Capital Gains Taxes and Real Corporate Investment*

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Abstract

This paper assesses the effects of capital gains taxes on investment by exploiting a unique institutional setting in Korea, where the capital gains tax rates vary by firm size. I use a difference-in-differences design that compares the outcomes of firms whose tax rates were reduced, due to an unanticipated reform in 2014, to the outcomes of unaffected firms. I find that firms whose capital gains tax rates dropped from 24 percent to 10 percent experienced an average increase in market value of 16 percent compared to unaffected firms. I estimate that these firms increased investment by 51 percent, with the implied elasticity of 2.8 with respect to the net of tax rate, and increased newly issued equity by 4 cents per dollar of lagged revenue. The effects of the tax cut were larger for firms that appeared more cash-constrained, suggesting that these firms faced a higher marginal cost of investment, and for firms that appeared to have more agency conflicts. Taken together, the findings are 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.

JEL Codes: G11, G32, H25, O16.
Keywords: Investment Decisions, Capital and Ownership Structure, Business Taxes and Subsidies, Saving and Capital Investment.

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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.[1] 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.[2] A traditional class of models, sometimes referred 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 has 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

[1] The 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 Ohrn (2018).

[2] One 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).

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 51 percent 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 percent 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, I find that the parallel trend assumption is satisfied 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 larger than the estimates in recent empirical findings on corporate taxes (Hassett and Hubbard 2002) and much larger than the estimates in Yagan (2015) based on dividend taxes. I propose two reasons to explain the magnitude of my estimates. First, the affected group in my analysis sample mostly consists of small firms that recently became big - a consideration which 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). 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 and demonstrate the importance of this channel, and I find that the elasticity estimates are much larger for firms whose CEOs own a lower fraction of the firms' stock.

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 stocks 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 stocks 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 stocks 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 capital stocks is larger than the optimal level of investment at the steady-state, yielding a higher percentage change in investment in response to lower capital gains tax rates.

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 the effects may appear small if we do not account for the underlying ownership structure of firms.

The remainder of the paper is organized as follows. Section 2 describes the institutional background for the capital tax system in Korea. In Section 3, I discuss the theoretical framework to derive empirical predictions of the tax effects. I describe empirical strategy and data in Section 4. In Section 5, I present results and discuss economic interpretation and policy implications of my findings in Section 6. Section 7 concludes.

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.

2.1 Capital and Payout Taxes

In general, a firm faces two main types of capital taxes, explicitly and implicitly: (1) corporate income tax on profits, and (2) payout taxes. The former tax is based on net profits generated by a firm in a given year, and the firm explicitly pays the tax. 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.[3]

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

[3] In 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).

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.

2.2 Capital Gains Tax Rates in Korea

From 2005 to 2017, capital gains tax rates differed depending mainly on firm size.[4] An investor in a large firm faces a capital gains tax rate of 24 percent on average.[5] 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 concerning 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 the tax rate within a given firm affected by the rule change. To identify the effects of capital gains taxes on corporate outcomes, I compare the corporate 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.

2.3 Rules for 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) total assets below 500 million

[4]In 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.

[5]An 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.

dollars.[6] 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 (= $\frac{1000\times1+10\times99}{100}$). 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.[7]

To figure out which of the four criteria is most binding for firm size, I first examine the conditional probabilities, as shown in Table A.1.3 in Appendix A.1.3. 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 including or excluding these firms as part of the treated group does not affect my results (see Section 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 August 6, 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

[6] Top 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% and 91% of the entire sample of publicly listed and private firms, respectively.

[7] The 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.1.3).

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 5). I describe how I use this reform for identification in Section 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.2.1.

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.2.

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.

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) pay out dividends, D, (2) do share repurchases, R, 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.[8] In period 2, the firm generates net profits $f(I)$, where f is strictly concave. The firm then returns its profits and principal to shareholders by selling their entire stock to a different set of investors. 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 $D = 0$.[9] The manager can also buy a government bond that yields a fixed, untaxed interest rate of $r > 0$.[10] 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:

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

[8]Firms 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.

[9]Based 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, effective dividend tax rates are higher than effective capital gains tax rates 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 B.

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

3.2 Model Predictions

3.2.1 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.[11] Then we have the following first order condition:

$$\frac{\partial V}{\partial E}(R=0) = \frac{(1-\tau_g)(1-\tau_c)f'(C) - r}{1+r} \leq 0 \tag{2}$$

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

$$(1-\tau_c)f'(C-R^*) = r \tag{3}$$

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.

3.2.2 The "Old View" Prediction

The "Old View" considers firms that have retained profits 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:

$$(1-\tau_g)(1-\tau_c)f'(C) < r \implies E^* = 0 \tag{4}$$

$$(1-\tau_g)(1-\tau_c)f'(C) \geq r \implies (1-\tau_g)(1-\tau_c)f'(C+E^*) = r \tag{5}$$

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.

[11] If 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 $\frac{\tau_g r}{1+r}$.

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:

$$y_{it} = \alpha + \theta Small_{it} + \mathbf{X_{it}}\beta + \alpha_t + \alpha_i + \epsilon_{it} \tag{6}$$

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 estimation 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.[12] In public finance literature, one would use this tax notch to estimate the bunching in order to compute the corresponding elasticity (Kleven and

[12] Panel A of Figure A.1.3.2 in Appendix A.1.3 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.1.3.2 illustrates firm density around the revenue cutoff, conditional that the firms are below the other thresholds. I use log revenues of 0.05 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.

Waseem 2013; Bachas and Soto 2018). However, to back out the investment elasticity from the labor elasticity or revenue elasticity 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.

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 in 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 tax rate that was 14 percentage points lower.[13] 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

[13]The 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.2.1.

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.[14] 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.[15] 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 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 D, along with the analysis where I combine both types of treated firms. Figure 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:

$$y_{it} = \sum_{\tau=2009}^{2017} \theta_\tau \mathbb{1}[t = \tau] \times Treated_i + \alpha_i + \alpha_t + X_{it}\beta + \epsilon_{it} \tag{7}$$

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

[14] I 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.

[15] Firms 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 quantitively similar to dropping firms in the range of 1 to 10 percent right above this new cutoff.

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 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:

$$y_{it} = \alpha + \theta Treated_i \times Post_t + \alpha_i + \alpha_t + X_{it}\beta + \epsilon_{it} \tag{8}$$

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 (7). I report the estimates from this equation (8), as well as those from equation (7) in Section 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 D.

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, I show that the parallel trend assumption is satisfied 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.

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 economy. 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 D).

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 firm size.

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% share, foreign stock share, board members and their characteristics.

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 physical assets (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.3, C and D. 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 D).[16]

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

[16] By 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).

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.

5 Results

This section shows the results from the estimation of the difference-in-differences models in Section 4, and presents additional tests supporting the interpretations of the results.

5.1 Investment and Capital Structure

Panels A and B of Figure 2 plot the raw means of log(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 that 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 3 plots the coefficients θ_τ, where $\tau \in (2009, ..., 2017)$, for log(investment) as in equation (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:

$$\epsilon_{y,1-\tau} = \frac{\%\Delta y}{\%\Delta(net\ of\ tax\ rate)} = \frac{\Delta y}{y_0} * \frac{(1-\tau_0)}{\Delta\tau} \tag{9}$$

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 3 plots the coefficients θ_τ, where $\tau \in (2009, ..., 2017)$, 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 gains tax rates affect equity issuances and investment, without affecting corporate payouts.

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 combing 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 4 plots the coefficients θ_τ, where $\tau \in (2009, ..., 2017)$, for log(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.

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.[17] 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 (7) and (8), replacing the outcome variable as log(Stock Prices) and log(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 5 plots the coefficients on the $Treated \times Time$ dummies for *ln*(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 the tax cut experienced an increase in their market value. Moreover, these results confirm that the reform was unexpected, as the stock prices reacted only after the reform date.

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 quartics 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

[17] In 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.

validity of my results, and show heterogeneity tests to uncover potential mechanisms behind these findings.

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 (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 D.

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 D).

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. Comparing changes in investment for firms that bunched at the old cutoffs to the control firms after the cutoffs are removed to back out the investment elasticity is comparable to using the bunching strategy to estimate the investment responses to changes in tax rates 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 that crossed the old thresholds did so either 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 D).

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 D.

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 trough 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 C.2. 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.

5.4.1 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).[18] Then I estimate the following triple-difference model, where I interact the dummy with the difference-in-differences model (8):

$$y_{it} = \alpha + \theta_1 Treated_i \times Post_t + \theta_2 Treat_i \times Post_t \times CR_i + \theta_3 CR_i \times Post_t + \alpha_i + \alpha_t + X_{it}\beta + \epsilon_{it} \tag{10}$$

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 (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.[19]

Panel A of Figure 6 plots the coefficients on the $Treated \times Time$ dummies for ln(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 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.

[18] As 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 similiar whether I use different percentiles (i.e. tercile) for the dummy variable.

[19] Popular 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 C.1.

Table 7 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 cash constrained firms may face a relatively higher marginal cost of investment, and may react more aggressively to lower capital gains taxes than do cash-rich firms.

