	(1)	(2)	(3)	(4)
	$\ln(I)$	$\ln(K)$	dK/K	I/K
Treated x Post	0.115 (0.085)	0.059 (0.044)	-0.003 (0.014)	0.012 (0.013)
Basic Control	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	Yes	Yes	Yes	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.892	17.060	0.093	0.146
Implied Elasticity wrt (1-tau)	0.63	0.32	-0.16	0.43
R-squared	0.72	0.93	0.20	0.42
Observations (firm-years)	12135	12324	12331	4624
Clusters (firms)	1440	1440	1440	1440

Notes: This table reports the tax effects on the main corporate outcomes as a placebo test. I use a random cutoff of \$450 million to define treated firms. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Variables are as defined in Table Section 2.3. Each time period is a year, and the sample period is from 2009 to 2017. The reform was announced in the middle of 2014 and implemented by the end of 2014. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Appendix C

Supplementary Materials for Chapter 3

C.1 Additional Tables

In Appendix C.1, we include additional tables relevant for the interpretation of our results.

Table 1: News Articles Concerning Appointments of Lee Myung Bak-Related Individuals

Date	Paper, Headline	Content
Jul. 18, 2010	Kyunghang Business “Major Appointments in Financial Sector Have Parachute Appointments Without Question Regardless of Their Expertise”	In the non-governmental financial sector, numerous MB figures with KU, Hyundai, presidential campaign, and presidential transition team ties have been appointed. In fact, 3 out of 4 CEOs of major financial share holding companies (KB, Hana, and Woori) in Korea have all graduated from Korea University.
Dec. 27, 2010	DongA.com “The Sword of Justice Passes Over Public Firms’ Parachute Auditors”	Out of 23 auditor positions that were replaced after this speech in public firms, 14 (60.8%) positions had backgrounds in President Lee’s presidential election campaign, work experience in the Blue House, and outside organizations in conservative faction. The Blue House is refusing to give a clear feedback on this concern. Some advisers even hold the theory of “inevitability of parachute appointments,” arguing that 1) it is a practice that has been done by past administrations, 2) realistically, the administration has to repay the people who have helped the president during the election and 3) the administration has to secure allies during the next presidential election.
Jun. 2, 2011	Yonhap News “Democrats: 53 People Connected to the Government Appointed in the Financial Sector”	53 people with MB ties (presidential transition team, Korea University, Somang Church) were appointed in the financial sector as chairmen, board members, external directors, etc.
Sep. 1, 2011	Pressian “MB’s Continued Love for Hyundai Engineering & Construction...”	The opposition Democratic Party vehemently protested against the potential appointment of Kim Joong-Kyum as Korea Electric Power CEO. Kim had previously been CEO of Hyundai Engineering and Construction. Chang Byung-Wan of the Democratic party pointed out that, faced with difficulty in business operations, what Korea Electric Power needed was someone with great expert knowledge in the relevant field (and/or has a broad, international network of personal connections to leverage), and not someone like Kim who comes from a very different background. Five more appointments of former Hyundai Engineering & Construction employees as CEOs of SEOs.
Oct. 4, 2011	Kyunghang Shinmun “Many Promotions of People from KU and the TK Region in the Prosecutor’s Office During MB’s Administration”	Senior executives in the public prosecutor’s office during MB administration have shown a tendency to be promoted if they graduated from Korea University or are from TK (Taegu & Gyeongbuk) regions. According to the Department of Justice’s report that shows the present conditions of promotions, 17.6% of chief prosecutors who have been promoted (9 out of 51) for the last 4 years have graduated from Korea University. This is about twice the rate during President Roh Moo-hyun’s administration. Last August, Mr. Choi Gyo-il, former Department of Justice’s director of public prosecutions, was appointed with promotion as Director of Seoul Central District Prosecutor’s Office, a position that is referred to as “the flower of the prosecutor’s office.” He is both from Gyeongbuk region and Korea University.
Oct. 6, 2011	Hankyoreh “MB’s Parachute Appointments 332 People”	At a congressional strategic meeting, Kim Jin-Pyo noted that in the most recent inspection of government offices, there were as many as 332 unfair/preferred appointments to key government and public company positions. Kim added that even the mass power outage in September could have been caused by the prevalence of such biased appointments. Congress’ Land, Transport and Maritime Affairs Committee governs 20 public companies, and 70.5% of the non-executive directors at these companies had been identified as being pro-MB, had endorsed the Hannara Party, had graduated from Korea University, or were from Yeongnam.
Jan. 16, 2012	Pressian “Privatization of KTX, It Was MB’s Korea University Figures Controlling After All”	Dongbu (Chairman from KU), Daewoo (Chairman from KU), Saneun that lead the privatization are all on the side of MB. The firms that are widely agreeing on the administration’s KTX partial privatization plan all have special connections to President Lee. Even though Grand National Party’s opposition is apparent, the Blue House said that it will continue its plan.
Feb. 21, 2012	Media Today “Until the End of the Administration, Appointments Based on Revolving Doors/Parachutes”	Using Social Network Analysis, JoongAng Daily announced that one additional tie with the president would lead to a 43% in the chance to be appointed in a high position again. JoongAng Daily also reported that most of 76 officers who were appointed in the “4 major authority institutions” – National Intelligence Service, prosecution, police, National Tax Service – are from Yeongnam region or Korea University. All institutions generally increased the number of officers who are from Yeongnam region or graduated from Korea University as time passed. The newspaper also pointed out that it is an unprecedented event that three Seoul Central District Prosecutors in a row are from Korea University.