5.4.2 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 CEO with high ownership rate has already invested at a level closer to the optimal, his investment response will be smaller than the CEO with a low share.[20] Therefore, this model predicts

[20]This 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 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.

that CEOs with low ownership will increase investment after a tax reduction more than will CEOs with a larger share, regardless of whether firms are cash constrained. I formalize this intuition and describe the details of the model in Appendix B.2.

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

$$y_{it} = \alpha + \theta_1 Treated_i \times Post_t + \theta_2 Treat_i \times Post_t \times CEO_i + \theta_3 CEO_i \times Post_t + \alpha_i + \alpha_t + X_{it}\beta + \epsilon_{it} \tag{11}$$

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 (7).[21] 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 7 plots the coefficients on the $Treated \times Time$ dummies for ln(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 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.

Table 8 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).

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

[21] In my analysis sample, low-ownership CEOs have 1.5 percent of their firms' stock on average, while high-ownership CEOs have 22 percent on average. I also find qualitatively similar results when using different cuts (i.e. tercile) of CEO ownership rates.

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 E.1 of Appendix E. 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 E.2 of Appendix E. 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.

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.

6.1 Economic Interpretation

The hypothesis that a capital tax cut can 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

$$C_K = \frac{r}{(1-\tau_c)[(1-\tau_d)\rho + (1-\tau_g)(1-\rho)]} \tag{12}$$

where r is the interest rate, 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.[22] 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.[23] 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 near the upper range of the estimates in recent empirical findings on corporate taxes and much larger than the estimates in Yagan (2015) based on dividend taxes. I propose two main reasons to explain the magnitudes of my estimates. First, firms in my analysis sample are either small firms or firms that recently became big. 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). Therefore, 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 and CEO incentives. This paper is one of the few studies to empirically test and demonstrate the importance of this channel, where I find that the investment elasticity is much larger for firms with low ownership CEOs.[24] This finding suggests that a change in capital gains tax rates might be much more binding for firms whose CEOs do not have long-term interests on firm growths. On the other hand, 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 gains tax cut.[25]

[22]I 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.

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

[24]A 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.

[25]Alternatively, firms whose CEOs own a larger fraction of firms' stock may face an off-setting incentive

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 B.4, 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 (12), is a function of the interest rate, 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 we increase the keep rate by one percent, the capital stock would increase at least by one percent.[26]

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 D). 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 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.[27]

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: $\epsilon_{I,1-\tau_g} = \frac{1}{\delta}\epsilon_{K,1-\tau_g}$. 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

to increase share buybacks after a capital gains tax cut (Chetty and Saez 2010).

[26] The 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 $\epsilon \in (-10, -1)$.

[27] Another 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.

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.

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 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.[28] 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.[29] 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 the underlying ownership structure of firms.

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 determining firm size for tax purposes. The reform was unanticipated and was implemented six 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. Also, CEOs' stock share may not be the only incentive that can influence firms' reactions to a capital tax cut. For example, various measures of corporate governance,

[28]This 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).

[29]The 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).

such as board independence and monitoring, may potentially explain the heterogeneity in investment responses to a tax cut.

Despite these cautionary remarks, findings in this paper are significant and provide concrete evidence that capital gains taxes affect firms' investment, market value, and equity issuances. Another important policy implication from this paper is that policymakers may benefit from incorporating both capital structure and ownership structure of firms when designing an optimal capital tax policy.

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, which predicts that lowering capital tax rates spurs equity-financed investment by increasing the marginal returns on 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 mechanism 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.

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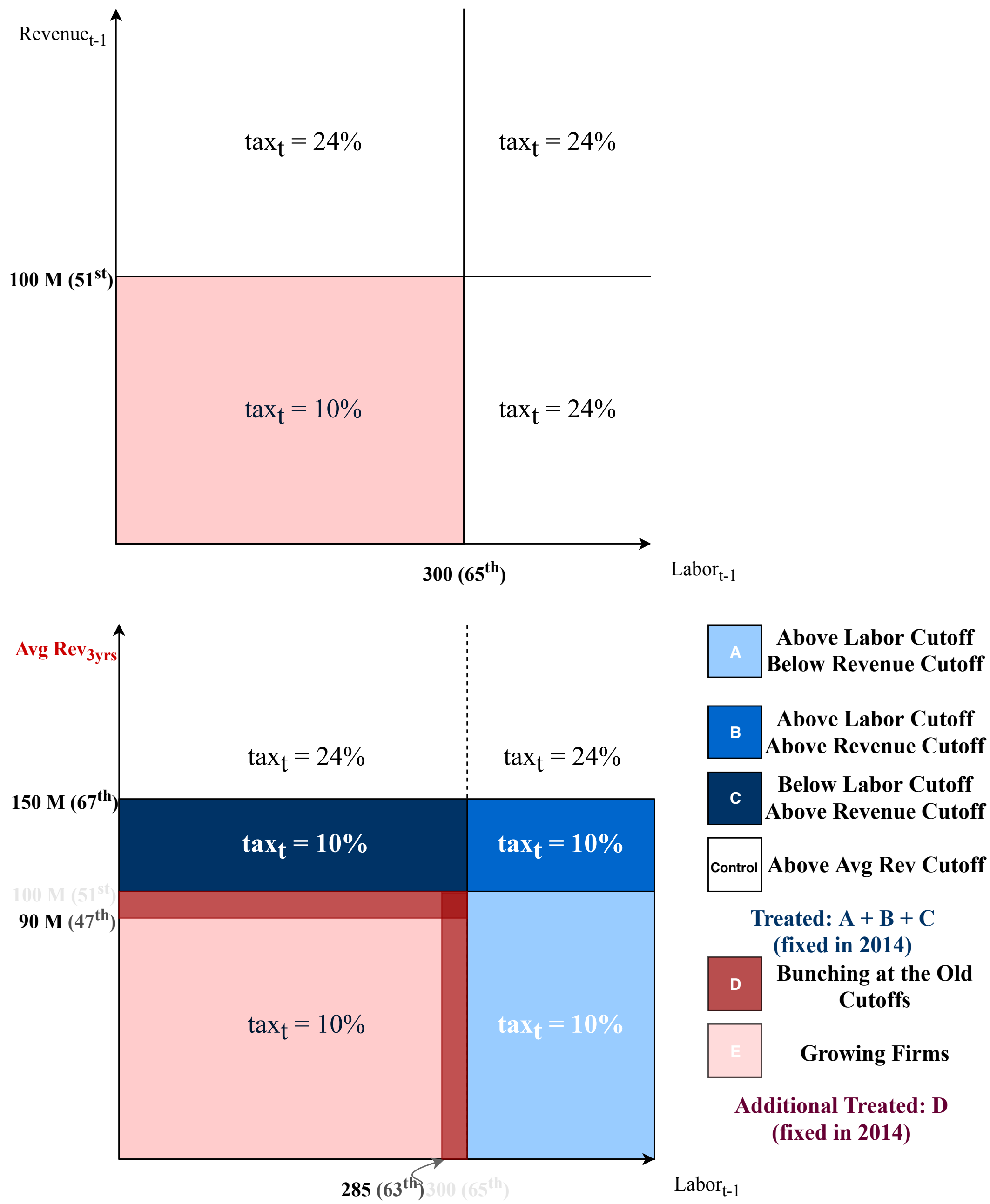
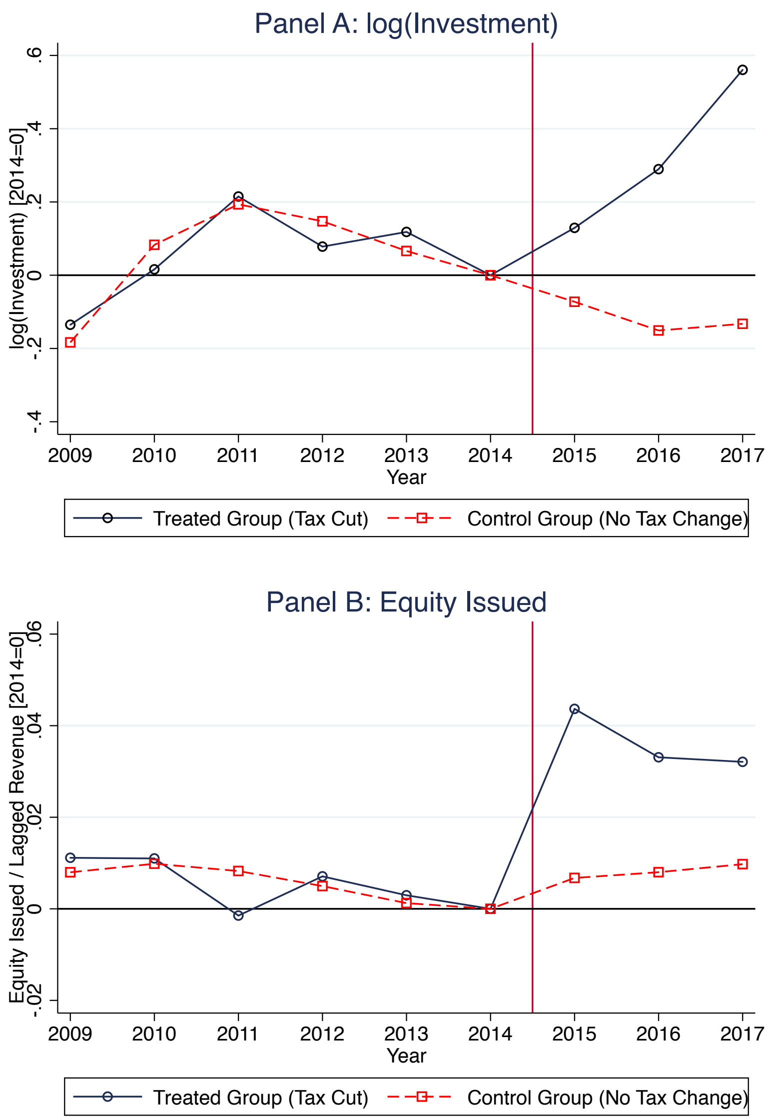
Figure 1: Policy Reform 2014 and Treated vs. Control Groups



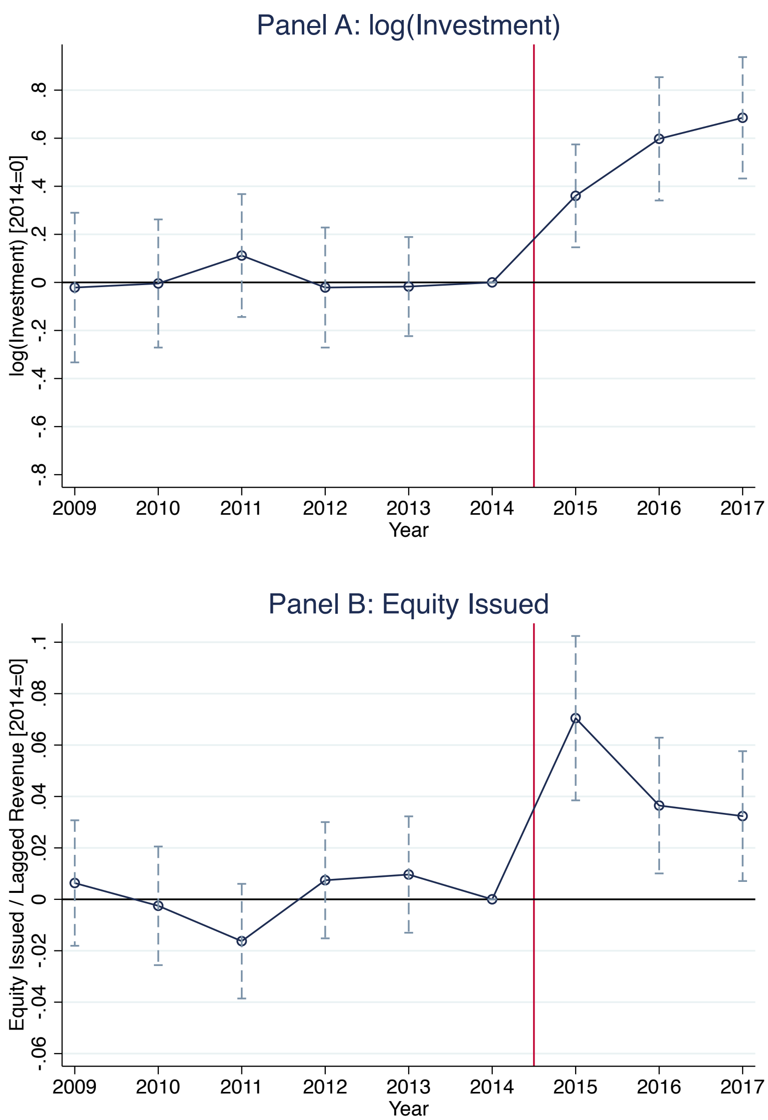
Notes: This figure illustrates how the reform in 2014 assigned firms into treated (tax cut) or control groups. The first figure on the top shows the initial rule on firm size prior to the reform, where firms in the pink area are jointly below labor and revenue threshold at time $t-1$ and face a tax rate 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 D. 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 2: Raw Means of Corporate Outcomes of Affected and Unaffected Firms



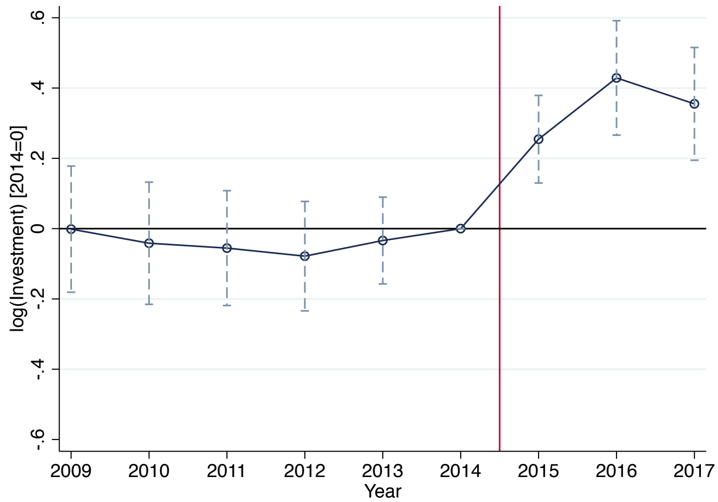
Notes: Panel A in this figure shows raw means of *ln*(investment) of treated (solid) and control (dash) firms in each year, from 2009 to 2017. Panel B shows raw means of equity issuance, defined as non-negative annual changes in total paid-in capital, scaled by lagged revenues. Means of each outcome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

Figure 3: Tax Effects on Real Outcomes of Affected and Unaffected Firms



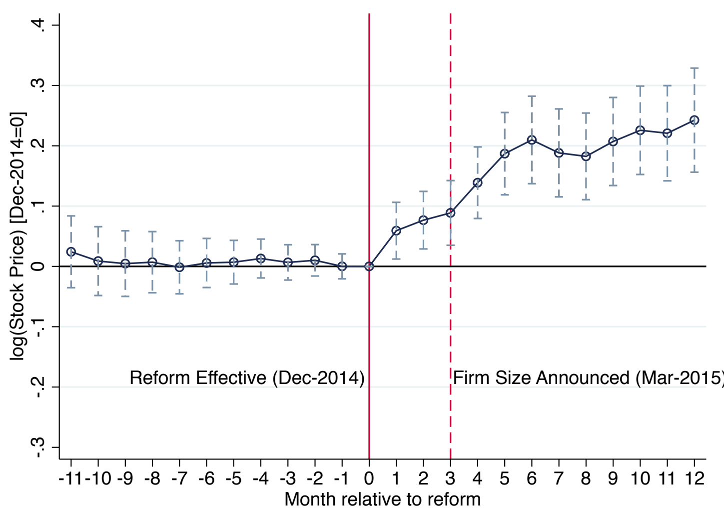
Notes: Panel A in this figure shows the coefficients on $Treated \times Time$ for firms' investment, defined as log(expenditures on physical capital assets), in equation (7). The dashed lines indicate 95% confidence intervals for these coefficient estimates. The solid vertical line indicates the reform year. The same graph with two additional lines (without any controls or with only basic controls) are included in Appendix D. 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 (7).

Figure 4: Tax Effects on *ln*(Investment): Listed and Private Firms



Notes: This figure shows the coefficients on *Treated* × *Time* for firms' investment, defined as log(expenditures on physical capital assets), in equation (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 5: Tax Effects on *ln*(Stock Prices): Listed Firms

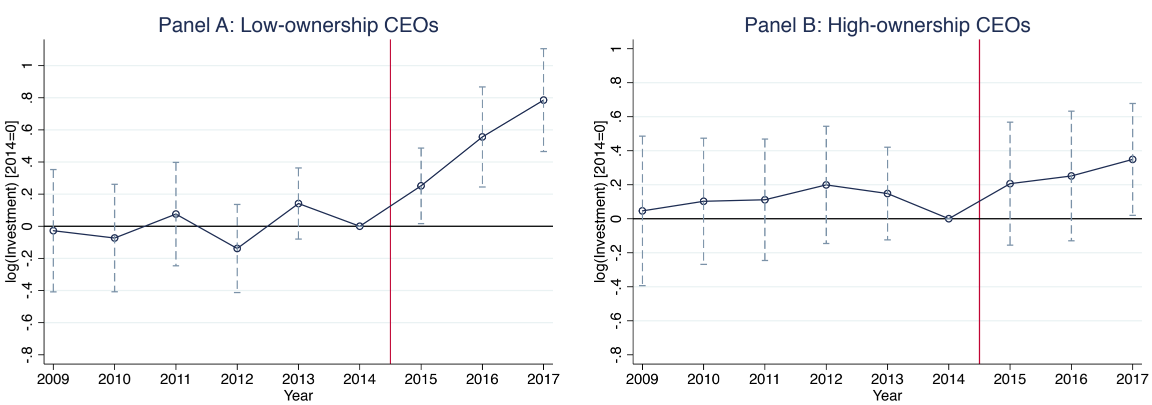


Notes: This figure shows the coefficients on *Treated* × *Time* for each time period (month). The outcome variable is log(Stock Prices). The dashed lines indicate the 95% confidence intervals for those coefficient estimates. The solid vertical line indicates the month in which the reform was implemented, 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 6: Tax Effects on *ln*(Investment) by Cash Constraints

Notes: Panel A in this figure shows the coefficients on *Treated* × *Time* for ln(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* × *Time* for ln(Investment) of cash-rich firms.