Table 2: News Articles Concerning Appointments of Lee Myung Bak-Related Individuals (continued)

Date	Paper, Headline	Content
Mar. 20, 2012	eDaily “4 Years of MB Administration Changed Financial Power Structure”	Since the very beginning of his term as President, there has been controversy surrounding appointments to key government posts—in particular, the administration established very strong ties with alumni of Korea University, Christians who go to Somang Church, and those from the Yeongnam region. These biased appointments were not just restricted to government roles, however. For example, the financial sector was heavily influenced by the MB administration’s preference for Korea University, Somang Church, and Yeongnam personnel. In its analysis, E Daily News found evidence of a major transformation in the power structure among managers of major banks since ‘08.
Apr. 16, 2012	Segye Daily News “Supposed to Have Hired Experts to Lead Educational Organizations; In Reality 66% Are Unqualified Parachute Appointments”	In June of 2009, President Lee Myung-Bak had promised to pay more attention to fair appointments, with the ministers being held responsible rather than the president intervening in the appointment process. According to Sekye Daily News’ comprehensive survey of 10 organizations supervised by the Ministry of Education and Science, 66% of the appointees had political ties to the President. Even those institutions requiring higher levels of expertise in science were led by unqualified appointees. Supposed to monitor the management and finances of public institutions, inspectors and directors have also been appointed by the MB Administration.
May. 14, 2012	Sedaily “Banks Parachute Kingdom During the MB Administration”	As the current administration’s poor performance was publicized through the investigation of major firms, it was also discovered that 73 out of 207 non-executive directors who were appointed during 4 years and 3 months of MB administration are MB figures or from the MB administration. Knowing that over one-third of the commercial bank system is filled with people appointed with their ties, we can assume a serious loophole in the bank’s checking system. One high official from the financial sector has pointed out that the institution of nonexecutive directors is being used as a means of lobbying for the government, not for the intended purpose of checking and balancing power.
Jul. 24, 2012	news1Korea “80% of Leaders of National Research Council for Economics, Humanities, and Social Sciences Dubious of Being Parachutes”	Representative Sung Wan-jong of Advancement Unification Party criticized the National Research Council for Economic, Humanities, and Social Sciences for an appointment corruption, saying its objectivity and impartiality have been disrupted from the activity. He announced that “Out of 23 chief directors under National Research Council, 13 are from President Lee’s Presidential Transition Committee and Policy Advisory Committee. If you add Ko-So-Yeong (Korea University – Somang Church – Yeongnam) ties, the number becomes 18.”
Sep. 27, 2012	Hankyoreh “Ambassador Appointments Changed to Payback Appointments by MB Government”	Another avenue for unfair appointments—Ambassador-at-large. 9 out of 12 were involved in the election campaign process or were active in the presidential transition committee.
Oct. 9, 2012	Nocutnews “Media Industry Controlled by MB Loyalist Parachutes”	Broadcasting industry, dominated by MB’s special broadcasting advisers and Korea University people. . . criticize checks in power, loss of freedom of expression. During a parliamentary inspection of the Korea Communications Commission, criticisms arose that the media’s independence and impartiality have been disrupted and freedom of press has been diminished after the start of President Lee’s administration. Congressman Kang Dong-won announced that as soon as MB administration was established, the press special advisers during MB’s presidential candidate days and people from Korea University have dominated the press, deteriorating Korea’s freedom of press. He also released a list of MB administration’s dominance over press as it appointed people who have connections to the president.
Jan. 1, 2013	Seoul Shinmun “1 Out of 2 Public Institution Auditors is Parachute”	118 out of 250 auditors in government institutions and public institutions worked in Blue House or government institutions or had other political backgrounds.
Jan. 2, 2013	Media Today “During 5 Years of MB Government Journalism Was Devasted With Korea University Parachutes”	The three major broadcasting companies MBC, KBS, and SBS were all led by Korea University graduates. Korea University student leaders noted that such bias in the media is shameful, and that even conservatives would criticize the state of affairs.