Figure 7: Tax Effects on *ln*(Investment) by CEO Ownership



Notes: Panel A in this figure shows the coefficients on *Treated* × *Time* for ln(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* × *Time* for ln(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 millon)	74.69	251.8	95.13	284.4	67.10	226.6
	(45.16)	(176.5)	(44.34)	(189.3)	(43.06)	(161.5)
Labor (Average Employees used)	243.1	393.4	254.3	467.5	238.9	335.9
	(174.5)	(295.1)	(135.5)	(300.8)	(186.9)	(277.3)
Total Asset (in million)	75.02	237.3	123.1	305.2	57.16	184.7
	(64.65)	(189.4)	(68.29)	(201.2)	(53.15)	(161.1)
Total Capital (in million)	40.50	123.1	74.52	165.8	27.86	89.99
	(41.94)	(109.1)	(46.68)	(113.0)	(31.83)	(93.43)
CAPEX (in million)	4.067	11.22	5.763	13.34	3.436	9.580
	(6.086)	(13.83)	(7.647)	(14.67)	(5.257)	(12.92)
CAPEX / lagged PPE	0.219	0.202	0.186	0.180	0.231	0.219
	(0.264)	(0.234)	(0.224)	(0.209)	(0.277)	(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 4.

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

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payouts	Share Buybacks
Treated x Post	0.511***	0.044***	0.002	0.001
	(0.110)	(0.011)	(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 (8). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, 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. 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)	(2)	(3)
	ln(CAPEX)	ln(CAPEX)	ln(CAPEX)
Treated x Post	0.356***	0.511***	0.239***
	(0.068)	(0.110)	(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 (8). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, 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. 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 and Firm Value (Publicly Listed Firm)

	Stock Prices		Price to Book Value	
	(1)	(2)	(3)	(4)
	log(Price)	log(Price)	Firm Value	Firm Value
Treated x Post	0.118***	0.160***	0.112***	0.111**
	(0.034)	(0.041)	(0.041)	(0.046)
Basic Controls	Yes	Yes	Yes	Yes
Profit Quintile x Time FE	No	Yes	No	Yes
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	1.45	1.45	0.29	0.29
Implied Elasticity wrt (1-tau)	0.64	0.87	0.61	0.60
R-squared	0.96	0.96	0.87	0.87
Observations (firm-months)	12405	12405	12381	12381
Clusters (firms)	543	543	543	543

Notes: This table reports the tax effects on stock prices and firm value based on a variation of specification (8). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, 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). 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. 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***	0.066***	0.002	0.001
	(0.144)	(0.019)	(0.001)	(0.001)
Treated x Post x Cash-Rich	-0.553***	-0.044**	-0.001	-0.001
	(0.205)	(0.022)	(0.002)	(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 (10). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, 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). The dummy for CR_i is 1 if the firm is cash-rich firm, as defined in Section 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)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payout	Share Buyback
Treated x Post	0.532***	0.075***	0.002	0.001
	(0.131)	(0.018)	(0.001)	(0.001)
Treated x Post x CEO	-0.405**	-0.068***	-0.004	0.000
	(0.203)	(0.020)	(0.003)	(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 (11) The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, 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). The dummy for CEO_i is 1 if the firm's CEO has stock shares above the median, as defined in Section 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.

Appendix Materials

A Institutional Details

In Appendix A, I provide further institutional details regarding corporate tax rates and firm size regulations in Korea. In Appendix A.1, I describe historical corporate income tax rates on profits and dividend tax rates. In Appendix A.2, I give more institutional details on the firm-size regulations and the policy reform in 2014. In Appendix A.3, I describe additional tax benefits associated with being a small firm and show a set of tests to argue that my main results are not driven by these extraneous benefits. Lastly, in Appendix A.4, I show how capital gains taxes affect largest shareholders' ownership rates by analyzing an earlier policy reform in 2005, as a further support for the salience and importance of capital gains taxes.

A.1 Corporate and Capital Tax Rates in Korea

A.1.1 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 (See Appendix D).

A.1.2 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.

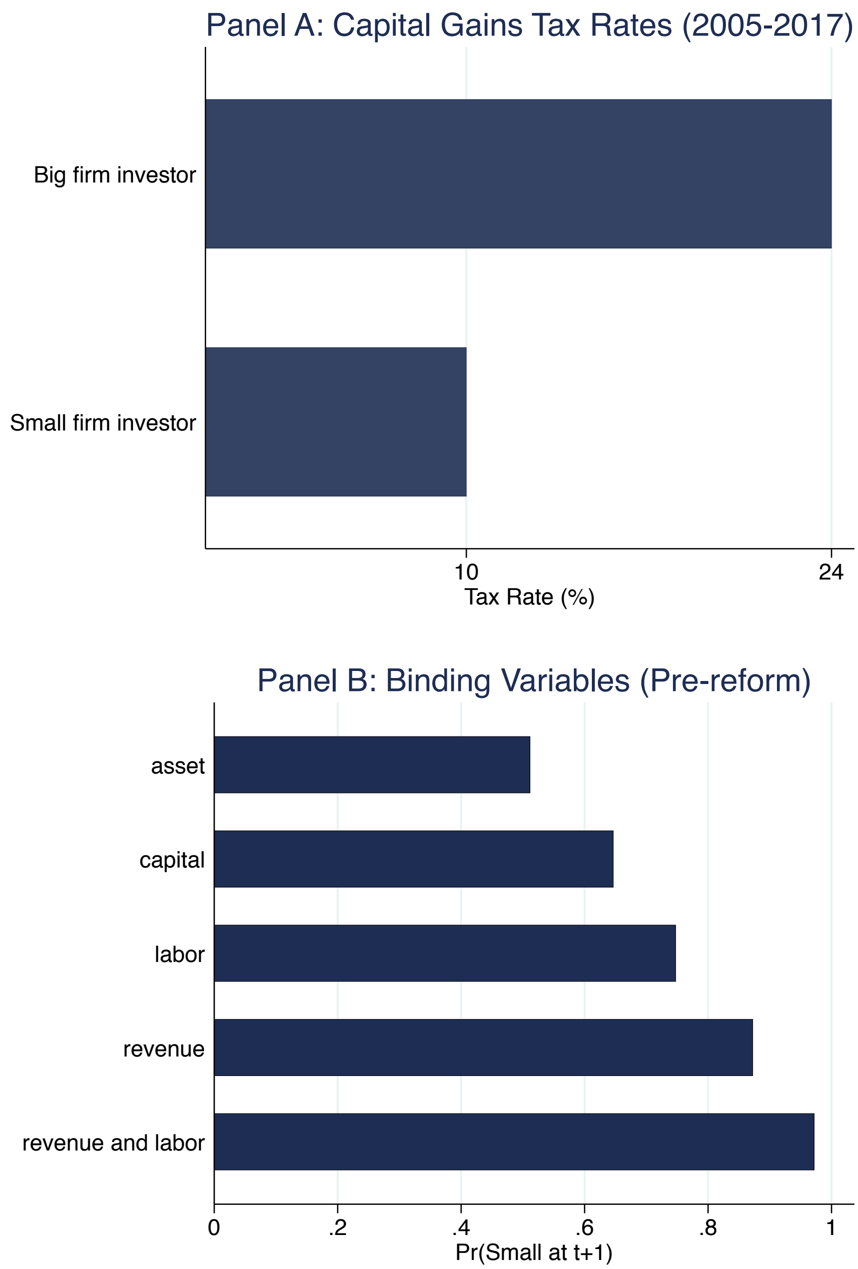
A.1.3 Binding Variables and Conditional Density

Table A.1.3: Conditional Probability Matrix for each running variable

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

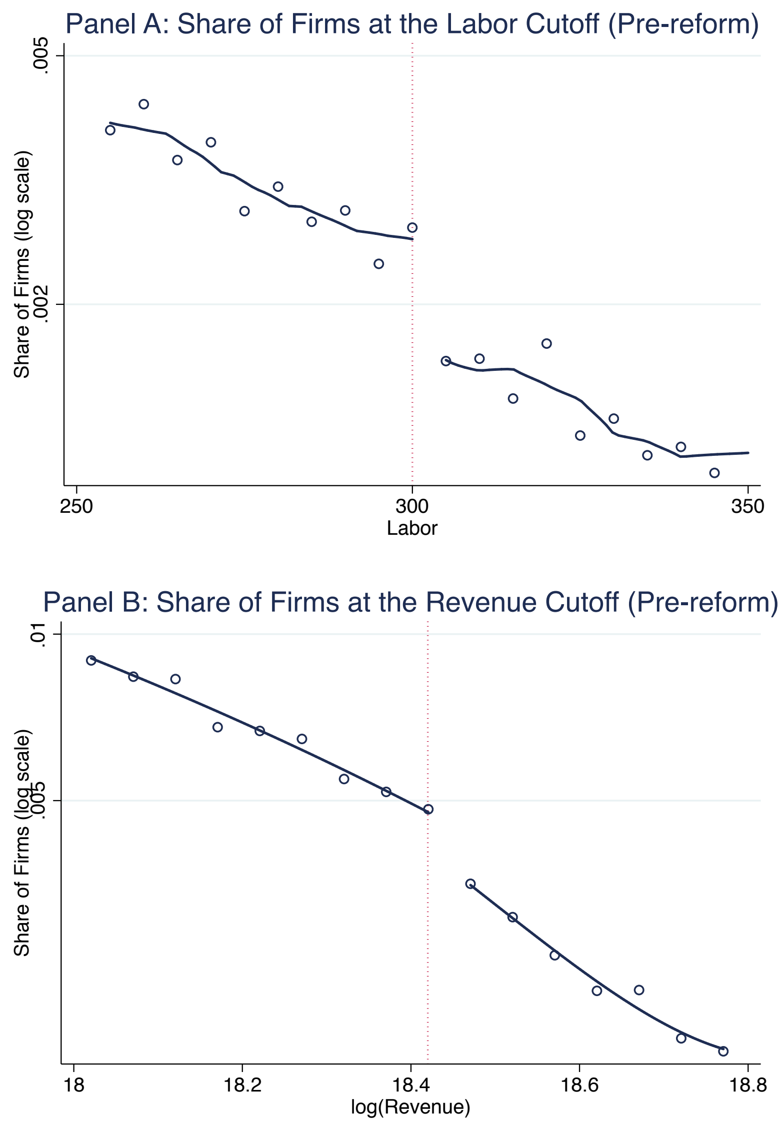
Notes: This table reports the conditional probability matrix for each of the running variable determining firm size prior to the reform 2014. Each row represents the probability of a running variable being below its own threshold conditional on the other running variable is either below or above its 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.1.3.1: 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.1.3.2: 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 0.05 log revenues. The McCrary (2008) test rejects the null that the coefficient at the jump is statistically not different from zero.

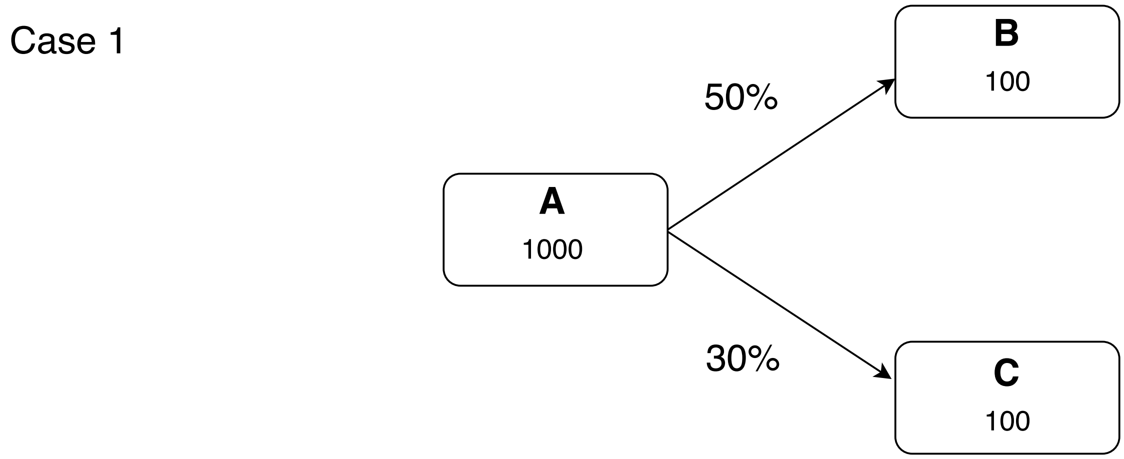
A.2 How to Account for Subsidiaries for Tax Purposes

Table A.2: Computing Accounting Variables for Tax Purposes

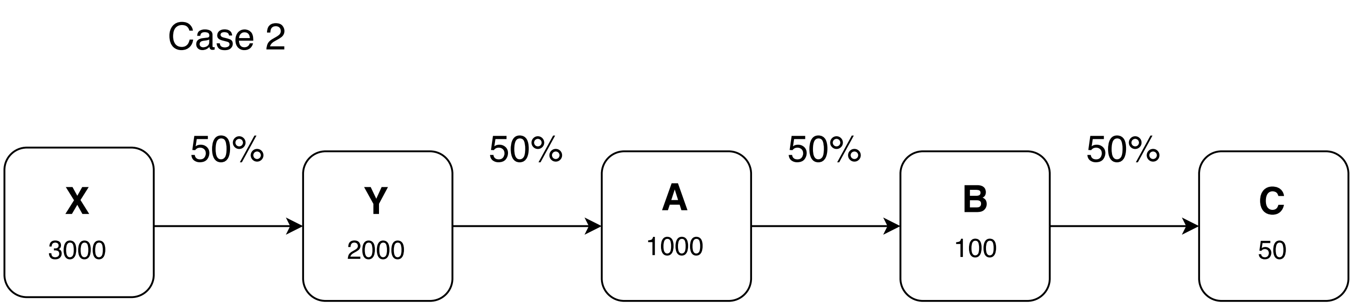
Firm	Relationship	Labor	Ownership	Labor Size for Tax Purposes
Case 1				
A	Parent to B & C	1000	-	$1000 + (1.0) * 100 + (0.3) * 100 = 1130$
B	Subsidiary to A	100	50%	$100 + (1.0) * 1000 = 1100$
C	Subsidiary to A	100	30%	$100 + (0.3) * 1000 = 400$
Case 2				
X	Parent to Y	3000	-	$3000 + (1.0) * 2000 +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



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×(100% of firm A)+100×(100% of firm B)+100×(30% of firm C)=1130
(2) firm B: 1000×(100% of firm A)+100×(100% of firm B)=1100
(3) firm C: 1000×(30% of firm A)+100×(100% of firm C)=400



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: $2000 + (1.0) * 3000 + (1.0) * 1000 + (0.5) * 100 = 6050$
(3) firm A: $1000 + (0.5) * 3000 + (1.0) * 2000 + (1.0) * 100 + (0.5) * 50 = 4625$
(4) firm B: $100 + (0.5) * 2000 + (1.0) * 1000 + (1.0) * 50 = 2150$
(5) firm C: $50 + (0.5) * 1000 + (1.0) * 100 = 650$

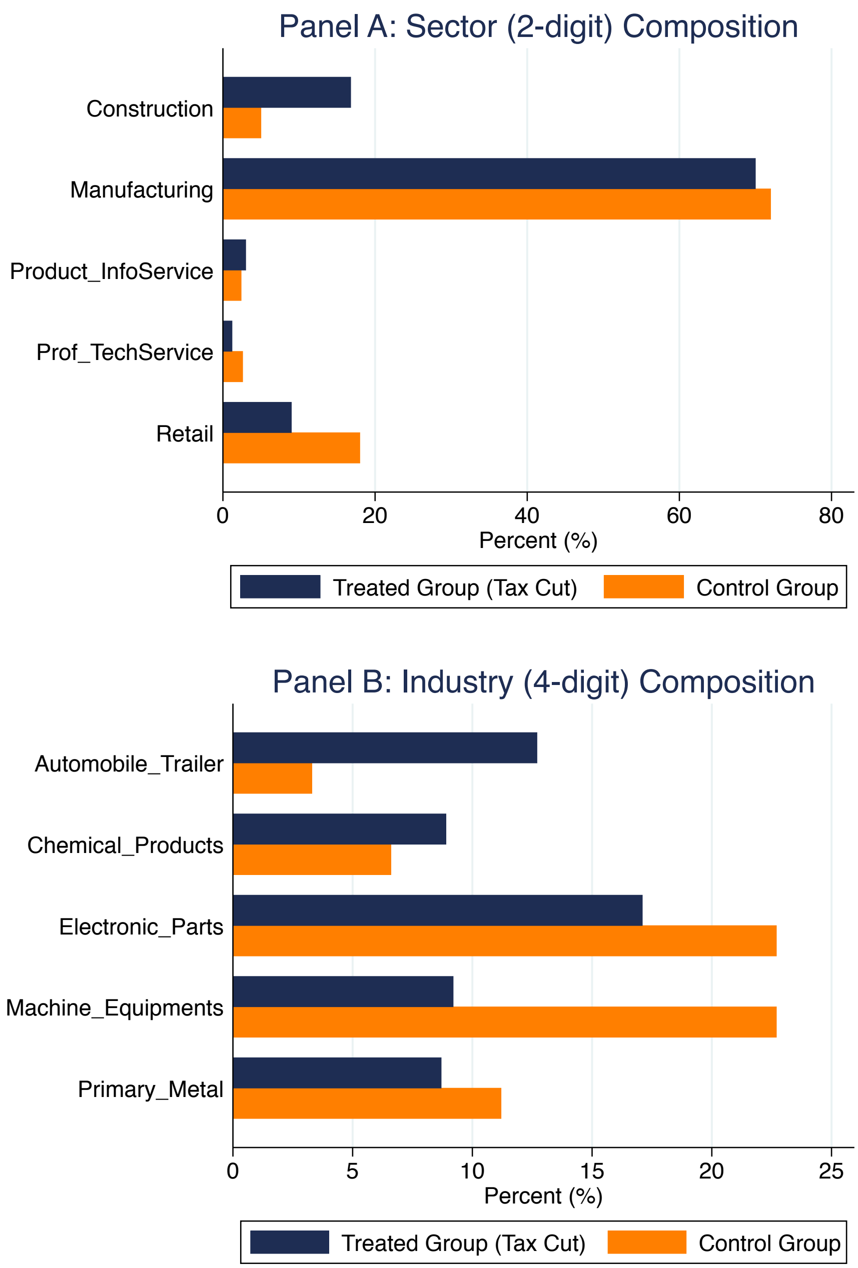
A.2.1 Reform Effects Across Industries

Table A.2.1: 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.2.1: 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.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.