This table lists articles from the financial press related to the appointment of people from Lee Myung Bak’s networks in different places in the administration and the appointment of people from his networks in private banks. The first column shows the date of publication, the second column depicts the source of the article, and the last column summarizes the relevant passages of the article.

Table 3: News Articles Concerning the Fate of Lee Myung Bak-Related Individuals under Park Geun Hye

Date	Paper, Headline	Content
Feb. 20, 2013	Kyunghang Shinmun “Financial MB Man, Board Chairman of Smile Credit Resigns”	Kim Seung-yu [entered Korea University Finance major in same year as MB], board chairman of Smile Credit, has resigned, leaving one year in his term after resigning from the chairman position of Hana Financial Group. Because the board chairman position of Smile Credit is appointed by the president, he said, “I thought it was reasonable to resign after the new president has been elected.” With strong ties to President Lee, Board Chairman Kim has been considered one of the major 4 leaders of financial sector, along with Uh Yoon-dae [Korea University alumni], chairman of KB Financial Group, Lee Pal-sung [Korea University alumni], chairman of Woori Financial Group and Kang Man-su [First Minister of Strategy and Finance under MB], chairman of Saneun Financial Group.
March. 23, 2013	Money Today “MB Man Kang Man-Su Resigns... April Mass Resignation Starts”	Chairman Kang Man-Su of KDB Saneun Financial Group, representative MB figure, and Chairman Lee Ji-Song of Korea Land and Housing Corporation, the leader of the biggest public corporation, have resigned. Chairman Kang was considered as a symbol of MB-nomics as President Lee’s first minister of Ministry of Strategy and Finance. Even though he indicated that he will not step down immediately because of the works like Saneun Bank privatization and KDB Financial University, he seems to have been pressured by President Park’s words on replacement. Chairman Uh Yoon-Dae of KB Financial Group and Chairman Lee Pal-Sung of Woori Financial Group might not be able to complete their terms either. The cascade of replacement seems to have already started with Chairman Lee’s resignation, following resignation of Chairman Kim Kun-ho of Korea Water Resources Corporation.
March. 28, 2013	Newspim “MB Men in Public Enterprises Are Resigning One After Another Creating a Domino Effect”	Chairman Kang Man-su of KDB Financial Group, the most influential person of President Lee’s administration, has resigned. He has shown signs that he will resign, regardless of his term (1 year left), as the new administration starts. His decision seems to be in line with President Park’s recent remarks on selecting officers who follow the philosophies of the new administration. Public officers, like Chairman Kang, from financial sector and public corporations who have ties to President Lee have already resigned or are predicted to resign soon.
Apr. 1, 2013	Newspim “20 CEOs of Financial Public Enterprises Are Changed”	As the major leaders from financial sector who took office during MB administration are resigning without finishing their terms, the public is predicting that stock firms’ CEOs will follow. The leaders of public corporations affiliated with the stock market are also considered to be in the list of resignation. Chairmen of major financial groups that showed major ties to President Lee are resigning. While Saneun Financial Group has already chosen Hong Gi-taek as the next chairman following ex-Chairman Kang Man-su, KB and Woori financial groups are on their processes to choose their next leaders.
Apr. 10, 2013	Newspim “Board Members of FSS Resign Together, Mass Resignation from 2008 Repeated?”	All 9 board members of Financial Supervisory Service have resigned, making the public wonder whether mass resignation from 2008 would be repeated. On the year 2008, when President Lee’s administration started, 11 board members have resigned and 6 people were replaced with people from outside. MB administration was distinct in that it specifically elected members to be replaced, rather than following the traditional move to simply investigate the members.
Apr. 19, 2013	Seoul Finance “Are MB Men Financial Sector Chairmen Followed by Public Companies?”	As many representative MB figures start to resign, leaders in the stock market have fears that they might be next in line for replacement. Some of the four major finance leaders, like Chairman Lee Pal-sung of Woori Financial Group and Chairman Kang Man-su of KDB Financial Group, who had ties to President Lee with their schools and hometown already resigned. The public is predicting that the people next in line to be replaced are leaders in securities businesses.