A.3.1 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.[30] 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 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 A.3.1 shows the main results in Section 5, where I interact the dummy for $Treated \times Post$ with the dummy for whether a firms is located in a rural area (which is fixed at the time of the reform and accounts for roughly 35% of the analysis ample). 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.

[30] In 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.

A.3.2 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.[31] 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% 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 A.3.2 shows the main results in Section 5, where I interact the dummy for *Treated* × *Post* with the dummy for whether a firms 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.[32]

A.3.3 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, 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.

[31] R&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.

[32] One 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 decreases 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.

Table A.3.1: Main Results by Firms' Location

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payout	Share Buyback
Treated x Post	0.483***	0.047***	0.001	0.001
	(0.135)	(0.014)	(0.001)	(0.001)
Treated x Post x Rural	-0.193	-0.004	0.001	0.001
	(0.205)	(0.025)	(0.002)	(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 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.3.1). 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 A.3.2: Main Results by Firms' R&D Expenditures

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payout	Share Buyback
Treated x Post	0.429***	0.064***	0.002	0.000
	(0.140)	(0.016)	(0.001)	(0.001)
Treated x Post x RD Intensive	-0.070	-0.054***	-0.000	0.001
	(0.199)	(0.020)	(0.002)	(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 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.3.2). 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.

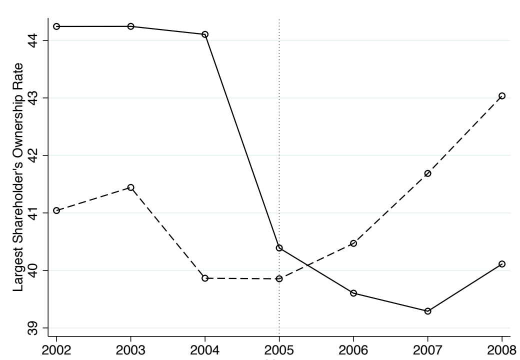
A.4 Tax Effects on Largest Shareholders' Ownership Rates

In this section, I show evidence for the salience of capital gains taxes on investors by looking at how exogenous changes in the tax rate affects the ownership structure of the firm using the 2005 tax reform as a natural experiment. From 2001 to 2004, the capital gains tax rate for the largest shareholder of a small firm was 11% if he owns less than 50% of the shares, and 11.5 % if he owns more than 50%. In 2005, the government equalized the largest shareholders' and other investors' capital gains tax rate for small firms, reducing the top tax rate by up to 1.5 percentage points.

If the largest shareholders, were either unaware of such tax reforms or insensitive to such changes in tax rates, then we might observe no changes in the ownership structure assuming there were no other policy reforms. On the other hand, if the ownership elasticity with respect to tax rates is high, then we might see either a significant drop or increase in the ownership rate following the tax cut. In this case, the largest shareholder will increase the ownership rate after a tax cut if he believes that he will continue to invest on his firm and the firm size will not change in the near future. On the other hand, the largest shareholder will sell his stock right after the tax cut if he believes that the firm may grow in size (which will increase the average tax rate) without any future increases in returns. Either way, a significant change in the ownership rate implies that the capital gains tax is salient and burdensome for the largest shareholder.

To estimate the tax effects on the ownership rate, I estimate a difference-in-differences model using large firms unaffected by the reform as a control group. Figure A.4 plots time-series ownership rates for both small and large firms, prior and post the reform year. The largest shareholder's ownership rate drops by around 4 percentage points (with t-stats of -5.64), which is statistically significant. Therefore, this result suggests that capital gains taxes are salient and burdensome for the largest shareholders.

Figure A.4: Effects of Tax Reduction for Small Firms on Largest Shareholders' Ownership



Notes: This graph shows the impacts of the tax rate change in 2005 on the largest shareholder's ownership rate. The solid line indicates the largest shareholder's ownership rate on average for small firms (treated). The dash line indicates the one for large firms (control).

B Model Extensions

In this section, I extend the two-period investment model in Section 3 to incorporate (1) dividend payouts and (2) agency conflicts, separately. Then I discuss both theoretical and empirical implications of these extended models.

B.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 dividend tax rate, τ_d. The neoclassical model in Section 3 can be extended in the following way:

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

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.

B.2 Extended Model with Agency Conflicts

I extend the model in Section 3 by incorporating agency conflicts (Shleifer and Vishny 1986; Chetty and Saez 2010).[33] The main source of departure from the model in Section 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,

[33] I 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.

the manager's problem is:

$$Max\ V^M = \underbrace{\alpha_M}_{\text{CEO share}} \{\underbrace{(1-\tau_g)R - E}_{\text{period 1 cash flow}} + \overbrace{\frac{(1-\tau_g)[(1-\tau_c)f(I)+C-R]+E}{1+r}}^{\text{net-of-tax return to shareholders}}\} + \overbrace{\frac{g(J)}{(1+r)}}^{\text{private returns}}$$

$$= \underbrace{\omega\{R - \frac{E}{(1-\tau_g)} + \frac{[(1-\tau_c)f(I)+C-R]+\frac{E}{(1-\tau_g)}}{1+r}\}}_{\text{firm's profit-maximization side}} + \underbrace{\frac{g(J)}{(1+r)}}_{\text{CEO's private utility}}$$

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.[34]

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" 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 privates 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 $\omega r = g'(C - I^* - R)$.[35] 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

[34] Examples 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.

[35] See Chetty and Saez (2010) for detailed proofs behind this.

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 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.

B.3 Model Prediction on the Magnitude

In this subsection, I use the model described in Section 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 (5):

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

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.

B.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 interest rate, capital gains tax rate, and corporate tax rate, as described in equation (12) in Section 6, with $\rho = 0$.

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

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

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:

$$y = AL^{\alpha_L}K^{\alpha_K} \tag{13}$$

$$w = \alpha_L \frac{y}{L} \tag{14}$$

$$r = \alpha_K(1-\tau_g)(1-\tau_c)\frac{y}{K} \tag{15}$$

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

$$C(y; w, r, \tau_g, \tau_c) = (\alpha_L + \alpha_K)\left[\frac{y}{A}\left(\frac{w}{\alpha_L}\right)^{\alpha_L}\left(\frac{r}{\alpha_K(1-\tau_g)(1-\tau_c)}\right)^{\alpha_K}\right]^{\frac{1}{\alpha_L+\alpha_K}} \tag{16}$$

$$MC(y; w, r, \tau_g, \tau_c) = \left[\frac{y^{1-\alpha_L-\alpha_K}}{A}\left(\frac{w}{\alpha_L}\right)^{\alpha_L}\left(\frac{r}{\alpha_K(1-\tau_g)(1-\tau_c)}\right)^{\alpha_K}\right]^{\frac{1}{\alpha_L+\alpha_K}} \tag{17}$$

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 $MR(y;\epsilon) = MC(y;w,r,\tau_g,\tau_c)$:

$$y = \left[\left(\frac{1}{\epsilon}+1\right)^{\alpha_L+\alpha_K} AD^{\alpha_L+\alpha_K}\left(\frac{\alpha_L}{w}\right)^{\alpha_L}\left(\frac{\alpha_K(1-\tau_g)(1-\tau_c)}{r}\right)^{\alpha_K}\right]^{\frac{1}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\epsilon}+1\right)}} \tag{18}$$

$$K = \left[\left(\frac{1}{\epsilon}+1\right) A^{(1+\frac{1}{\epsilon})} D\left(\frac{\alpha_L}{w}\right)^{\alpha_L\left(1+\frac{1}{\epsilon}\right)}\left(\frac{\alpha_K(1-\tau_g)(1-\tau_c)}{r}\right)^{\left(1-\alpha_L\left(1+\frac{1}{\epsilon}\right)\right)}\right]^{\frac{1}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\epsilon}+1\right)}} \tag{19}$$

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

$$\frac{\partial K^*}{\partial(1-\tau_g)} = \left(\frac{1-\alpha_L(1+\frac{1}{\epsilon})}{1-(\alpha_L+\alpha_K)\left(1+\frac{1}{\epsilon}\right)}\right)\frac{K^*}{(1-\tau)} \tag{20}$$

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

$$\frac{\partial K^*/K^*}{\partial(1-\tau_g)/(1-\tau_g)} = \frac{1-\alpha_L(1+\frac{1}{\epsilon})}{1-(\alpha_L+\alpha_K)\left(1+\frac{1}{\epsilon}\right)} \tag{21}$$

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:

$$\begin{aligned} dK = &\left[\left(\frac{1}{\eta}+1\right) A^{\left(\frac{1}{\eta}+1\right)} D\left(\frac{\alpha_L}{w}\right)^{\alpha_L\left(1+\frac{1}{\eta}\right)}\left(\frac{\alpha_K}{r}\right)^{\left(1-\alpha_L\left(1+\frac{1}{\eta}\right)\right)}\right]^{\frac{1}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\eta}+1\right)}} \\ &\left((1-\tau^*)^{\frac{\left(1-\alpha_L\left(1+\frac{1}{\eta}\right)\right)}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\eta}+1\right)}} - (1-\tau_0)^{\frac{\left(1-\alpha_L\left(1+\frac{1}{\eta}\right)\right)}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\eta}+1\right)}}\right) \end{aligned} \tag{22}$$

$$\frac{dK/K_0}{d(1-\tau_g)/(1-\tau_g)} = \left[\left(\frac{1-\tau^*}{1-\tau_0}\right)^{\frac{1-\alpha_L(1+\frac{1}{\epsilon})}{1-(\alpha_L+\alpha_K)\left(1+\frac{1}{\epsilon}\right)}} - 1\right] * \frac{1-\tau_0}{\tau_0-\tau^*} \tag{23}$$

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 (see Appendix D).

C Additional Heterogeneity Results

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

C.1 Additional Results by Firms' Cash-Constraints

In Section 5, I use retained earnings scaled by assets (averaged over three past years at the time of the reform) as a proxy for firms' financial constraint, and define that firms are financially 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 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-Zinagles (KZ) Index following Lamont, Polk and Saaa-Requejo (2001).[36] 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 5, substituting the dummy variable for Cash-Rich with this new indicator. Column (2) 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, 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 Hennessy and Whited (2007).[37] 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 5, substituting the dummy variable for Cash-Rich with this new indicator. Column (3) in Table C.1 shows the results based on this triple difference estimation. The difference-in-differences coefficient for investment is positive

[36] KZ 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.

[37] WW 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).

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).[38] 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 5, substituting the dummy variable for Cash-Rich with this new indicator. Column (4) 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, 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 C.1: 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***	0.367**	0.648***	0.554**
	(0.147)	(0.182)	(0.150)	(0.222)
Treated x Post x Cash-Rich	-0.500**	-0.204	-0.411**	-0.291
	(0.194)	(0.271)	(0.208)	(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.

[38] HP 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.

C.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 shock, 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* × *Post* with the indicator for the firm's share of long-term investors. Table C.2 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 C.2: Main Results by Ownership Structure (Publicly Listed Firms)

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payouts	Share Buybacks
Treated x Post	0.777***	0.022	0.002	-0.000
	(0.178)	(0.014)	(0.002)	(0.001)
Treated x Post x Long-term Investors	-0.522**	-0.027	-0.001	0.001
	(0.225)	(0.018)	(0.002)	(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 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.

D Robustness Checks

In Appendix D, I provide a set of robustness tests for the main results in Section 5.

D.1 Without Controls

I repeat the main analysis in equation (8), without basic or additional controls and with only basic controls. Table D.1.1 shows the main results without any basic or additional controls, and Table D.1.2 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 (8).

D.2 With Different Levels of Winsorizing

I repeat the main analysis using the same specification as in equation (8), winsorizing (top-coding) the main outcome variables at the 95% level , instead of at the 99% level. Table D.2 shows the main results based on this different level of winsorizing. The coefficient estimate for log(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.

D.3 Using Balanced Panel

I repeat the main analysis using the same specification as in equation (8), using a balanced panel. Table D.3 shows the main results based on using the balanced panel. The coefficient estimate for log(investment) is larger in the balanced sample, but the main results are qualitatively similar whether the sample is balanced or not.

D.4 Including Firms in Other Sectors

I repeat the main analysis using the same specification as in equation (8), including firms in other sectors. Table D.4 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(investment) remains similar when I include firms in other sectors.

D.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 D.5 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.

D.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 D.6 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.

D.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 groups × 9 years 2009-2017) 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 D.7 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.

D.8 Accounting for Potential Selection of Treated Firms

One potential concern that can undermine 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 D.8 shows the main results based on this subset of treated firms. The coefficient estimates are almost identical to the main estimates. Therefore, the results are robust to the potential selection of treated firms.

D.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 D.9 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.

D.10 Firms that were Growing

I repeat the main analysis on investment using firms that were growing below the bunching firms. Even though these growing 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 setting. 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 growing firms are potentially and indirectly treated by the reform.

To use these growing firms as additional treated groups, I first define that firms are 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 D.10 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 D.10 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.

D.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 D.11 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.

D.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 D.12 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.

D.13 Dropping Firms Above the Old Cutoffs

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 D.13 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.

D.14 Re-scaling Estimates Using an Instrumental Variable

As discussed in Section 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, $Treated_{i2015}$ is exogenously determined by the reform and highly correlated with treatment dummies in latter years. Table D.14 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.

D.15 Measuring Cash-constraints Using Different Cuts (Tercile)

In Section 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 D.15 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.

D.16 Measuring CEO Incentives Using Different Cuts (Tercile)

In Section 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 D.16 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.

Table D.1.1: Main Results Without Basic or Additional Controls

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payouts	Share Buybacks
Treated x Post	0.409***	0.046***	0.001	0.001
	(0.103)	(0.012)	(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

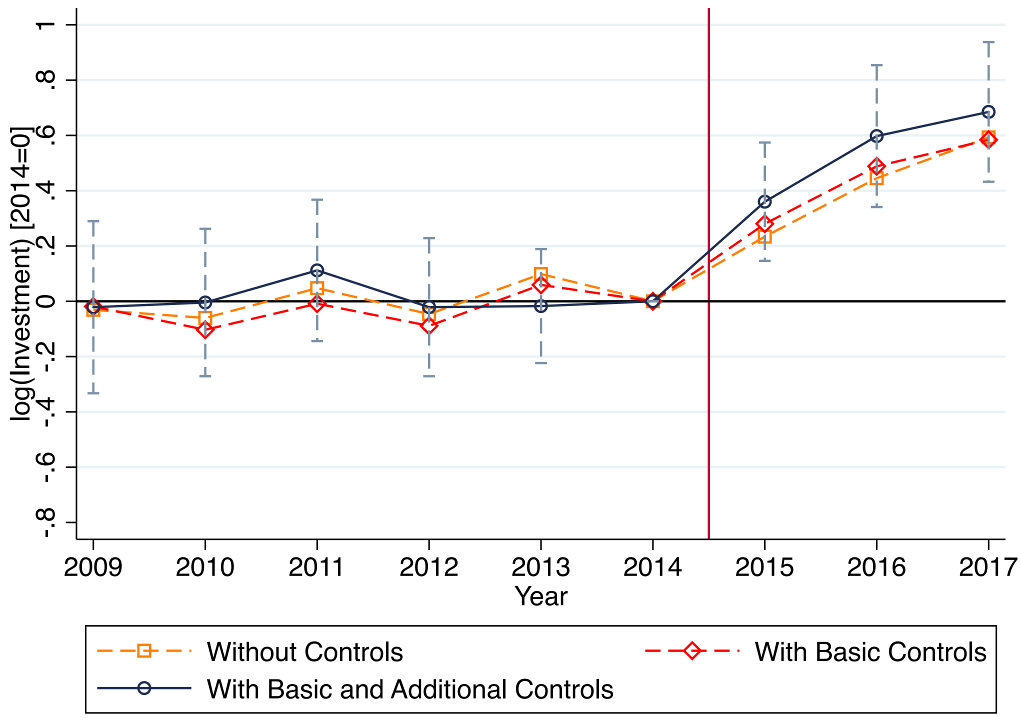
Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as log of expenditures 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 D.1.2: Main Results With Only Basic Controls

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payouts	Share Buybacks
Treated x Post	0.449***	0.046***	0.001	0.001
	(0.107)	(0.011)	(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 D.1: Tax Effects on Real Outcomes of Affected and Unaffected Firms



Notes: This figure shows the coefficients on *Treated* × *Time* for firms' investment, defined as log(expenditures on physical capital assets), in equation (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 (7) without any controls and the red dash line indicates those with only basic controls.

Table D.2: 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***	0.015***	0.002	0.001
	(0.110)	(0.004)	(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 D.3: 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***	0.023**	0.001	0.001
	(0.121)	(0.010)	(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 D.4: Main Results by Including Firms in Other Sectors

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payouts	Share Buybacks
Treated x Post	0.519***	0.046***	0.002	0.001
	(0.108)	(0.012)	(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 D.5: 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***	0.044**	0.015***	0.069***	0.103**
	(0.018)	(0.017)	(0.006)	(0.021)	(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 D.6: 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***	0.025***	0.002	0.001
	(0.125)	(0.008)	(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 D.7: 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***	0.033**	0.001	0.000
	(0.120)	(0.013)	(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)	2.43	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 D.8: Main Results with Accounting for Potential Selection of Treated Firms

	Investment	Capital Structure		
	(1) ln(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.511***	0.060***	0.002	0.001
	(0.129)	(0.013)	(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