Table 4: News Articles Concerning the Fate of Lee Myung Bak-Related Individuals under Park Geun Hye (continued)

Date	Paper, Headline	Content
Sep. 23, 2013	Ilyosisa "Park Geun-Hye Administration is Erasing MB Before and After"	The Park administration tries to erase all memories of MB administration by developing investigations of last administration's controversies and scandals and replacing the people who were appointed from MB years. A good example would be the "Financial Big 4" appointed during MB who have resigned one after another. Numerous CEOs of public firms have been pressured to resign by themselves, like Mr. Jang Tae-pyung, CEO of the Korean Horse Affairs Association who resigned, leaving 1 year and 2 months left in his term. Mr. Jung Jeong-kil, director of the Academy of Korean Studies, also resigned. Both of them share the fact that they are widely known as "MB men."
Nov. 11, 2013	Seoul Shinmun "Weeding Out MB Parachute Appointments in Park Administration... Appointment Cruelty Every 5 Years"	Even though there are legal processes and organizations to prevent the measure, public firms face a mass change in their leaders every 5 years, especially the ones who were appointed by the past administration. During the first 9 months of Park administration, many MB men whose terms have been extended by one year have received the full impact of this change, including Korea Gas Corporation, Korea District Heating Corp., and Korea Exchange. Many officials of the public firms, for this reason, have resigned themselves first, like Korea Technology Finance Corporation, Koscom, and Korea Securities Depository. Also, many Hyundai figures and past participants of presidential transition team resigned, expecting the tide of mass personnel change.
Nov. 12, 2013	Sedaily "End of MB Men in Financial Sector Speeds Up"	Leaders in the financial sector from past administration are getting nervous as the Park administration tries to expand its effort to cut off influences of MB ... For example, Kim Bong-soo, the chief director of Korea Exchange, resigned as one of the representative MB figures who graduated from the Law School of Korea University. Mr. Kim was replaced by Mr. Choi Kyung-soo, the former chairman of Hyundai Securities, whose appointment created some controversies as he himself was involved in President Park's election campaign. Other leaders of financial sector, like Mr. Kim Jung-guk, chief director of Korea Technology Finance Corporation, Mr. Kim Kyung-dong, chairman of Korea Securities Depository, Mr. Woo Ju-ha, chairman of Koscom, all expressed their desire to resign as MB men who received pressures from the new administration. Also, Kang Man-su, Lee Pal-sung, Uh Yun-dae, and Kim Seung-yu who were called the Big 4 of the financial sector during MB administration all resigned. One representative of the financial sector commented, "Until now every time the administration changes, the financial sector repeated its parachute appointments, which in most cases ended in a negative ending. I am worried as the new administration tries to erase the influences of the past administration completely, a big tornado would arise in the financial sector at the end of this year."

This table lists articles from the financial press related to resignations of people from Lee Myung Bak's networks in different places in the administration and from private banks following the election of Park Geun Hye. The first column shows the date of publication, the second column depicts the source of the article, and the last column summarizes the relevant passages of the article.

Table 5: Credit Volumes and Interest Rates - Long-Connected Firms

	I	II	III	IV	V	VI	VII
	Firm-Level			Relationship-Level			
Dep. Var.:	$\Delta \log(loans)_i$	$\Delta \left(\frac{loans}{assets} \right)_i$	ΔIR_i	$\Delta \log(loans)_{ij}$	$\Delta \left(\frac{loans}{assets} \right)_{ij}$	ΔIR_{ij}	Δrel_{ij}
KU_i	0.1789*	0.0145**	-0.0027**				
	(0.1067)	(0.0065)	(0.0013)				
KU_{ij}				0.5949***	0.0185*	-0.0076***	0.0692*
				(0.2230)	(0.0101)	(0.0023)	(0.0371)
con_{ij}				0.5805***	0.0295***	-0.0044**	0.0213
				(0.1518)	(0.0084)	(0.0019)	(0.0426)
Firm FE	-	-	-	yes	yes	yes	yes
Bank FE	-	-	-	yes	yes	yes	yes
Clustered SE	-	-	-	firm	firm	firm	firm
Observations	3,738	3,528	3,749	6,110	5,835	6,136	23,339
R-squared	0.001	0.001	0.001	0.658	0.0694	0.691	0.710

This table shows the results from estimating equation (3.1) in columns I to III, and equation (3.2) in columns IV to VII. The dependent variable is the log change in total loans for firm i after the election (2008-2012) compared to before the election (2003-2007) in column I, the change in loans to assets for firm i in columns II, the change in the average interest rate for firm i in column III, the log change in total loans from bank j to firm i in column IV, the change in loans from bank j to firm i , scaled by firm i 's assets in column V, the change in the average interest rate on loans from bank j to firm i in column VI, and takes the value of one if bank j and firm i start a new lending relationship after the election, and zero otherwise, in column VII. The variable KU_i takes the value of one for firms with a CEO from Korea University in 2003, and zero otherwise, the variable KU_{ij} takes the value of one for firm-bank relationships in which firm i has a CEO from the KU network in 2003 and bank j appoints an executive from Korea University after the election. The variable con_{ij} takes a value between minus one and one, and increases by 0.2 for each year in which firm i has a CEO from an alumni network from which bank j has an executive after the election, and decreases for each year in which firm i and bank j are connected to the same alumni network before the election. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Credit Volumes and Interest Rates - No State Bank Credit Years

	I	II	III	IV	V	VI	VII
	Firm-Level			Relationship-Level			
Dep. Var.:	$\Delta \log(\text{loans})_i$	$\Delta \left(\frac{\text{loans}}{\text{assets}} \right)_i$	ΔIR_i	$\Delta \log(\text{loans})_{ij}$	$\Delta \left(\frac{\text{loans}}{\text{assets}} \right)_{ij}$	ΔIR_{ij}	Δrel_{ij}
KU_i	0.1370** (0.0684)	0.0151*** (0.0060)	-0.0014* (0.0009)				
KU_{ij}				0.6876*** (0.1778)	0.0350*** (0.0107)	-0.0063*** (0.0020)	0.0540 (0.0476)
con_{ij}				0.6518*** (0.2116)	0.0285*** (0.0105)	-0.0047** (0.0020)	0.0460 (0.0476)
Firm FE	-	-	-	yes	yes	yes	yes
Bank FE	-	-	-	yes	yes	yes	yes
Clustered SE	-	-	-	firm	firm	firm	firm
Observations	3,486	3,284	3,494	5,154	4,911	5,176	20,387
R-squared	0.001	0.001	0.001	0.701	0.732	0.720	0.719

This table shows the results from estimating equation (3.1) in columns I to III, and equation (3.2) in columns IV to VII excluding firm-year observations for which firm i borrows from a government bank. The dependent variable is the log change in total loans for firm i after the election (2008-2012) compared to before the election (2003-2007) in column I, the change in loans to assets for firm i in column II, the change in the average interest rate for firm i in column III, the log change in total loans from bank j to firm i in column IV, the change in loans from bank j to firm i , scaled by firm i 's assets in column V, the change in the average interest rate on loans from bank j to firm i in column VI, and takes the value of one if bank j and firm i start a new lending relationship after the election, and zero otherwise, in column VII. The variable KU_i takes the value of one for firms with a CEO from Korea University in 2007, and zero otherwise, the variable KU_{ij} takes the value of one for firm-bank relationships in which firm i has a CEO from the KU network in 2007 and bank j appoints an executive from Korea University after the election. The variable con_{ij} takes a value between minus one and one, and increases by 0.2 for each year in which firm i has a CEO from an alumni network from which bank j has an executive after the election, and decreases for each year in which firm i and bank j are connected to the same alumni network before the election. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.