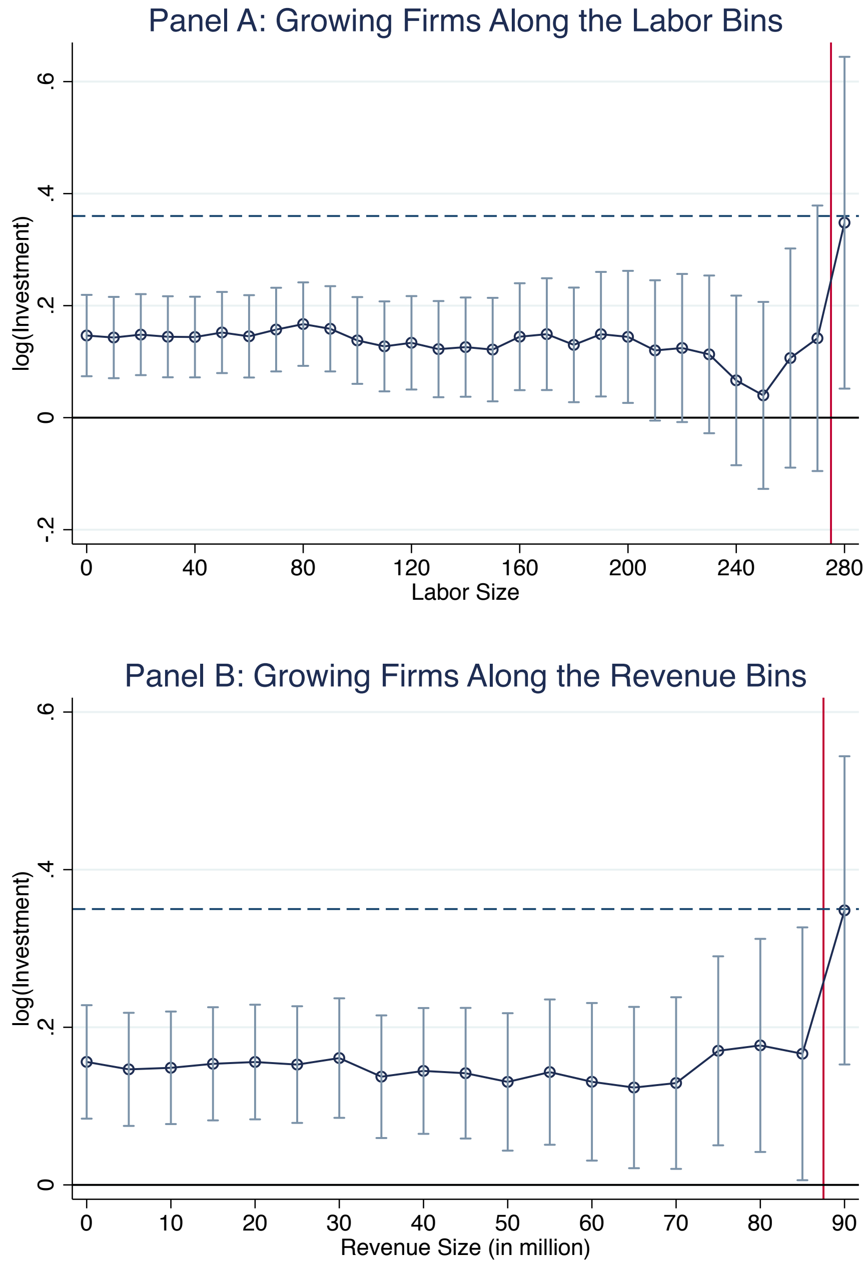
Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt (that passed any threshold in 2010), as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment 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 D.9: Main Results by Treated Firms with Either Tax Cut and/or Bunching

	Bunching Firms	Tax Cut Firms	Bunching and Tax Cut Firms
	(1) ln(CAPEX)	(2) ln(CAPEX)	(3) ln(CAPEX)
Treated x Post	0.388***	0.356***	0.361***
	(0.109)	(0.068)	(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 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 D.10: Coefficients on Treated x Post for *ln*(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 D.12). The outcome variable is the firm's investment, as defined by log(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 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 D.11: Placebo Test using earlier periods

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payouts	Share Buybacks
Treated x Post	0.138	0.004	0.002	0.000
	(0.096)	(0.005)	(0.001)	(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

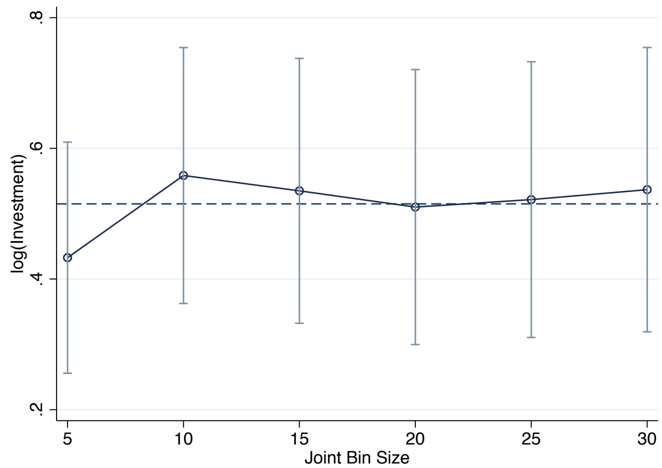
Notes: This table reports the tax effects on the main corporate outcomes as a placebo test. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 ppt, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the placebo year (2011). Investment 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 D.12: Placebo Test using random cutoffs

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payouts	Share Buybacks
Treated x Post	0.065	0.017	0.002	0.001
	(0.088)	(0.011)	(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 D.13: Dropping Treated Firms Right Above the New Cutoff



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 D.13). The outcome variable is the firm's investment, as defined by log(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 D.14: 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***	0.047***	0.002	0.001
	(0.120)	(0.012)	(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 7: Results on Investment and Capital Structure by Cash Constraints (Listed Firms)

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payouts	Share Buybacks
Treated x Post	0.660***	0.057*	0.001	0.000
	(0.194)	(0.030)	(0.001)	(0.001)
Treated x Post x Cash-Rich	-0.565**	-0.031	-0.001	-0.000
	(0.265)	(0.032)	(0.002)	(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 (10). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, 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). 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 8: Results on Investment and Capital Structure by CEO Ownership (Listed Firms)

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	ln(CAPEX)	Equity Issuance	Dividend Payout	Share Buyback
Treated x Post	0.526***	0.104***	0.004	0.000
	(0.160)	(0.022)	(0.003)	(0.001)
Treated x Post x CEO	-0.550**	-0.102***	-0.006	0.000
	(0.230)	(0.026)	(0.004)	(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 (11) The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction of 14 percentage points, 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). 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.

E External Validity

In Appendix E, 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.

E.1 Comparing Treated-listed firms to Private firms in Korea

Table E.1 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.

E.2 Comparing firms to C-corporations in the U.S.

Table E.2 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 E.1: 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 millon)	74.69	95.13	67.10	208.1	252.4	179.1
	(45.16)	(44.34)	(43.06)	(172.7)	(187.5)	(155.6)
Labor (Average Employees used)	243.1	254.3	238.9	356.3	431.5	307.0
	(174.5)	(135.5)	(186.9)	(278.0)	(290.9)	(257.6)
Total Asset (in million)	75.02	123.1	57.16	197.2	274.4	146.7
	(64.65)	(68.29)	(53.15)	(181.5)	(197.7)	(149.9)
Total Capital (in million)	40.50	74.52	27.86	102.7	150.4	71.48
	(41.94)	(46.68)	(31.83)	(103.3)	(110.2)	(85.07)
CAPEX (in million)	4.067	5.763	3.436	9.456	12.06	7.750
	(6.086)	(7.647)	(5.257)	(12.76)	(14.03)	(11.54)
CAPEX / lagged PPE	0.219	0.186	0.231	0.206	0.181	0.222
	(0.264)	(0.224)	(0.277)	(0.242)	(0.212)	(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 E.2: Descriptive Statistics for C-corporations (US) and Listed & Private firms (Korea)

Variables	C-corporations (Mean)	Listed and 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.

F Other Outcomes

In Appendix F, I provide a set of estimates of the tax effects on other outcomes: (1) average employees, (2) total revenues, (3) Debts, (4) Equity to Debt Ratio, (5) return on assets, (6) probability of firms going public through initial public offerings, and (7) expenditures on Research and Development.

Table F.1 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 the coefficient is 0.092, implying that affected firms increased employees by 9.2 percent 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 F.1 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 19.7 percent 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 percentage points lower for publicly trade firms. Overall, increases in total revenues are consistent with the fact 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 F.2 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 increased equity to debt ratio, 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 (3) 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 (4) 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 (5) 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 after the reform.

Table F.1: 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***	0.197***	0.100*	0.154***	0.083***	0.197***
	(0.027)	(0.031)	(0.053)	(0.054)	(0.032)	(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 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 F.2: Results on Other Outcomes (Publicly Listed Firms)

	Capital Structure		Profits	Initial Public offerings	Other Investment
	(1)	(2)	(3)	(4)	(5)
	Debts	Equity to Debt	Returns on Assets	Pr(Going Public)	RnD Expenditures
Treated x Post	0.033	0.044***	0.051	0.159***	0.006**
	(0.070)	(0.011)	(0.045)	(0.038)	(0.003)
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	17.127	0.035	0.285	0.614	0.017
Implied Elasticity wrt (1-tau)	0.18	6.78	0.98	1.40	1.78
R-squared	0.90	0.30	0.61	0.73	0.69
Observations (firm-years)	4736	4503	4736	4736	4735
Clusters (firms)	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 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 debts. Equity to debt ratio is the market value of total equity scaled by the book value of total debts. 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.